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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NATURAL SCIENCE:

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INTRODUCTION.

IN a comparison of the position of Science to-day with that which it held fifty years ago, one is apt to be so dazzled by the brilliance of the progress as to overlook the fact that the advance has not been unaccompanied by certain tendencies which are now a stumbling block, and threaten even greater trouble in the future. Half a century ago, scientific research was almost entirely in the hands of amateurs—independent workers, as Humboldt, Darwin, Lyell, Murchison, Hugh Miller, Waterton, and others — men possessed of wide experience of affairs, who carried into scientific investigation the broad grasp of general principles, and the sound judgment gained by contact with the intricate problems of the world of work and business. Since then, there has been a great change, mainly wrought by the very rapidity of the progress. The more rigorous accuracy demanded in scientific work, the necessity of a knowledge of at least three or four modern languages, and the enormous increase in periodical literature, have combined to discourage amateurs and to encourage the growth of specialisation, with a consequent development of professionalism. This tendency was, at first, strongly resisted, and *The Natural History Review*, with other serials such as *The Intellectual Observer*, *The Popular Science Review*, and *The Quarterly Journal of Science*, strove to proclaim the latest results of scientific enquiry. The last of this group of publications, however, has been discontinued, and for some time no adequate systematic attempt has since been made in Britain to interpret simply, and without excessive technicalities, the main results of contemporary work in Natural Science to those who try to follow the general progress of modern thought.

Both naturalists and the public have suffered in consequence. The former have lost the stimulus of popular sympathy and criticism, and the contraction of the recruiting ground has diminished the number of the unattached local workers upon whose efforts progress in certain departments ultimately depends. Moreover, an ever-increasing proportion of the new recruits have entered upon their work with minds cast in the mould of one of the orthodox universities or schools, and carry it on rather as a means of livelihood than of any special enthusiasm for the cause. Hence Science has had to suffer, on the one hand, from a certain degree of lukewarmness, and, on the other, from a conservatism that has had its origin in impatience at unsympathetic and ignorant control.

The result, while it has been detrimental to Science, has been no less injurious to the cause of popular education. Tennyson, in his "Despair," represents, with powerful pathos, the condition of a man whose peace of mind had been shattered by the teachings of a science which he woefully misunderstood: it had destroyed the foundations of his old beliefs without substituting anything in their stead: it had broken the old moorings, and left the man to drift aimlessly instead of endowing him with the strength and stability with which to use the intellectual liberty he had gained. The history of our literature shows that this case has been by no means a rare one.

It is the hope of extending the knowledge of the general principles of Natural Science, of which the influence on contemporary education should be not only to stimulate, but also to moderate and strengthen, that leads to the issue of the present Review. It will be our constant aim to expound and deal in a critical manner with the principal results of current research in Geology and Biology that appear to be of more than limited application. Original articles referring to the existing status of certain special branches of Natural Science, with suggestions for further development, will be a prominent feature. Periodical summaries of the latest results in the various departments are contemplated. Reviews of the more important new books will be not merely critical, but also descriptive. Special attention will be given to the latest news concerning the work of all the principal societies and institutions throughout the world devoted to scientific and educational matters; and the correspondence columns will be open both for the discussion of the latest problems and for the purpose of replying to enquiries as to the literature of any special subject in Natural Science on which information is desired.

Such, briefly, is our ideal. We invite the co-operation of all contributors to the progress of Natural Science in striving towards its realisation.

NOTES AND COMMENTS.

The Teaching University for London.

THE agitation for a Teaching University for London seems at last likely to produce some practical result. Unless an address of protest be presented to Parliament before the 18th inst., the Draft Charter of the New Albert University will be granted by the Crown. This may seem a nominal success, but we greatly doubt whether most of those who have worked at this question would have troubled themselves about the matter had they seen how insignificant would be the result. Briefly stated, the ideal for which the younger London educationalists have been working is somewhat as follows. In London we have a University which is cosmopolitan in its scope; which is a great examining board for the whole Empire, but which, in consequence, has not been able to assist much in the actual work of local teaching. On the other hand, we have in London enormous educational endowments, and a considerable number of independent institutions which for years have been doing splendid service in the cause of metropolitan education: but owing to the restrictions dictated by the special interests of the founders of these institutions, they have inherited with their endowments a legacy of loneliness that has prevented any extensive co-operation. Hence has arisen the scheme for federation of all the leading educational bodies into one great University. In this would be included not only the Royal College of Science, the City and Guilds Institute, University, King's, Bedford, and Queen's Colleges, and the great professional schools, medical, legal, and theological, but also those less formally organised institutions in which the bulk of the London middle class continues its education, such as the City of London College, the Birkbeck, the Working Men's College, and Toynbee Hall. The University Extension Society, with its more popular lecture courses, would also be represented, while the London School Board would be associated by a system of scholarships that would enable the best of its 440,000 students to train for a career of literary or scientific usefulness.

Five years ago the scheme seemed to be a beautiful, though impossible dream; but as the Royal Commission of 1889 reported in favour of the establishment of such a University, it has been brought within the sphere of practical politics. This Commission, however, reported in favour of the reform of the existing London University, but the Senate and the Convocation have failed to agree on any definite scheme, though both are ready to move in the desired direction.

The result of the disagreement between the Convocation on the one hand, and the Senate and the Special Committee of Convocation on the other, seems to have inspired the authorities of King's and University Colleges to another effort for that joint University, the claim for which had been expressly rejected by the Royal Commission in 1889. On the present occasion, their scheme, hastily, and almost secretly discussed, has been more successful, and the draft now before Parliament is the result.

How far does this scheme answer to the ideal towards which the London educationalists have been working? Whereas the great feature of the desired University was to have been its breadth, the scheme proposed is characterised mainly by its narrowness. Out of all the institutions mentioned by the Royal Commission of 1889, only University and King's Colleges and the Medical Schools are included; and the first two of these are to have the main power, though they represent only an average aggregate of under 250 degree- or associate-seeking students. Whereas the desired University was to have been democratic and elastic, capable of constant adaptation to the ever-varying needs of London life, the proposed University is planned on the old lines, insists on large collegiate buildings for every constituent college, and requires practically whole time attendance. The scheme has, in consequence, aroused much hostile criticism, but, so far, the London scientists have held aloof, regarding the whole thing simply as a plan to enable the students at the Medical Schools to obtain easy degrees without going to Scotland. Moreover, the fact that the most noisy opposition has been based on an appeal to sectarian prejudices has not helped to rouse scientific interest in the matter. This apathy seems to us quite inexplicable. The progress of popular education is of such fundamental importance, that indifference is inexcusable. If the Draft Charter be granted, we shall have in London a University started on antiquated lines, and on a basis ridiculously inadequate to London needs: two Colleges, both with small muster rolls of students, and a few Medical Schools will be dignified by the title of the "Teaching University of London," from which, by its constitution, the vast majority of real London students will be necessarily excluded. The opportunity will have been lost for a union of all the institutions helping forward the higher education of London, that would have raised the standard of teaching, necessitated a more systematic course of study, and led to the removal of the few indifferent or unqualified teachers who at present do so much to discredit the great evening educational institutions of the metropolis. Hence, if, as we hope, the Charter now before Parliament be not granted, and the establishment of the Teaching University be again delayed, this will not have been in vain, should it result in the ultimate establishment of one based on wider lines, and more adequate to the great mission it was originally proposed to fulfil.

Newspaper Science.

Some of our British daily newspapers occasionally recognise the fact that scientific news may be included among the topics of current interest. This is satisfactory. But general articles by journalists on matters relating to science are capable of doing either good or harm; and we fear the leading article on "Young Collectors," published in the *Daily News* of February 6, falls within the latter category. It is calculated to deter parents from encouraging scientific pursuits, as the following extracts will show:—

"Boys will go collecting things—it is difficult to say why. Indeed, it is not easy to explain why anybody collects anything. Probably the pleasure consists—first, in having what other collectors have not; and, secondly, in the passion for perfection and the enjoyment of the chase. . . . . The things which people will collect, judging from the list of Manuals for the Young, are numerous, nor do they all excite a well-regulated cupidity. Butterflies, beetles, and moths we could do without, but London is a great place for moths, and the young collector, with the aid of sugar, may even in London pick up some moths, and a little knowledge of natural history. For fungi it is quite vain to pretend any enthusiasm. . . . Fossils are heavy, but harmless, and there are some who consider marine shells decorative objects; among these amateurs conspicuous are the keepers of lodging houses. . . . . There is probably no great harm in amassing a collection of diatoms, if they do not smell disagreeably, but boys were more in the habit of collecting dormice in the years that were. We suspect diatoms, as they appear to live in ponds with algæ and the like. . . . People should be very careful how they put these things into the heads of boys."

The general tone of this article is unfortunate, to say the least, for science owes much to collectors. Many distinguished naturalists laid the foundations of their knowledge upon the collections they made in the days of their youth.

Central Asian Exploration.

At a meeting of the Royal Geographical Society held on February 8th, Captain F. E. Younghusband gave an interesting account of his two adventurous journeys to the Pamirs—the so-called roof of the world—lying to the northward of Gilgit, and westward of Turkestan. The first of these journeys was commenced in July, 1889, when the traveller, with an escort of five Goorkhas, left the British station of Abbotabad, in Hazara, to make his way to Leh, the capital of Ladak. From Leh the party proceeded northwards by the now well-known route across the Karakoram to Shahidula, in Turkestan, from which point their exploration may be said to have commenced. From Shahidula the route lay in a north-westerly direction, their object being first to reach the Pamirs, and thence to turn southwards and reach Hunza, in Yaghistan, to the north-east of Gilgit, by crossing the great Mustagh range. A full description of the route to the Tagh-dum-bash Pamir, by way of the Khal Khuskun Valley, and especially of the enormous and deeply crevassed glaciers,

from which the Oprang river takes its rise, was given by the lecturer. Some of these glaciers descending from the northern flanks of the Mustagh — the westerly continuation of the Karakoram — were, indeed, so cut up by crevasses that progress was found impossible, and the party met with some hair-breadth escapes from the avalanches which seem to be perpetually thundering down these valleys. The Tagh-dum-bash Pamir was eventually reached in October by an easy pass of 14,600 ft., and the transition from the deep gorges and lofty cliffs of the Yarkand river and its tributaries to the broad and open valleys of the Pamir, appears to have been very striking. Here a cold of 5° below zero was experienced. From the Pamir, Hunza territory was reached by crossing the Mintaka pass, due north of Hunza itself; the scenery down the Hunza valley to Gilgit being described as grand in the extreme, peaks of 20,000 ft. being comparatively common, while a few, like Raki-Poshi, exceed 25,000 ft. At Gilgit the party were received by Colonel Durand, the British agent. The concluding portion of the paper described a second journey round the Pamirs, commenced in the summer of 1890, when Captain Younghusband, in company with Mr. Macartney, travelled by way of Kashmir and Leh to Yarkand, thence journeying westward to the Pamirs, and reaching Gilgit in October, 1891. Although Captain Younghusband has given us much information as to the geography and ethnology of the Pamirs, we cannot help a feeling of regret that, not being either a zoologist or geologist, he is unable to give us an account of their fauna and geology.

Thus much of exploration to the north of the Karakoram and Mustagh. We learn, however, from a contemporary, that a small expedition, under Mr. W. M. Conway, has just left this country for the purpose of exploring the glaciers flowing from the Mustagh southwards into the valleys of Baltistan, or little Tibet. The great Palma (Punmah), Biafo, and Baltoro glaciers, which unite their streams in the upper affluent of the Shigar river, are believed to be the largest in the world outside the Arctic and Antarctic regions, and, although mapped by Colonel Godwin Austen, have never yet been fully explored. One of the main objects of the expedition is to attempt the ascent of Peak "K.2" of the Indian survey maps—now known as Mt. Godwin Austen—which has a height of 28,265 ft., and is believed to be the second highest mountain in the world. This magnificent peak stands on the crest of the Mustagh, and gives rise to one of the chief affluents of the Baltoro glacier, and the party are determined to see how far it is possible to ascend this giant. In their ascent of the Biafo glacier—the largest of the three—we venture to hope that the scientific members of the expedition will endeavour to give us some exact data as to the direction and extent of the band of Triassic limestone rocks which is known to lie in a trough of the gneissic rocks, and to extend towards the crest of the Mustagh in the direction of the glacier in question.

There is more news concerning the dentition of the Duck-bill (*Ornithorhynchus anatinus*), the discovery of which by Messrs. E. B. Poulton and O. Thomas excited so much interest three or four years ago. Professor Charles Stewart has met with a very young specimen in the stores of the Royal College of Surgeons, London, showing the three teeth in position in each jaw. The teeth are firmly implanted in the gum by two irregular, ridge-shaped roots, which had become absorbed in the more nearly adult specimens in the British Museum described by Mr. Thomas. Beautiful figures by Hollick accompany Professor Stewart's short paper in the first part of the new volume of the *Quart. Journ. Microscopical Science* (vol. xxxiii.).

A writer in the current number of the *Cornhill* devotes some pages of a very readable article to arguing for the high position of the Parrot tribe among birds. The power of imitation, he thinks, puts them at the very summit of the feathered race. If Mr. Grant Allen (who is, we presume, the author of this article) has ever heard the Indian Myna at the Zoological Gardens, he should have considered its claims to represent the aristocracy of birds. For clearness of enunciation the Parrot is far inferior to the Myna; and the Starling and Crow tribes generally share its capacity for imitating sounds. Professor Newton, in common with many other Ornithologists, puts these birds in the foremost place; and so far as the faculty of mimicking the human voice is a test of ornithic excellence they are justly so placed.

Surgeon-Major A. S. G. Jayakar, stationed at Muscat, at the mouth of the Persian Gulf, has for several years been making large collections of fishes. He has presented these to the British Museum, where they have been worked out by Mr. Boulenger. Through the exertions of Dr. Jayakar the fish-fauna of that part of the Indian Ocean where he has been stationed is now better known than that of many points on the coasts of India. Nearly 300 species are already on record from Muscat, among which are many described as new, or belonging to forms not before recorded from the Indian Ocean. At the last meeting of the Zoological Society a third report was read by Mr. Boulenger on the collections of fishes made by Dr. Jayakar.

Though Malta has for so long a period belonged to the British Crown, its geology is still imperfectly known. A good deal of confusion has been caused by want of exactness in the determination of the fossils; still more by mistakes as to the horizons and localities from which they were obtained. At last, however, the careful observations and the fine collection of Maltese fossils made by Mr. J. H. Cooke, are providing material for a better understanding of the Geology of the Island; and in a memoir just issued by the Royal Society of Edinburgh ("Transactions," vol. xxxvi., pt. iii.), Mr. J. W.

Gregory has availed himself of the opportunity to revise the Echinoidea, while making an analysis of the evidence they provide as to the age and origin of the Tertiary strata of Malta. The Echinoidea always form a most valuable group, when evidence is required as to the correlation of strata, or as to the conditions under which certain sediments were laid down. Mr. Gregory comes to the conclusion that the strata in question belong partly to the Oligocene, partly to the Miocene period, and that they probably range from the Tongrian (Lower Oligocene) to the Tortonian (Upper Miocene). The evidence brought forward of changes of depth, as shown by the Echinoderms, is curious. The Tertiary series in Malta begins and ends with shoal-water deposits, but the intervening strata indicate deep water. Other parts of the Mediterranean basin yield indications of a totally different succession of events. It appears as if the earth-movements on the shores of the Mediterranean have always been extremely irregular.

The origin of the "Parallel Roads" of Glen Roy is a question of perennial interest to geologists, and Mr. T. F. Jamieson returns to the subject in the February part of the *Quarterly Journal of the Geological Society*. He refers the formation of the roads (or more correctly beaches) to the action of lakes at different levels. These lakes owed their existence to the ponding-back of the water in the Glen by an ice-dam during the retreat of the last ice-sheet. The accurate maps of the Glen made by the Ordnance Survey, and the publications of the Danish Commission for the Exploration of Greenland, show that Glen Roy was once, in all probability, singularly like certain parts of Greenland, where ice-dammed lakes are still to be found in several of the Fjords.

Pleistocene botany is now attracting more attention. In *Nature* (Jan. 21) Dr. A. G. Nathorst has brought forward "fresh evidence concerning the distribution of arctic plants during the Glacial Epoch," and gives a good map of Northern Europe showing the localities from which the plants were obtained. In the *Naturwissenschaftliche Wochenschrift* (Jan. 24) Professor A. Nehring records a number of plants from the "diluvial" deposits of the province of Brandenburg; these include the Holly and Hornbeam, and are not arctic.

The *Cape Argus*, of November 18, 1891 (as we learn from the *Kew Bulletin* for January), announces that with the commencement of the present year the Cape Town Botanic Garden will cease to exist as a botanical establishment, and will be taken over by the municipal authorities to be maintained as "a town *pleasaunce* of flowers and shady walks." The director, Professor MacOwan, will be transferred to the Agricultural Department as Government Botanist. He will

also act as Keeper of the Government Herbarium, which is handed over to the same institution. According to the *Argus*, this change is necessitated by the insufficiency of the Government grant—a subsidy of £500 per annum having to meet an expenditure of about £1,400. The balance has been made up by the business of seed-selling, "and the whole energies of the staff were incessantly directed to the one object of making money enough to pay the wages and repair dilapidations." For the last ten years Professor MacOwan has maintained the institution in this manner without incurring debt. As there is apparently no hope of improvement, the Professor has at last given up the business, assuming, probably, that his scientific knowledge and ability will be more useful as Government Botanist in the Agricultural Department. The gardens will be placed under the charge of a competent horticulturist, who will doubtless be all that municipal authorities desire. Under the circumstances detailed in the *Argus*, the change may have been inevitable, but it is none the less distressing. The value of experimental gardens and stations cannot be over-rated. We have only lately had a splendid instance in the work of Messrs. Harrison and Bovill at Barbados, resulting in the production of sugar canes from seeds, for the first time in the New World. This may be of the greatest importance to the sugar-cane industry and to our sugar-producing Colonies. Hence it is sad news to hear that a botanical garden cannot be supported in a country with a Flora so rich and characteristic as that of the Cape.

Good portraits, with brief biographies, of two British naturalists appeared last month. Mr. H. T. Stainton, F.R.S., forms the subject of the monthly biographical sketch in the February *British Naturalist;* and Mr. Robert Etheridge, F.R.S., was included in the *Mining Journal's* series of "Familiar Faces" on February 6th. We are glad to learn that, notwithstanding his retirement from the official staff of the British Museum, Mr. Etheridge continues to devote his energies to the arrangement of the Stratigraphical Collection of British Sedimentary Rocks.

The *Scottish Naturalist* has this year been transformed into a beautifully printed quarterly publication, the *Annals of Scottish Natural History*, with occasional lithographed plates. It is under the editorial direction of Professor J. W. H. Traill and Messrs. J. A. Harvie Brown and W. Eagle Clarke. A similar journal, to be published monthly in Dublin, is promised next month under the style of the *Irish Naturalist*. This will be the official organ of the Royal Zoological Society of Ireland, of the Belfast Natural History and Philosophical Society and Naturalists' Field Club, of the Dublin Naturalists' Field Club, and of the Armagh Natural History and Philosophical Society. It will be edited by Messrs. G. H. Carpenter and R. Lloyd Praeger.

I.

Some Recent Observations upon Mimicry.

SUPERFICIAL resemblances between animals, often remote from each other in the system of nature, have long been familiar to naturalists. Messrs. Kirby & Spence refer in their "Introduction to Entomology" to the case of the fly *Volucella*, which, as these authors justly remark, strikingly resembles the bee in whose nest its young are parasitic. Long before Kirby & Spence, Belon had classified whales with fishes, and similar likenesses, which imply no real affinity, even now mislead systematic zoologists every day. An excellent series of cases has been lately added to the Natural History Museum at South Kensington, illustrating a few of the more remarkable instances of particular resemblances among insects not nearly related to each other. It is to these close and often wonderfully detailed likenesses that the term "Mimicry" is usually restricted.

Before giving some account of recent facts and suggestions concerning this subject, it may perhaps be useful to remind the reader of the current theory of mimicry. Many creatures which belong to groups commonly preyed upon by larger and stronger animals, are possessed of some weapon of defence or some disagreeable attribute, which is duly advertised by a conspicuous appearance. The hornet, for example, though a giant among its own tribe, is a puny creature when compared with even a small bird. But the hornet is amply recompensed for its disadvantages in point of size and strength by possessing a formidable sting. This gives the insect a usefully evil reputation, and enables it to defy most of its natural enemies.

In this country there is a rather rare moth, which is called the "Hornet Clearwing," on account of the singular likeness which it bears to a hornet. Moths, like butterflies, have four wings densely clothed with scales; exceptionally—as with the "Clearwings" (named so for this very reason)—the wings are largely denuded of these scales; thus there is evidently at once a certain likeness to a bee or a hornet, or to any insect without scaly wings, and a corresponding difference from the scale-winged Lepidoptera. But the resemblance goes further than this; the moth has a body banded with brown and yellow, precisely like the hornet, and is of about the same size. Moreover, it has been said that this moth, when handled, moves its abdomen "in a very suggestive fashion," as if hinting at a sting

in the background; finally it appeals to another sense, for it has the characteristic odour of hornets. Presumably, therefore, this perfectly harmless insect deludes its foes into the belief that it is a veritable hornet, sting and all complete. In this particular case it is necessary to say "presumably," for, since the moth is so rare, it has naturally escaped much experiment. Mr. Poulton offered one to a lizard; the reptile looked with marked suspicion at the moth, but after licking it with its tongue, seized it by the thorax, which was at once crushed so as to kill the insect. This experiment is not, therefore, very conclusive. From the moth's point of view, it is not more satisfactory to be cautiously destroyed than to be bolted hastily. But there are other instances where an insect, owing to its similarity to some other insect, which is protected by a sting or a bitter flavour from much persecution, shares the same immunity. It is, therefore, intelligible how, as Mr. Bates first suggested, these facts of mimicry can be explained by Natural Selection. Given the utility of the resemblance, it is quite conceivable that minute variations in the required direction were perpetuated and increased.

The classical instance of mimicry is, of course, afforded by the butterflies belonging to the families Pieridæ and Heliconidæ, which were studied in South America by Mr. Bates. The Heliconidæ possess a rank flavour and odour, not only disgusting to the human nose, but to monkeys, birds, and other creatures, which, as a rule, are partial to butterflies. Two or three genera of the Pieridæ have assumed the livery of these unpopular Heliconidæ, and are believed to trade upon their reputation. It is supposed that the birds and other natural enemies of insect life have gradually, through a series of unpleasant surprises, learnt to distrust the gaudy colours of the Heliconidæ, and to assume that any butterfly coloured after the same plan would prove to be also inedible. In all these cases there is, as Mr. Poulton has admitted, "too little direct experimental proof of the unpalatability of the specially protected groups which are the chief models of mimicry." The burden of proving that the imitators as well as the imitated enjoy a considerable immunity evidently lies with the advocates of the theory. When Mr. Poulton wrote that "a peculiar and frequently unpleasant smell has been noticed by all observers who have studied" the three principal families of butterflies which are models for mimicry, viz., the Acraeidæ, Danaidæ, and Heliconidæ, he was without doubt expressing the general opinion upon the subject. Dr. Seitz, however, who has paid particular attention to this question, was unable to recognise the least odour—disagreeable or otherwise—in 50 distinct species of Danaids, both African and American. In some, but not in all the Heliconidæ, Dr. Seitz found a decidedly objectionable odour; and curiously enough in *Heliconius beski*, a species with a particularly evil odour, it was discovered that only a small percentage of individuals were odoriferous. This last fact is, of course, not contrary to the theory of mimicry, because it may be

assumed that the majority trade upon the unpleasant reputation of the few; just as the sweet-tasting Pierids are thought to lead comparatively peaceful lives by imitating the nauseous Heliconidæ.

It is, to our mind, a highly significant fact that cases of mimicry are, to a certain extent, proportionate to the number of species in the group where they occur. Among insects the vast majority of examples are met with; there are a few only reputed cases of mimicry in the two groups, Birds and Reptiles, and it is difficult to point to one example of advantageous mimicry in the Mammalia, which is the smallest group of the four. There is no *à priori* reason why mimicry should be less advantageous to a mammal than to an insect. On the contrary, indeed; for an insect as a rule leads a very short life, and when it has succeeded in laying its eggs, its claims upon life are exhausted. Natural Selection, as it has been said, cares nothing for the individual; she only concerns herself with the race. Merely from the point of view of the continuance of the race, it seems hardly necessary for a butterfly to have any assistance in prolonging its life; pairing takes place so soon. In fact, in some Lepidoptera the impatient males wait round the cocoon for the emergence of their mate. On the other hand, a mammal which has to live for a long time before being able to reproduce its kind, and is frequently preyed upon by many other creatures, both mammals and birds, seems much more entitled to the protection afforded by such a device as mimicry; so, too, with birds and reptiles.

Considering the immense multitude of insects, and the daily increase in our knowledge of hitherto unknown species (no less than 1,500 new species of Lepidoptera are recorded in the Zoological Record for 1889 as having been described during that year), and considering also the comparatively few plans of coloration that are met with, it would not be at all surprising to meet with accidental resemblances. The larger the amount of material, the larger would the number of accidental resemblances be; and this is precisely what we find.

Of more importance than purely fortuitous likenesses, supposing that such exist, are the resemblances due to similar conditions. Andrew Murray laid weight upon these little understood causes in attempting an explanation of the phenomena of mimicry. Dr. Seitz has lately discovered some curious instances, which seem to be only explicable on such a theory. In a forest of Southern Brazil, this naturalist found a perfectly circumscribed region, inhabited by insects almost entirely blue in colour; a few miles away from this spot the insects were red, yellow—any colour but blue; but in the particular area blue was so characteristic a tint that out of twenty butterflies ten were entirely blue and the remaining ten partially blue. Another equally remarkable instance of an apparent connection between colour and locality has been longer known. Ransonnet, in his work upon Ceylon, gives two beautiful plates representing life on a coral reef.

These plates were drawn by the author from a diving-bell, and illustrate the prevailing green hue of the inhabitants of the sea; animals, which in other localities are very rarely green, are green here; particularly is this so with the corals. Professor Haeckel admits this to be a remarkable fact. The prevalence of green among the inhabitants of the adjoining land is not so noteworthy a fact, since it might be reasonably put down to a protective imitation of the surrounding foliage; and green is not an uncommon colour among forest-dwelling animals.

One of our handsomest English butterflies, the "Clouded Yellow" (*Colias edusa*), has a well marked variety known as *Helice*, and this variety chiefly differs in its paler colour. In the Argentine Republic there occurs a species much like *Edusa*, which has also a varietal form differing from it about as much as *Helice* does from *Edusa*. In certain districts of the same region of America, Dr. Seitz met with a common butterfly which he at first mistook for the European *Vanessa levana*, so closely does it resemble that insect in colour and marking. Furthermore, it has a varietal form which is, in the same way, exceedingly like a variety of the European insect. A closer examination of the American butterfly showed that it belonged to a quite different genus. "If," remarks Dr. Seitz, "these insects were found in our country, no one would doubt that this is a case of mimicry as perfect as any which exists."

Sir Walter Buller, in his work upon New Zealand birds, has commented upon the extraordinary likeness which a cuckoo of the genus *Eudynamis* bears to a hawk. This fact is not new, for other cuckoos are like hawks; our own cuckoo is so much so that there is even a legend to the effect that in the winter it becomes a hawk. The interesting point about this particular cuckoo is that it "mimics," not a New Zealand, but a North American, hawk. This last instance may be fairly regarded as purely accidental; but with regard to the butterflies, we cannot but suppose that similarity in the conditions of life is responsible for their mutual resemblances. This phrase is doubtless vague and unsatisfactory, for at present we have not sufficient knowledge to formulate a more accurate statement.

It has been already pointed out that evidence is in many cases wanting of the advantages which an insect gets by mimicking another; the "Bee Hawk Moth" is an example of what might, in an oft-quoted phrase, be called "a palpably intentional likeness that is perfectly staggering"; even a naturalist of experience might be excused for putting it down as belonging to the Bee tribe. And yet Mr. Poulton found that it was eaten "without hesitation or caution" by a lizard. Dr. Seitz[1] noticed in South America a kind of "Humming-bird Hawk Moth" which closely mimicked a particular species of humming-bird found in the same locality. The resem-

[1] *Stettiner Entom. Zeitschr.*, 1890.

blance, although striking enough to our eyes, did not deceive the insect, for it was never observed to make any attempts to pair with the bird; and it is notorious that butterflies are often deluded into believing an insect of a different species to be a suitable mate. If an insect with its limited and imperfect vision is proof against a deception which is enough to strike an observant naturalist, would birds be so?

A very singular case of apparent mimicry has not yet been quoted in popular works dealing with this subject. Dr. Bovallius[2] has lately described a species of Amphipod Crustacean which he named *Mimonectes* on account of its likeness to a *Medusa*. The body of this animal is produced into a dome-like structure transparent as glass, from the underside of which depend the minute legs, crowded together so as to suggest the tentacles of a *Medusa*. Here we have a resemblance which is the more striking, inasmuch as it exists between animals widely removed from each other in zoological position. And the poisonous character of the *Medusa*, armed as it is with innumerable "lasso cells," would seem to make it an excellent model for the defenceless Crustacean to copy. Furthermore, it is a pelagic animal living in the surface waters just as *Medusæ* do. Nevertheless, it is exceedingly doubtful how far the *Mimonectes* profits by its departure from the usual plan of structure of the group to which it belongs. A school of whales or a shoal of pelagic fish, rushing through the water open-mouthed and devouring all before them, could hardly be supposed to stop and analyse carefully the advantages or disadvantages of selecting or rejecting a given animal as food. And these must be the chief enemies with which small pelagic creatures have to reckon.

Another case of mimicry, which would seem to confer no particular advantage on the disguised insect, is noted by Dr. Seitz.[3] He calls attention to the close similarity between two moths—*Brephos nothum* and *Ploseria diversata*—belonging to different families, which emerge from the chrysalis at about the same time. So detailed is the resemblance, that, according to Dr. Seitz, "it would be carrying scepticism too far to regard the agreement as merely the result of accident." This is plausible but not entirely convincing, if we take other facts into account. Mr. Poulton, in arguing for the efficacy of "warning colours" (and of course this will include mimicry), points out "the entire disappearance of all insects with warning colours during the seasons when insect life is scarce," and therefore in great requisition by birds. During the earlier and later months of the year, anything in the shape of an insect will be eagerly snapped up, and warning colours would become a danger signal, not to the bird, but to the insect. Now March is the month in which these two moths appear; and they are generally the only insects on the wing. They would therefore be an obvious mark for any birds that were about,

[1] *Nova Acta, R. Soc., Upsala*, 1885. [3] *Zoologische Jahrbücher*, 1889

which would probably be inclined, in the absence of their favourite insects, to drop the prejudices which they are supposed to entertain against brightly coloured moths.

The mimicry shown by the Sesiidæ (Clearwings) and by *Leptalis* and some other genera of the Pieridæ is less striking, inasmuch as all the members of the family or genus, as the case may be, have moved in the same direction. The very fact of the absence of scales from the wings of the Sesiidæ constitutes in itself a considerable step in the direction of resemblance to a bee or fly. It can hardly be assumed that each species of *Sesia* independently copied some Dipteron or Hymenopteron, first of all losing its scales and then becoming modified in colour and size, habits, &c. The development of this family of moths shows that the scales are partially formed during the pupal stage, but that they are arrested before they reach their full size, and then disappear. The study of the development of animals always shows, with more or less clearness, an epitome of the history of the race. Every animal, as it has been put, "is compelled to climb up its own genealogical tree." Thus these moths are evidently derived from scaled moths. A tendency to lose the scales characterises other families of Lepidoptera besides these Sesiidæ; and it is at least a reasonable supposition that this tendency may have been accentuated by the need for protection.

The principal difficulty in taking this view is the singular unanimity which the very numerous species of this group have shown in their choice of a disguise. As these insects are found in most parts of the world, and have, therefore, to cope with all kinds of hindrances to their prosperity, it might have been thought that they would display a corresponding variety in the methods by which these disadvantages are overcome. One is tempted, therefore, to believe that this family of moths were moulded into their present form by causes of which we know nothing, and that to the very considerable initial likeness to the Hymenoptera generally, such special resemblances as they show to particular bees and wasps, were added later through the action of Natural Selection.

Much more striking as evidence of the truth of Mr. Bates' theory are such cases as *Basilarchia hipparchus*. This genus of butterflies, with the exception of the species mentioned, are dark coloured with blue spots round the margins of the wings; *B. hipparchus* is tawny-brown and black, closely simulating in tints and plan of colouration the Danaid *Anosia plexippus*. If this Danaid is really avoided by birds, we can understand why the *Basilarchia* has, so to speak, diverged from the road it ought to have followed, and imitated the *Anosia*. On the theory of mimicry, we interpret this divergence as due to Natural Selection gradually acting on a small initial difference; and if an insect is found whose colouration is strikingly unlike that of its immediate allies, it is apt to be assumed that another insect will be found which it imitates. But this assumption

is by no means always correct; and the existence of such forms is a difficulty in the way of accepting the theory of mimicry. If a butterfly has abandoned the usual dress of its genus without any object in doing so, it might fairly be asked whether, when a resemblance to another form does occur, it is not accidental. If a divergence from the normal style of colour and marking does not always go with the existence of a prototype belonging to a different genus or family, a certain amount of doubt is naturally thrown upon the validity of the explanation in other cases where there is a prototype. Mr. Scudder has suggested this difficulty, and has grappled with it in his magnificent work upon the butterflies of North America, recently published.[1] He quotes two suggestions that have been made by way of explaining the matter. The first is the obvious one that the model which is imitated *does* exist, but has not been found; the second suggestion is that the prototype *did* exist, *i.e.*, that it has become extinct. The mimicking form may then be supposed to have secured and established its position by the help of its vanished model, just as a dishonourable firm may trade for a time under an old-established name no longer represented in the firm. Both these suggestions are easier to make than either to prove or refute. The third explanation is the most satisfactory. Mr. Scudder believes that in cases of this kind we have to do with a general mimicry in course of conversion into a special mimicry—that is to say, the mimicking insect has got so far as to bear a certain likeness to the members of another group, but has not yet acquired a particular likeness to any one species. The general resemblance might in the meantime profit the insect. The Marbled White butterfly may be mentioned as a possible example; it has departed from the usual brown colour of its immediate relatives, and has become something like the "Whites," but is not yet like any one species. A comparatively small change would convert it into a good likeness of any species of "White."

FRANK E. BEDDARD.

[1] "Butterflies of the Eastern United States.

II.

Deep-Sea Deposits: A Review of the Work of the "Challenger" Expedition.[1]

THE long-expected report by Messrs. Murray and Renard on the deep-sea deposits collected during the cruise of the "Challenger," has made its appearance, and will be cordially welcomed by all geologists. Many of the facts and most of the conclusions have been published in the proceedings of various societies, and have even found their way into text-books. Nevertheless, it is to this volume that all those who are desirous of studying the geological operations at present going on in our great ocean basins will necessarily turn.

The work forms a bulky quarto volume of five hundred pages. It is illustrated by forty-three charts indicating the positions of the sounding and dredging stations, a coloured map of the world representing the distribution of the principal deposits, twenty-two diagrams showing the vertical distribution of temperature and the relations between the deposits and depth, thirty-six woodcuts, and twenty-nine lithographic and chromolithographic plates, many of which are beautifully executed and give an excellent idea of the objects they represent.

The deposits were examined in the following way. The proportion of carbonate of lime was approximately ascertained in each case by the direct determination of carbonic acid. The relative amount of the remains of the different lime-secreting organisms was roughly estimated by microscopic examination. The residue in the carbonic acid apparatus was then separated into three parts—siliceous organisms, minerals, and fine washings—by fractional decantation, and the relative proportions of these three constituents also estimated. The percentage composition of each deposit is given in terms of (1) pelagic foraminifera, (2) bottom-living foraminifera, (3) other calcareous organisms, (4) siliceous organisms, (5) minerals, and (6) fine washings. Under the last head are included argillaceous material, organic matter, minute fragments of siliceous organisms,

[1] Report on Deep-Sea Deposits Based on the Specimens Collected during the Voyage of H.M.S. "Challenger." By John Murray, LL.D., Ph.D., and Rev. A. F. Renard, LL.D., Ph.D. London: Eyre and Spottiswoode, 1891.

and mineral particles too small for precise determination. These fine washings do not make up a pure clay, but a substance allied to bole-clay rich in iron and manganese.

The 100-fathom contour is taken as the more or less arbitrary boundary separating the deep-sea from the shallow water deposits. This contour line divides the continental masses from the great oceanic depressions. The area of the ocean between the shore and the 100-fathom line is about ten millions of square miles, whereas that between the 100-fathom and 500-fathom lines is only seven millions. The area between the two last-mentioned contours may, therefore, be spoken of as the continental slope.

Marine deposits in general are classified as follows :—

Deep-sea deposits beyond 100 fathoms.	Red clay.	Pelagic deposits formed in deep water far away from land.
	Radiolarian ooze.	
	Diatom ooze.	
	Globigerina ooze.	
	Blue mud.	Terrigenous deposits formed in deep and shallow water close to land masses.
	Red mud.	
	Green mud.	
	Volcanic mud.	
	Coral mud.	
Shallow-water deposits between low-water mark and 100 fathoms.	Sands, gravels, muds, &c.	
Littoral deposits between high- and low-water marks.	Sands, gravels, muds, &c.	

The following table indicates the average depth at which each of the more important deposits is found, the average amount of carbonate of lime present, and the area of distribution :—

	Mean depth in fathoms.	Mean percentage of $CaCO_3$.	Area, sq. miles.
Red clay	2,730	6·70	51,500,000
Radiolarian ooze	2,894	4·01	2,290,400
Diatom ooze	1,477	22·96	10,880,000
Globigerina ooze	1,996	64·53	49,520,000
Pteropod ooze	1,044	79·26	400,000
Coral mud	740	86·41	2,556,800
Coral sand	176		
Other terrigenous deposits ..	1,016	19·20	16,050,000

It is with the deep-sea deposits alone that the authors of the present volume are concerned. These cover more than one-half of the earth's surface, and pass gradually into the shallow-water deposits. Phenomena of erosion are almost entirely absent from the deep sea, and the only mechanical actions of any consequence are those connected with submarine volcanic eruptions. The effects of chemical action, on the other hand, are strongly marked by the occurrence of glauconite, phosphatic nodules, manganese nodules, and zeolites.

The pelagic deposits are essentially made up of the remains of pelagic organisms—pteropods, heteropods, foraminifera, diatoms, and radiolaria—together with the products of volcanic eruptions and secondary products resulting from the decomposition of the latter. Uniformity over large areas is a special characteristic of these deposits. The authors do not admit that any formations identical with the typical pelagic deposits are to be found in the rocks of which the land is composed. They refer, however, in a footnote, to the oceanic deposits of Barbados, recently described by Messrs. Harrison and Jukes-Browne, and to the radiolarian cherts of the south of Scotland described by Dr. Hinde.

The first three chapters deal with the collection and examination of deep-sea deposits, the composition of the samples obtained at 354 stations (this being given in a tabular form) and the general nature and distribution of the different kinds of deposit. The last three chapters treat of the constituents of the deposits, and we will refer to these first. They are classified as follows:—(1) Materials of organic origin; (2) mineral substances of terrestrial and extra-terrestrial origin; and (3) chemical products formed *in situ* on the floor of the ocean.

Nearly all the deep-sea deposits contain traces of albuminoid and other organic matters. These substances give rise to sulphuretted hydrogen by decomposition, and thus lead to the formation of sulphides, especially sulphide of iron, in the blue muds. The food of the deep-sea animals is the organic matter which falls from the surface and intermediate waters. The stomachs of echinoderms, annelids, and other organisms living on the bed of the sea, were always found to be full of the surface layers of the ooze. Thus a large proportion of marine deposits must have passed through the intestines of marine animals. That the same kind of action took place in past time is shown by the extraordinary abundance of worm-casts in some geological formations, as, for example, in the Cambrian rocks of the North-west of Scotland.

The different classes of organisms whose remains occur in the deposits are considered in detail, and the relative frequency of these remains in the different kinds of deposit is described. The division of the oceanic organisms into those which live in the waters and those which dwell on the floor of the ocean—the Plankton and Benthos of Haeckel—is of primary importance from the point of view of the origin of deep-sea deposits. The "Challenger" observations show that 90 per cent. of the carbonate of lime in these deposits is due to the remains of pelagic organisms. The nature of the deposit varies with the latitude, and depends upon the character of the surface fauna and flora. The calcareous organic remains belong principally to pelagic foraminifera, mollusca (pteropods and heteropods), and algæ (coccoliths and rhabdoliths).

The general absence of the exoskeletons of the higher crustacea

and the bones, other than otoliths, of fishes is remarkable, and indicates that hard parts containing a large amount of phosphate of lime and organic matter are especially liable to decomposition and solution. The siliceous organisms which contribute to the formation of widely distributed deposits are radiolaria and diatoms. Sponge spicules are very commonly present, but do not form an important part of any of the deposits examined. Hexactinellid spicules are characteristic of the deeper waters; tetractinellid and monaxonid spicules, on the other hand, occur most abundantly in the shallower regions. Vertebrata are represented principally by the teeth and otoliths of fishes, and the ear-bones of whales; some of these remains, including the *Carcharodon megalodon* of the Crag, are believed to be referable to extinct Tertiary species. More than fifteen hundred sharks' teeth were obtained in one haul at a point in the South Pacific.

The mineral constituents not formed on the floor of the ocean separate themselves into two well-marked groups: (1) pyroclastic materials, including crystals, crystal fragments, pumice, lapilli, and minute glassy particles; (2) detrital materials, resulting from the destruction of crystalline, schisto-crystalline, and epiclastic rocks. The former are universally distributed; the latter are practically limited to a comparatively narrow zone round the land masses, and only occur in the pelagic deposits where these are formed under the influence of ice-drift, or in regions where the surface is exposed to powerful winds blowing from desert regions.

The universal distribution of pyroclastic materials, including pumice, is not surprising when we take into consideration the positions of active volcanoes in relation to the sea and the characteristic phenomena of paroxysmal eruptions. Submarine volcanic action must also contribute to the supply of pyroclastic material. Fragments of pumice varying in size from that of a man's head to that of a mustard seed, and generally more or less rounded, were dredged at many stations, sometimes hundreds of miles from land. The surface layers, in all cases, had been more or less changed to a soft clayey material—a fact of some importance as bearing on the origin of abyssal clays. The three main types of volcanic rocks—acid, intermediate, and basic—are all represented amongst the "Challenger" collections. By the disintegration of these different varieties of pumice, minute glassy particles are formed and minerals liberated, both of which are identical with the constituents of fine volcanic ash. It is not always possible, therefore, to say whether a given crystal or glassy fragment has been produced by the destruction of pumice or by the actions which accompany paroxysmal eruptions.

In many regions of the deep-sea, lapilli and pebble-like fragments of a basic volcanic glass were obtained. These were always found to be more or less altered to a resinous, yellowish green, or reddish brown substance, having all the characters of palagonite. The minerals

embedded in the glass are those of basic igneous rocks—olivine, augite, and plagioclase. The fragments of basic glass are very commonly associated with manganese nodules, and often form the nuclei round which deposition has taken place. In some localities actual deposits of palagonitic tufa, with the different lapilli in their original relative positions, have been brought to the surface enclosed in the larger and somewhat tabular concretions. The change from basic glass to palagonite is shown by analysis to be accompanied by hydration, removal of lime and magnesia, oxidation of the ferrous iron, and addition of alkalies.

Non-volcanic minerals and rocks are, as a rule, absent from the pelagic deposits. The most characteristic minerals are glaucophane, white mica, sericite, tourmaline, zircon, and microcline. Most of the quartz is also of non-volcanic origin. It is in the terrigenous deposits of the continental slope that the above minerals are found.

When the particles which can be extracted from a deep-sea deposit by a magnet are examined under the microscope, a few black and brown spherules may not unfrequently be observed amongst them. These are supposed to be of extra-terrestrial origin. The black spherules rarely exceed ·2 mm. in diameter. They have an external crust of magnetic oxide of iron, and usually an internal metallic nucleus. They would be perfectly spherical in form if it were not for a slight depression or cupule which is almost invariably present. The metallic nucleus either consists of iron or of iron alloyed with cobalt and nickel. The brown spherules have a mean diameter of ·5 mm. and a peculiar radial, excentric, lamellar structure known only in the chondres of meteorites. At first they were believed to be formed of bronzite, the mineral of which the chondres are composed, but more detailed examination has led the authors to the conclusion that the mineral in this case is a monoclinic pyroxene. Blackish-brown inclusions, similar to those occurring in hypersthene, are present in these peculiar bodies, and it is to these inclusions that their magnetic properties are due.

The black and brown spherules were found in the greatest abundance in the red clays of the Central and Southern Pacific. Twenty or thirty black, and five or six brown spherules may usually be obtained from a quart of the clay. Manganese nodules, sharks' teeth, and the ear bones of whales abound in the same localities. In the pelagic deposits of the Central Pacific, other than red clay, magnetic spherules are far less abundant, but a careful search through a large quantity will usually result in the discovery of one or two. These facts clearly point to the conclusion that the spherules are most abundant where the rate of accumulation is slowest.

The chemical deposits forming *in situ* on the floor of the ocean are considered under the following heads :—(1) Clay, (2) Manganese nodules, (3) Glauconite, (4) Phosphate nodules, (5) Zeolites. The argillaceous matter of the red clay and other pelagic deposits is

believed by the authors to be due mainly to the decomposition of volcanic products. They do not deny, however, that a small quantity may be derived from the land.

Concretions rich in hydrated oxide of manganese were found in the Atlantic, Indian, and Pacific Oceans. They were frequently brought up by the trawl from great depths in extraordinary numbers. Thus, from one haul in the South Pacific between two and three bushels were obtained, together with 50 bones of whales, 1,500 sharks' teeth, 6 ice-borne pebbles, and 12 rounded pieces of pumice. The concretions vary in size from microscopic particles to large masses, of unknown dimensions, from which fragments have been torn off by the action of the dredge. The common form is that of more or less rounded or nodular masses, varying from 1 to 15 cm. in diameter. The larger nodules frequently contain several nuclei. The depositions commenced round several foreign bodies lying near each other on the bed of the sea, and increased so as finally to enclose them all in one compound nodule. The nuclei are of the most varied character. They consist of fragments of pumice, lapilli, sharks' teeth, earbones, sponges, and even portions of the local deposit. A concentric arrangement is seen in section. This is brought out by a variation in colour in the successive zones, the lighter zones containing a larger admixture of clayey or earthy material. The manganese hydrate is opaque in the thin sections, and without crystalline form. The characteristic dendritic structure may, however, be seen in the lighter-coloured layers of the nodules.

Dittmar's experiments indicate that the oxygen present is more than sufficient to form a binoxide of manganese. Renard concludes from his analysis that the condition of the manganese as regards oxidation and hydration, is expressed by the formula $MnO_2+\frac{1}{2}H_2O$, and that this compound is associated with 26·7 per cent. of limonite. It must not be supposed that the nodules are pure manganese compounds. They always contain hydrated ferric oxide, as well as clay and other constituents. Dr. Gibson subjected a very large quantity of the material of the nodules to an exhaustive examination for the purpose of determining the presence or absence of the rarer elements. In addition to the ordinary constituents, he found tellurium, lead, copper, molybdenum, thallium, vanadium, zinc, nickel, and cobalt. Nickel is present in sufficient abundance to be estimated, even in small quantities of material.

Mr. Murray believes that the manganese has been chiefly derived from the decomposition of the basic volcanic material with which the nodules are so frequently associated. The amount of manganese present in this material is, however, so small, that if the nodules obtained their supply from this source they would probably be embedded in such a mass of clay as not to be obtainable in great numbers by the dredge. We accordingly agree with the opinion expressed by M. Renard, in a footnote, that the bulk of the

manganese has been obtained in some way from the sea water. May not the presence of manganese in the deep-sea deposits be due to the action described by Dieulafait—precipitation from a solution of the carbonate, in consequence of oxidation—and its local accumulation as nodules to some of the little known chemical processes going on upon the sea-bed? The wide distribution of manganese, in a finely divided state, in the red clay areas of the Pacific, is shown by their chocolate-brown colour, and also by microscopic examination of the deposits themselves.

Another chemical product of great geological interest in course of formation in existing seas is glauconite. It forms an appreciable part of the green sands and muds, and is found as isolated grains in most of the blue muds. The grains rarely, if ever, exceed 1 mm. in diameter. The typical grains are always rounded, often mammillated, and of a black or dark green colour. Their forms often suggest in a vague manner those of foraminifera. Mixed with the typical grains are others, of a pale green colour, which are clearly the casts of those organisms. Foraminifera wholly or partially filled with glauconite, or allied substances, were very commonly observed, and the appearances often suggested the conclusion that the growth of the glauconite in the interior had broken off portions of the shells. We may therefore suppose that the typical grains, of vague form, are produced by growth after the shell has disappeared. The chemical and microscopic characters of the recent glauconite are identical with those of the same substance occurring in our greensands, as may be seen by comparing the description and analysis given in the Survey Memoir on the Geology of the Isle of Wight with those of this Report. The minerals associated with glauconite are those of the terrigenous deposits. All the facts point to the conclusion that glauconite is formed in the first instance in the hollow spaces of calcareous organic remains, and especially in the interior of the shells of the foraminifera.

A few phosphatic nodules were dredged by the "Challenger," and those from the outer edge of the Agulhas bank and from a station in deep water to the south of the Cape of Good Hope are described in detail. They vary in diameter from 1 to 3 cm., as a rule, but may occasionally measure as much as 6 cm. across. The phosphate of lime is amorphous, and merely acts as a cement to the other constituents which are accumulating at the spot where the nodule was obtained. Thus, in the concretions from the station on the Agulhas bank, the phosphate of lime encloses grains of glauconite and quartz; while in those from the deep-water station, the shells of foraminifera filled with and more or less pseudomorphosed by phosphate of lime are to be found.

The only chemical deposits which remain to be described are crystals of phillipsite. These were especially noted in the red clay areas of the central Pacific, where they occasionally make up 20 or

30 per cent. of the deposit. They have been found also in the Central Indian Ocean, and in other deposits besides the red clay. They occur as extremely minute prisms, crossed twins, and as globular or spherulitic aggregates. There appears to be a connection between the distribution of phillipsite and that of the fragments of basic volcanic glass and basaltic lapilli. This leads the authors to conclude that the materials of the former have been derived from the latter; so that there is in this case no violation of the law connecting the formation of zeolites with the decomposition of igneous rocks in their immediate neighbourhood.

We have now to refer briefly to that portion of the report which deals with the nature and distribution of the different deposits. Red clay is found in the greatest depths and at the greatest distance from land. It is the most widely distributed of all the deep-sea deposits, and was found at seventy stations which range in depth from 2,225 to 3,950 fathoms. The colour and amount of hydrated silicate of alumina are both liable to considerable variation. In the North Atlantic the colour is a brick-red, owing to the presence of a thin pellicle of ferric oxide on the surfaces of the minute mineral particles. In the South Pacific and Indian Oceans, the deposit often assumes a chocolate-brown colour, in consequence of the presence of minute rounded grains of peroxide of manganese. Unlike a pure clay, this deposit will fuse in the blow-pipe flame to a black, often magnetic bead. As a rule, the red clay is smooth and soapy to the touch, but occasionally a gritty feeling, due to the presence of pellets of peroxide of manganese, zeolitic crystals, or fragments of pumice, may be noticed. The extraordinary abundance of manganese nodules, sharks' teeth and cetacean ear-bones has already been mentioned. Hydrated silicate of alumina never forms more than one-half of the deposit. Carbonate of lime ranges from 1 or 2 per cent. in the deeper parts to 20 per cent. near the upper limit. It is due to the presence of the remains of calcareous organisms which resemble those living at the surface in the same neighbourhood. Large and small fragments of pumice were dredged and trawled in great numbers and in almost all regions. The mineral constituents are those of volcanic rocks—sanidine, plagioclase, augite, hornblende and magnetite. Ice-borne fragments of rock and terrigenous minerals are found in the Southern Ocean.

Radiolarian ooze, like the red clay, is found only in the greatest depths. The constituents are the same as those of the preceding deposits, but the relative proportions are different. The remains of radiolaria and siliceous organisms make up at least 20 per cent.—in one case 80 per cent.—of the deposit. The nine specimens obtained by the "Challenger" range in depth from 2,350 to 4,475 fathoms. Radiolarian ooze is known only in the Pacific and Indian Oceans. It has not been found in the Atlantic.

Diatom ooze is mainly composed of the frustules of diatoms, but

the remains of radiolaria and sponges may also be present. When wet, it has a yellowish straw or cream colour; when dry it resembles flour. This deposit was found by the "Challenger" in the Southern Ocean only, and within the limits of the Antarctic ice-drift. The mineralogical and petrological constituents are accordingly much more varied than is the case with the previously described deposits. Fragments of granite, micaceous sandstone, amphibolite, gneiss, schists, and slate have been found, together with such minerals as might be derived from these rocks. The ubiquitous volcanic rocks and minerals are also present. Diatom ooze has been found by the "Tuscarora" in the northern part of the Pacific Ocean.

The term globigerina ooze has been applied by the authors to those deposits which contain over 30 per cent. of carbonate of lime, principally due to the shells of pelagic foraminifera belonging to the genera *Globigerina*, *Orbulina*, and *Pulvinulina*. Bottom-living foraminifera, such as those belonging to the Textulariidæ, Miliolidæ, and Nummulinidæ, may also be present, but not to any great extent. Shells of the pelagic mollusca are frequently found. In regions far removed from land, particles of volcanic glass and the minerals felspar, augite, and hornblende are found, without any admixture of land detritus; but where the globigerina ooze approaches land and merges into the terrigenous deposits, a considerable amount of such detritus is present. 118 samples are classed as globigerina ooze; they come from depths ranging from 400 to 2,925 fathoms. Pteropod ooze differs from globigerina ooze only in the greater abundance of pteropod and heteropod shells. It is found in shallower water than the typical globigerina ooze, and is known only in the Atlantic. In the following table, column I. represents the average composition of red clay; column II. that of radiolarian ooze, and column III. that of globigerina ooze:—

		I.		II.		III.	
Carbonate of lime	Pelagic foraminifera ..	4·77		3·11		53·10	
	Bottom-living foraminifera	0·59		0·11		2·13	
	Other organisms	1·34		0·79		9·24	
			6·70		4·01		64·47
Residue	Siliceous organisms ..	2·39		54·44		1·64	
	Minerals	5·56		1·67		3·33	
	Fine washings	85·35		39·88		30·56	
			93·30		95·99		35·53
			100·00		100·00		100·00

The percentage composition of each of the deposits is given in the same way; so that an enormous amount of labour must have been expended in the preparation of this Report.

We come now to the terrigenous deposits. Of these, blue mud is the most widely distributed. The upper layer is red or brown, owing to the presence of ferric oxide or ferric hydrate. Only the lower portions show the characteristic colour. In these, a reduction

of the iron oxide has taken place in consequence of the presence of organic matter, and sulphide of iron is present in a finely divided state. When fresh, these deposits usually have the smell of sulphuretted hydrogen. The proportion of carbonate of lime may vary from a mere trace to 35 per cent. The mineral particles range in size from ·06 to ·3 mm. in diameter, the average being ·115 mm. Quartz particles which are rare or absent in the pelagic deposits are the most abundant of the definitely recognisable constituents; orthoclase, plagioclase, green hornblende, augite, white and black micas, epidote, chlorite, zircon, and tourmaline have also been determined. Glauconite is not characteristic, but may be found in almost all the specimens collected during the cruise. The blue muds differ from the red clays in containing a much larger proportion of minerals (22·48 per cent. on the average), and a smaller proportion of fine washings (61·77 per cent.). They surround nearly all coasts, and are found also in inland seas, such as the Mediterranean. Off the coast of Brazil, a red mud is found in place of the more widely-distributed blue mud. This is said to owe its colour to the ochreous matter carried out to sea by the great South American rivers. It contains, however, less iron than the blue mud, so that the peculiar colour must be due to the absence of the reducing conditions which prevail over the areas covered by blue mud. Similar deposits occur off the Chinese coast near the mouth of the Yang-tse-kiang.

The green muds and sands owe their distinctive features to the presence of glauconite. They grade off on the one side into blue muds and on the other into globigerina ooze. Volcanic muds and sands are found in the immediate neighbourhood of volcanic islands. The volcanic material is mixed with a variable amount of calcareous and siliceous matter, chiefly due to the remains of the usual pelagic organisms. Coral muds and sands surround coral islands in the same way.

More than one hundred and fifty analyses of the deposits and of special substances obtained from them are given in an appendix to the Report.

It will be seen from the brief account we have given of this most valuable Report that the detailed study of specimens in the laboratory has confirmed and extended the conclusions arrived at during the cruise. The deep-sea deposits are essentially made up of the remains of pelagic organisms and volcanic products. Terrigenous material is almost entirely absent. The proportion of carbonate of lime decreases as the depth increases below 2,000 fathoms; this is owing to the solvent action of sea-water on the calcareous organic remains. The deposits of the greatest depths are, therefore, almost entirely derived from siliceous organisms and volcanic material.

The broad features of the distribution of the more important deposits may now be briefly summarised. Proceeding outwards

from the shore, we first meet with the variable deposits of the littoral and shallow-water zones. Banks of sand heaped up under the influence of tidal currents, and wide stretches of mud in the deeper and quieter regions. Here and there occur local accumulations of shells and shelly *débris*. Near the 100-fathom line blue muds are found, and as these are followed down the continental slope, they merge, near its base, into Globigerina ooze—a deposit which extends with wearisome monotony over immense areas. As we descend into the abysses of the ocean, to depths exceeding 2,500 fathoms, the globigerina ooze passes into "grey" ooze, and this again into red clay —the most widely distributed of all the deep-sea deposits.

The physical conditions at great depths are practically the same from the equator to high latitudes. Seasonal changes are absent, and the temperature is uniform. Notwithstanding this, there is a marked variation in the character of the organic remains found in the deposits. Foraminifera are larger and more varied in form in the tropical regions. Coccoliths, rhabdoliths, and the remains of the pelagic mollusca are entirely absent from the deposits of high northern and southern latitudes. These variations with latitude are due to the fact that the organisms which make up so large a portion of the deposits live at the surface, where their distribution is determined by the distribution of temperature.

J. J. H. Teall.

III.

The Evolution of Fins.

OF the multitudinous theories relating to the origin and development of fin-structures in fishes, some may already be transferred to the rank of definite principles, and others are rapidly advancing in probability. The combined results of Comparative Anatomy, Embryology, and Palæontology have already provided ample material for generalisation in certain directions, and the latest discoveries among the early Palæozoic Fishes make known several facts in regard to the primitive characters of fins which it seems impossible to misinterpret.

It is now the prevalent—perhaps universal—belief that the paired fins of fishes, or those which correspond to the four limbs of lung-breathers, are essentially the same in nature as the median fins which fringe the back and tail. This view is supported, not merely by the identity of structure so frequently observed between the paired and median fins, but also by the presence, in certain embryos of sharks and skates, of a transitory ridge of thickened epiblast between the pectoral and pelvic fins on each side, precisely like the original ridge from which the median fin-fold arises. Since the median fins are proved to result from various modifications of a once-continuous fold of skin and its skeletal supports, it is thus not too much to assume that the paired fins have likewise become differentiated from a similar fold, once extending continuously along each side of the trunk. If, therefore, the primitive character of the median fin-fold can be ascertained, and the various phases in its evolution traced, it ought to be possible to discover the mode and order of development of the more modified paired fins.

Through the numerous discoveries of Elasmobranch skeletons in the Carboniferous and Permian Formations of Europe, recently described, and from existing knowledge of many other early fishes, the primitive arrangement of the supports of the median fin-fold is fortunately well ascertained. There is no longer any doubt that the series of parallel vertical rods of cartilage supporting this fin-fold is directly continuous with the series of neural and hæmal arches of the axial skeleton of the trunk. In other words, the endoskeletal fin-supports are identical in origin with the processes of the vertebræ. This is well shown in the Pleuracanthidæ, in which the dorsal fin-supports of the caudal region are equal in number to the neural

arches (Fig. 1), while those of the abdominal region are exactly twice as numerous as the arches (Figs. 1, 2).

Again, it is a well-ascertained fact in Palæontology that the median fin-supports of the more primitive fishes are all divided by tranverse joints into two or three pieces, as shown in the Pleuracanthidæ (Figs. 1, 2). The distal piece supports the delicate dermal hair-like rays (when present), which are eventually replaced in higher fishes by typical fin-rays. As the dermal fin-rays are developed, the two or three vertical rows of fin-supports gradually become reduced to one; and if not fused together the remnant of this series, instead of retaining its association with the neural arches, finally becomes correlated numerically with the series of dermal rays it supports. The only exception to this rule known to the writer occurs

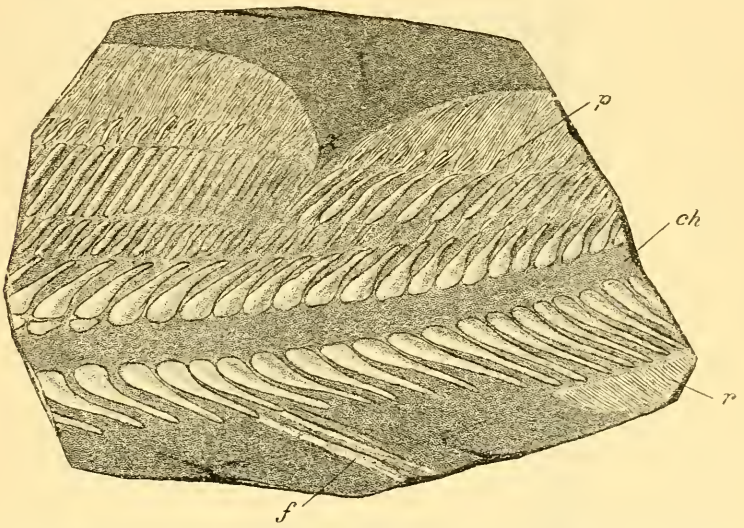

FIG. 1.—Portion of Trunk of *Xenacanthus decheni*, from the Lower Permian of Bohemia *ch*. Position of notochord; *f*. Anal fin; *p*, *r*. Dorsal and ventral portions of caudal fin. After A. Fritsch.[1]

in the caudal fin of some Crossopterygii (*e.g.*, the Cœlacanthidæ), where the fin-supports are equal in number both to the neural and hæmal arches and to the dermal rays.

The atrophy, or "shortening up," of the endoskeletal fin-supports seems to be the invariable result of specialisation in the median fins; and all the higher types of fishes with well-developed fin-rays exhibit the numerical correspondence of the supports, except where secondary fusion of these supports has taken place. The dorsal and anal median fins of such fishes, when not a continuous fold, owe their actual extent to the disappearance, from some unknown cause, of the rest of the fin-membrane, of which the rudimentary series of

[1] We are indebted to Dr. Anton Fritsch for the loan of Figs. 1, 2, 3, 7 and 8, from his well-known work on "The Fauna of the Bohemian Gas Coal."

tapering, rod-like supports is often still preserved between the lateral muscles of the trunk.

There is, however, another method of subdivision of the median fins, specially characteristic of the Elasmobranchii, Crossopterygii, Chimæroidei, and, perhaps, some Dipnoi. At two points in the dorsal region—and never at more or less than two points, so far as known[2]—there is a tendency towards the crowding and concentration of the endoskeletal fin-supports, usually resulting in the more or less complete fusion of the proximal parts that withstand the growth-pressure, accompanied by the varying fusion of the distal remnants of the mass of supports. At one or two points in the anal region, a similar phenomenon occurs. This is illustrated, to

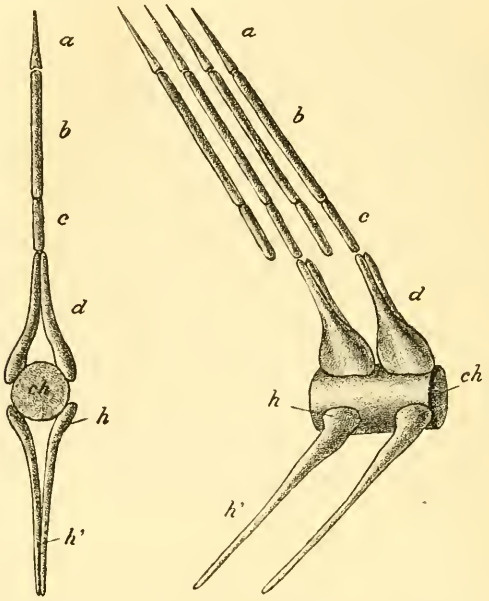

FIG. 2.—Abdominal vertebral segments of *Pleuracanthus*. *ch*. Notochord; *d*. Neural arch; *h*. Hæmal arch; *a*, *b*, *c*. The three series of dorsal fin-supports. After A. Fritsch.

some extent, by the accompanying drawing of the anterior anal fin of *Xenacanthus* (Fig. 3), in which two hæmal arches (*b*, *c*) directly support the crowded and modified cartilages. A more striking case, however, occurs in the dorsal fin of the Devonian Crossopterygian *Holoptychius* (Fig. 4), in which the basal portions of no less than seven supports are fused into one mass, and remnants of a still greater number are clustered together distally. A further stage in this specialisation is met with in the anterior dorsal fin of the Crossopterygian Cœlacanthidæ, where the completely fused triangular base alone remains, the distal remnants of the supports are lost, and the dermal fin-rays are

[2] The apparent exceptions of *Notidanus* and *Chlamydoselache* require examination.

in direct contact with the "basipterygium," reminding one precisely of the pelvic fins of a modern bony fish.

To sum up:—

(i.) The median fins are primitively supported by a series of rods, in continuation of the series of neural and hæmal arches of the axial skeleton of the trunk.

(ii.) The supporting rods gradually become atrophied as the dermal fin-rays develop, and are correlated in number with these rays in the higher fishes.

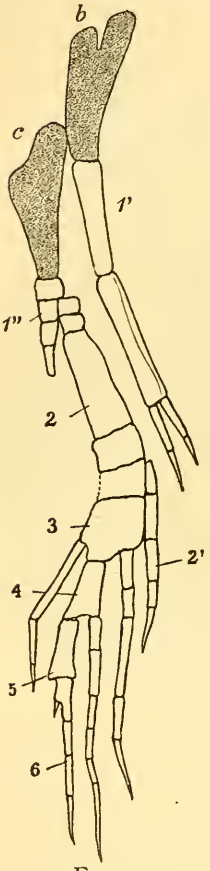

FIG. 3.

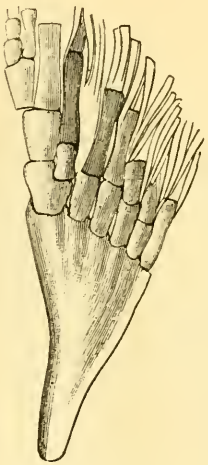

FIG. 4.

FIG. 3.—Anterior anal fin of *Xenacanthus decheni*. *b*, *c*. Hæmal arches. After A. Fritsch.

FIG. 4.—Posterior dorsal fin of *Holoptychius* (*Glyptolepis*) *leptopterus*, from the Old Red Sandstone of Nairnshire, showing traces of dermal rays distally. From the Catal. Foss. Fishes, Brit. Mus., pt. ii.

(iii.) The sub-division of the once-continuous fin results either (*a*) from the loss of portions of the dermal membrane and the subsequent degeneration of the supports; or (*b*) from the crowding together of the supports at two points in the dorsal region, and at one or two points in the anal region.

Now, to apply these results to the case of the paired fins. If the hypothetical primitive lateral fin-fold on each side was originally supported by a series of parallel rods of cartilage directed outwards,

no pectoral or pelvic fin hitherto described retains the arrangement. The well-known paddle of *Ceratodus* and of the Pleuracanth Sharks (Fig. 7), with its median axis and double series of divergent branches, has hitherto been the nearest approach to the primitive type; and this was at first regarded as the "archipterygium" by Gegenbaur. Within the last few years, however, several specimens of a peculiar elongated Elasmobranch, with *Cladodus*-shaped teeth, have been discovered in the Cleveland shale at the base of the Carboniferous Formation in Ohio, U.S.A.[3]; and, in the opinion of the present writer, the pectoral fin of this shark at last reveals one of the least-modified

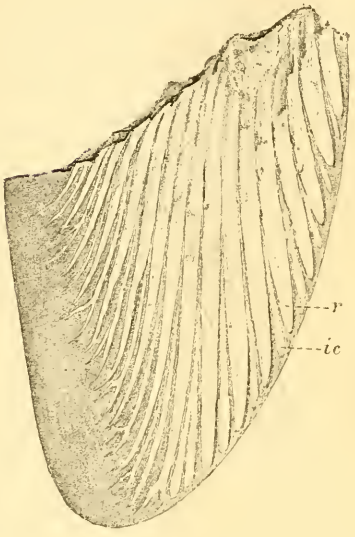

Fig. 5.

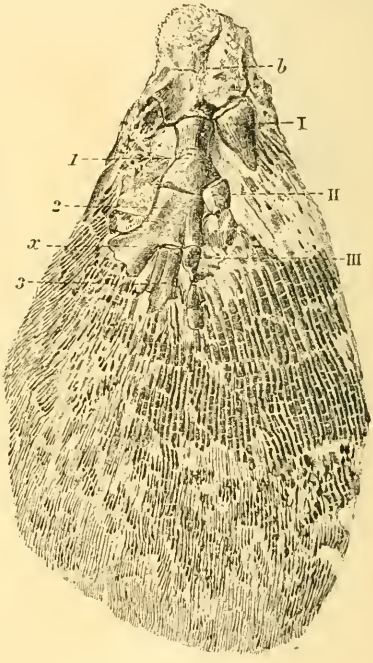

Fig. 6.

Fig. 5.—Pectoral fin of "*Cladodus*" *fyleri*, from the Lower Carboniferous of Ohio. *r*. Complete rays; *ic*. Intercalated remnants of rays. Right border preaxial. From a specimen presented to the British Museum by Dr. J. S. Newberry.

Fig. 6.—Pectoral fin of *Eusthenopteron foordi*, from the Upper Devonian of Canada. *b*. Basal cartilage of fin. Right border preaxial. Specimen in British Museum.

conditions of the endoskeleton of the lateral fin-fold that can be expected in a fish in which this fold is already sub-divided into its ordinary two remnants. No fused basals can be detected with certainty in any of the specimens the writer has examined; and, as shown in the fin represented in Fig. 5, none of the parallel cartilaginous rods that support the fin-membrane are transversely jointed. The most singular feature of the fin consists in the evidence it affords of that crowding and concentration we have already

[3] See photographs by J. S. Newberry, in Monogr. U.S. Geol. Survey, No. xvi., pls. xliv.–xlvi.

observed in the differentiated median fins of the earlier fishes. Between the extremities of the unaltered parallel bars (*r.*) there are the remnants of similar bars (*ic.*) that have evidently been reduced and displaced by growth pressure, just as a middle digit sometimes becomes reduced and displaced from connection with the carpus in *Ichthyosaurus*, some marine mammals, and birds. Most of the cartilages bifurcate a little distally, but that is a minor matter. The segmentation of the rays, the persistence of one of the middle rays, with the concomitant partial fusion of the still further crowded and reduced bordering rays, would soon, in the writer's opinion, result in the "archipterygium" of Gegenbaur. It is, moreover, significant

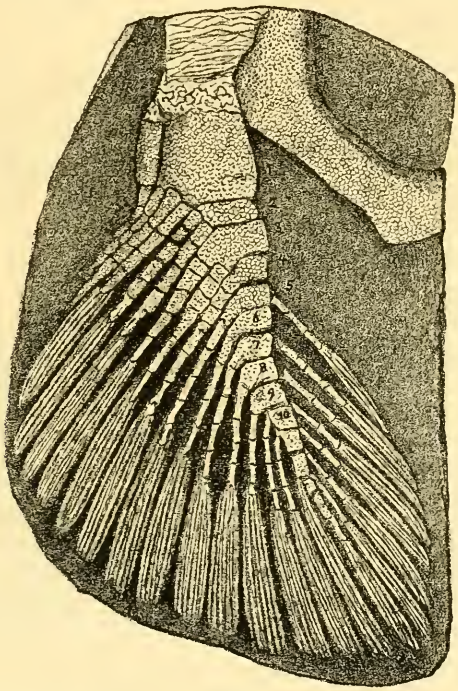

FIG. 7.—Pectoral fin of *Xenacanthus decheni*, showing dermal rays. After A. Fritsch. The left border is preaxial, the right border postaxial. In this interpretation the present writer is directly opposed to Dr. Fritsch.

that the anterior (preaxial) rays are much more robust than the posterior (postaxial) rays, exactly as in all known examples of the "archipterygium."

Whether or not the biserial "archipterygium" results from the crowding of the parallel cartilaginous rays as now suggested, or whether it arises much like the dorsal fin of *Holoptychius* (Fig. 4), as supposed by Dr. Anton Fritsch (Fig. 8), Palæontology appears to have demonstrated that this is the type of paired fin from which all later forms have been derived by abbreviation. Among Crossopterygian Ganoids, the Holoptychiidæ are admitted to rank below the

Rhizodontidæ, and these again below the modern Polypteridæ. In the first family, the pectoral "archipterygium" is as much elongated as in *Ceratodus*; in the second family, which survived after the first had become extinct, the same fin still shows the branched arrangement of cartilages (Fig. 6), but is much abbreviated; while in the

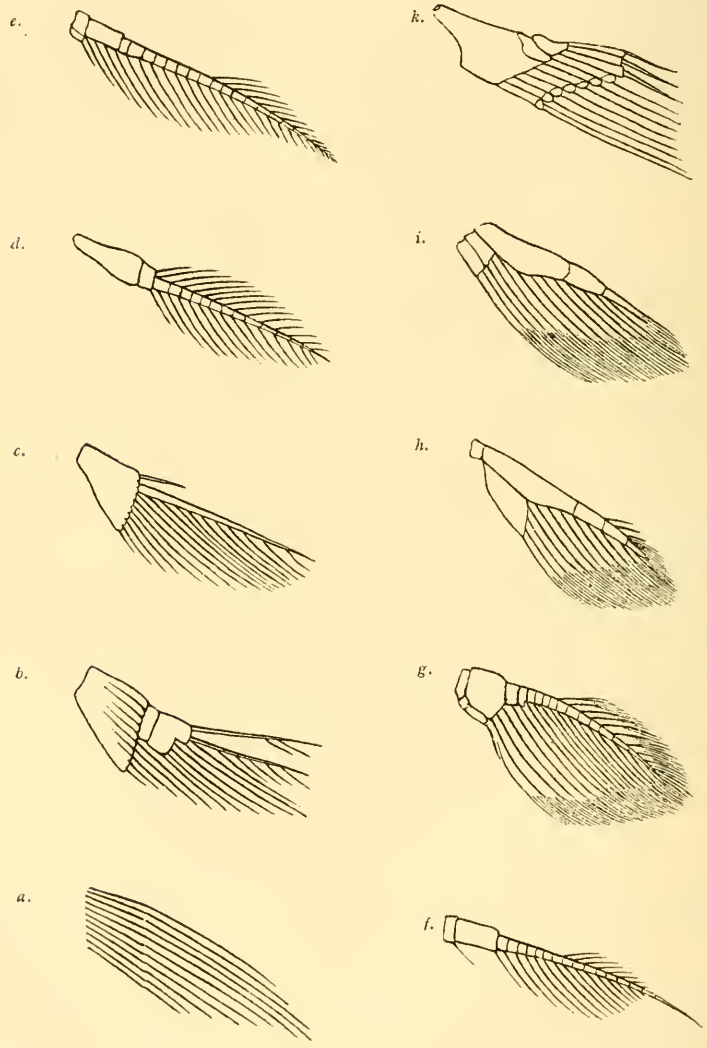

FIG. 8.—Series of Diagrams illustrating the views of Dr. Anton Fritsch in regard to the stages in the Evolution of the Paired Fins.

a. Hypothetical primitive fin.
b. Atavistic pelvic fin of aged female *Xenacanthus*.
c. Pelvic fin of young female *Pleuracanthus*.
d. Pectoral fin of *Ceratodus*.
e. Pectoral fin of *Orthacanthus*.
f. Pectoral fin of *Pleuracanthus*.
g. Pectoral fin of *Xenacanthus* (this and the following figs. with fringing dermal rays).
h, *i*. Pectoral fins of recent Sharks.
k. Pelvic fin of Sturgeon (*Acipenser*).

recent Polypteridæ, this short lobate fin is almost or quite tribasal, as in a typical modern shark. Among Elasmobranchii, the Carboni-

ferous and Permian genera *Xenacanthus* (Fig. 7 and Fig. 8, *g*) and *Pleuracanthus* (Fig. 8, *f*) also demonstrate how, by abbreviation and by the atrophy of the rays on the hinder (or postaxial) border, the same change has taken place; and clearer proof of the identity of the Selachian metapterygium with the axis of the so-called "archipterygium" can scarcely be required.

In short, so far as the facts of Palæontology are known, the evolution of the paired fins is essentially similar to that of the median fins; the only difference being that, while there are two different modes of reduction in the latter, the crowding of the fin-supports at two definite points is the sole process in the former. During this process, until the "shortening up" of the fin-supports begins, there is a tendency towards the contraction of the base of the fin to its narrowest limits; as soon as the shortening has made considerable progress, the basal portion again becomes relatively broad—in the pectoral fin, tribasal or polybasal. Just as the dermal rays in the median fins arising thus are never correlated numerically with their supports, so also the rays of the paired fins never bear any numerical relation to their basal cartilages. The results being the same, the causes are presumably the same; and it is an interesting problem to determine why both the dorsal fold and the lateral fold, when subdivided by concentration, invariably persists only at two points.

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A. Smith Woodward.

IV.

Some Salient Points in the Study of Mammals during 1891.

THE selection of certain points for special notice in the zoological work of any particular year is always a somewhat invidious task, and nowhere is it more so than in the case of mammals. The past year will, however, long be marked as a memorable one in the annals of the Mammalia, as being the one in which our first definite information was acquired as to the structure and affinities of the Marsupial Mole (*Notoryctes*) from the deserts of Central South Australia, the existence of which was first recorded in 1888. Such an important addition to the living Mammalian fauna of the world has not occurred since the publication, in 1860, of M. Du Chaillu's description of the West African insectivorous mammal *Potamogale;* both that genus and *Notoryctes* being the sole representatives of special families in the groups to which they respectively belong. Important as the addition of the Marsupial Mole to the ranks of the mammals undoubtedly is, yet the creature is not of the supreme interest which the preliminary notices, published in 1888, led some naturalists to think might prove to be the case. There were, indeed, suggestions in those notices of affinity with some of the small fossil mammals of the Jurassic rocks of Europe; and it was even doubtful whether the creature would turn out to be a Monotreme or a Marsupial, or something between the two.

Dr. Stirling's account[1] of *Notoryctes* leads us, however, to believe that the animal must certainly be included in the Marsupial order; where it would seem that it should form the representative of a distinct family in the so-called Polyprotodont sub-order of that group, or that which includes the Opossums, Bandicoots, Dasyures, Thylacine, &c. In the original description, beyond the bare reference of the animal to the Marsupials, nothing definite is said as to its exact serial position; but this deficiency has been made good by Mr. J. D. Ogilby, who, in his "Hand-List of Australian Mammals,"[2] places the family Notoryctidæ among the Polyprotodonts. It is true, indeed, that the last-named writer considers that in the Marsupial Mole "we

[1] *Proc. Zool. Soc.*, 1891, pp. 327–329; and *Trans. Royal Soc. S. Australia*, 1891, pp. 154–187, pls. ii.–ix.

[2] 8vo, Sydney, 1891.

have at last obtained a definite connecting link between the Monotremes and Marsupials;" and he is even inclined to think that the creature will eventually have to be placed in the former group, now represented, as we may remind our readers, solely by the Duck-bill and the Echidnas. We fail, however, to detect any marked signs of affinity with the latter group; the large size of the clavicles, and the abortion of the "marsupial," or epipubic bones, on which stress has been laid, not being characters of any ordinal importance.

Having said thus much as to the affinities of this strange creature, our readers who have not had the opportunity of studying the original papers will naturally expect a few words as to its appearance and habits. In length the new Marsupial Mole measures a little over six-and-a-half inches, of which the tail takes rather more than an inch. Its long and silky hair is of a golden red tint; while the general form of the body is mole-like (Fig. 1). It has no externally visible eyes, neither is it provided with distinct conchs to the ear. The short and stout limbs are adapted for digging, and are each furnished with five digits. Whereas, however, the digits of the hind limb (Fig. 2, Nos. 4, 5) are subequal in size, in the fore limb (Fig. 2, Nos. 1, 3) the claws of the third and fourth digits are enormously developed, so as to form powerful digging

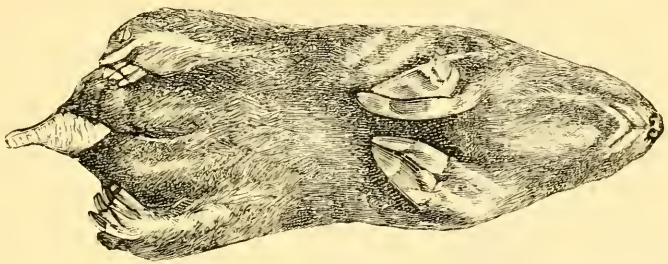

FIG. 1.—Under surface of the Marsupial Mole (*Notoryctes typhlops*). ⅔ natural size. After Stirling.[3]

organs, behind which the small claws of the other digits are concealed. The muzzle of the animal is protected by a curious flat horny shield, divided by a transverse ridge into two moieties; and the tail is a strange, leathery appendage, marked by conspicuous rings, and terminating in a kind of knob. The teeth—especially those in the anterior portions of the jaws—are small and feeble; the hinder ones being of that primitive type known as the tritubercular, that is to say, those of the upper jaw carry three distinct cusps arranged in a triangle, with the apex directed inwardly.

Notoryctes lives in or on the sand of the Australian deserts, burrowing at a short distance beneath the surface, and emerging for a time after travelling for longer or shorter distances below. When entering the sand afresh, it starts in an oblique direction. The horny shield on the nose serves as a protection to the conical snout in

3 We are indebted to the Publisher of *La Nature*, Paris, for this and the following figure.

burrowing; but the chief actual work is performed by the two large claws of the fore limb, while the sand is thrown backwards by the hind feet, so as to completely block up the tunnel after the animal has passed. The food of *Notoryctes* doubtless consists of the small invertebrate animals found in the desert sand.

The zoological world will look forward with great interest to the discovery of the mode of reproduction of this latest addition to the Marsupial order.

Although of infinitely less morphological importance than the Marsupial Mole, a considerable amount of interest attaches to the discovery, during the past year, of an entirely new type of Gazelle from Somali-land. So different, indeed, is this animal from all other gazelles that it has been described [4] as the representative of a distinct genus, under the name of *Ammodorcas clarkei*. Our readers may be aware that, with the exception of the aberrant Gerenuk (*Lithocranius walleri*) of Somali-land, the whole of the gazelles hitherto known are,

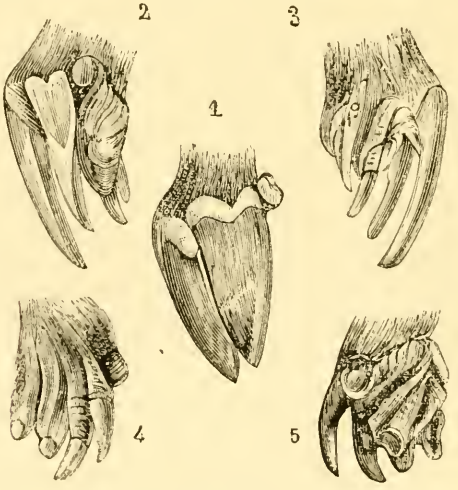

FIG. 2.—Feet of the Marsupial Mole. About $\frac{3}{2}$ natural size. 1. Outer aspect of left fore foot. 2. Profile view of same. 3. Inner aspect of same. 4. Upper surface of left hind foot. 5. Palmar surface of same.—After Stirling.

by common consent, classed by zoologists in the single genus *Gazella*. As a rule, the horns of the gazelles are either lyrate or sublyrate, with their tips bent inwardly. In the gerenuk, or Waller's Gazelle, the horns are, on the other hand, bent forwards at their tips in a distinct hook, so that they are not unlike those of the chamois, if turned the wrong way forwards. There are, moreover, important points of difference between the skull of the gerenuk and those of the true gazelles—notably as regards the much smaller proportionate size of the molar teeth in the former. Now Clarke's Gazelle occupies a somewhat intermediate position between ordinary

[4] Thomas, *Proc. Zool. Soc.*, 1891, pp. 207–210, pls. xxi., xxii.

gazelles and the gerenuk: the horns curving evenly upwards and forwards, with their basal halves running backwards, and the terminal portions nearly vertically upwards, so that the main curvature is exactly in the opposite direction to that obtaining in ordinary gazelles and the gerenuk. The horns of this curious creature are, therefore, more like those of a Reed-buck (*Cervicapra*) than those of a gazelle; and since the skull presents characters connecting the genus *Gazella* with *Lithocranius*, it is clear that we have in the Somaliland *Ammodorcas* an animal of considerably more interest than the ordinary "new species."

An unusual, if not an unparalleled event in the annals of recent zoology, is the description of a new genus and species based upon a single specimen which is not forthcoming. Such, however, is the case with the new mammal from Sumatra described by Professor Hubrecht[5] as *Trichomanis hoeveni*. The animal so named was kept for some time in captivity in Sumatra, in which island it was said to have been captured, but was lost on the voyage to Europe. It is described as of the approximate size of a large cat, having a long snout, and a slender cylindrical tongue, frequently thrust out to catch the ants on which the creature fed. The general colour of the fur is said to be grey, with a black stripe down the back; while the tail is described as being bushy, and the claws long and powerful. Such a description reminds us more of the South American Ant-eaters than of any other mammal, and it would certainly be a most remarkable and interesting circumstance if this group were represented in the islands of the Malayan region. We venture, however, to think that zoologists will have some hesitation in admitting this new mammal into their lists until further evidence of its existence is obtained.

R. Lydekker.

(To be concluded next month.)

[5] *Nature*, vol. xliv., p. 468, 1891.

V.

English Lake Dwellings.

THE records of pile dwellings in England are not numerous, and by far the most important is the one discovered a few years ago in Holderness by Mr. Thomas Boynton. Other examples have been found, principally in the Eastern counties, where, as in Holderness, meres and morasses were formerly abundant. In 1856, a small sheet of water was drained at Wretham, in Norfolk, and indications of a pile dwelling were found, which were described by Sir Charles Bunbury (1). Ten years afterwards General Pitt-Rivers (2) recorded the occurrence of numerous timbers driven into peat outside London Wall, and in Southwark, together with numerous bones and pottery, mostly Samian, and some Roman coins. An extremely interesting description of the remains is given. The Rev. H. Jones explored a pile dwelling at Barton Mere, near Bury St. Edmunds, in 1867 (3), and Dr. S. Palmer described the discovery of oaken piles dug out of a bog on Cold Ash Common, in Berkshire. In each instance, wooden piles, more or less pointed, were found driven into boggy ground, in some cases, as at Barton, supported by large stones. Associated with them were large quantities of bones of animals used as food, sometimes pottery and implements of bronze or iron. An isolated example of a pile dwelling has been found in Llangorse Lake, in South Wales (4, p. 660), it consists of an island of piles supported by stones, about thirty yards in diameter, in a depth of three feet of water, forming a central platform round which are arranged other piles in groups, from which it is inferred that the dwellings were arranged in groups round a central platform. Bones of the pig, cow, sheep, and horse, with those also of the red deer and wild boar, were found in great abundance. At this period the horse evidently formed an article of food. Some fragments of pottery were found with the bones.

Holderness, the south-eastern portion of Yorkshire, is a low-lying district whose almost uniform flatness is only relieved by occasional rounded hills of gravel and sand. Until about eighty years ago, it was marshy, boggy, and little cultivated; it was then drained, the water being conveyed to the sea by drains at low tide and prevented returning at high tide by an arrangement of sluices. The series of plant-laden semi-stagnant meres, connected with each other by equally sluggish streams, have given place to fertile, highly cultivated plains. The ground is highest near the sea-shore, immense

tracts inland being below the sea level; the natural drainage runs inland, and is emptied into the Humber.

In 1880, the Commissioners of the Beverley and Barmston Drain deepened one of its branches emerging at Skipsea. At Ulrome the drain is cut through one of the ancient meres, and has exposed on its surface the remains of a pile-dwelling. On the eastward edge of the mere is some slightly rising ground which is at the present time twenty-five feet below the sea-level. The pile-dwelling was erected on the shores of the lake and connected with the rising ground adjoining by a pair of large timbers extending parallel with each other, about five feet apart. The upper surface of the timbers is hewn flat, and they have been carefully fixed in position, evidently to form a means of communication between the shore and the platform of the pile structure. The latter consists, at the base, of a number of rough, unhewn trunks of trees, from which the branches have been removed, placed horizontally on the bottom of the lake. They are, in some cases, twenty feet in length, and a foot and a-half in diameter; they extend in an east to west direction, and are held in position by rudely pointed stakes. The addition of timbers placed diagonally against the larger trunks appears to indicate that it may have been necessary to resist the force of a current running in a northerly direction. Between the larger timbers shorter trunks were placed transversely, resulting in a rude but solid and compact framework. This structure, fastened in position by stakes four to six feet in length, driven firmly into the bottom of the lake, forms a rectangular platform thirty yards in length from east to west, and eighteen in breadth in the opposite direction. Oak, ash, birch, hazel, and willow appear to have been used indiscriminately to form the platform. All the interstices between the timbers were filled up level with the top with broken wood and twigs until a level surface was obtained; this, in its turn, being apparently covered with fragments of bark and sand. The eastern end of the platform rests on the gravel shore of the mere, the opposite one is separated from the gravel base by a varying thickness of peat. On the foundation thus securely formed were erected the dwellings of the builders, though no evidence remains of them to-day.

Amongst the bark and sticks of this structure a number of implements and some fragments of pottery have been found. The pottery is dark-coloured, half-baked, and possesses all the characteristics of Celtic origin at an early period. Flint flakes, which may have been used as knives, and for such purposes as cleaning skins, were found. Implements of stone, sharpened or pointed and pierced in the middle, for the introduction of a wooden handle, were used as hammers; picks and hammers were also made from the antlers of the red deer. The large leg bones of the ox, broken diagonally midway between the extremities and pierced near the joint with a circular hole for the insertion of a stick, may have been used as

hoes; the fractured surface is more or less smoothened, and seems to indicate such a use. A large granitoid stone of oval form, exceeding a foot in diameter, with a smooth, flat surface, was found, which may have been used for grinding food; and other smaller rounded and abraded stones were apparently used to pound corn. Numerous hazel nuts were obtained. In addition to the bones of ox, red deer, horse, sheep, dogs, and smaller animals, the jaws of wolves and tusks of wild boars were found. The objects were all obtained from a depth of more than six feet below the surface of the ground.

The excavation proved that the structure which has been described, for some reason or other became untenable, and apparently, after the lapse of a considerable time, a second one was erected above it. The two platforms together occupy a thickness of four to five feet. The newer one is arranged much in the same way as the lower. Timbers are placed horizontally and held in position by piles which not infrequently pierce the timbers of the lower stage, which at that time must have become more or less decayed. The later piles are distinguished from the earlier ones by possessing a long sharp point, and appear to have been shaped by a sharp metal instrument, those of the lower being cut by flint implements. Between the two platforms is an intervening mass of peaty brushwood two feet in thickness, and above the upper one three feet of peat, and one foot of warpy soil connects it with the surface of the ground. Between the two platforms bronze implements have been found, amongst others, a fine spear-head. There is no evidence of iron tools, and taking this fact into consideration it may be reasonably inferred that the latter metal was unknown to the pile-dwellers, and we may approximately form an idea of the period during which the structure was erected. The use of iron was known to the people whom the Romans found occupying the country, and it was probably in use two or three centuries previously; so that the later pile-dwellers must have occupied their abode earlier than the second or third century B.C. The lower structure existed during a period of much greater antiquity.

Since the discovery of the pile dwelling at Ulrome others have been found, and it is probable that they exist in considerable numbers throughout the district.

During excavations made at Preston, in Lancashire, three or four years ago, the Rev. E. Maule Cole found evidences of a pile-dwelling, which were recorded in the *Leeds Mercury*, but so far the matter has not received the attention it appeared to deserve.

Probably the first records of Lake dwellings were made in Ireland, where this method of habitation has been in existence from remote periods to comparatively recent times. There is documentary evidence that some of the Irish Crannogs were in existence and occupied in the time of Elizabeth. They were usually approached in canoes, and were not connected with the shore by a gangway. In

Scotland a large number of similar structures have been discovered. Not very long after attention had been directed to the Irish Lake dwellings, similar erections were found on the shores of several of the Swiss Lakes, and Dr. Ferdinand Keller (4) expended much time and energy in their investigation. Dr. Robert Monro (5, p. 302) has ventured an opinion that the original British Celts, who were probably the builders of the Lake dwellings, were an offshoot of the founders of the Swiss Lake dwellings, who emigrated to Britain and spread northwards and westwards over Scotland and Ireland. On this hypothesis, it is probable that the first settlers would occupy the eastern counties of England, and migrate thence northward and westward. This seems to be borne out by the evidence of the Yorkshire examples, which, if the objects found in them may be considered to indicate the period of occupation, are as early or earlier than those of any other locality in the British Islands.

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James W. Davis.

VI.

Marine Snakes.

AS in the case of the Mammalia, in which group we find a gradational series from amphibious to absolutely marine animals through such types as the Sea-Otters, the Eared Seals, the true Seals to the Sirenians and Cetaceans, the Reptiles present us with examples of almost every degree of adaptation to marine life. The Galapagos Iguana, living on land, but entering the sea for feeding on sea-weeds; the estuarine Crocodiles and Trionychoids, occasionally wandering miles out at sea, and of which *Crocodilus porosus* and *Pelochelys* are living representatives; the Turtles and Leatherbacks, essentially marine forms, which are, however, compelled to resort to land to deposit their eggs; the extinct viviparous Plesiosaurs and Ichthyosaurs are well-known examples. But no better instance of gradual modification from terrestrial into marine forms could be found than in the Ophidia living at the present day, among which also are to be found the only recent reptilian types that, being viviparous, never leave the water. These snakes are the Hydrophids, or Sea-snakes, and it is with the state of our knowledge of these and some other snakes allied to them in their mode of life, that it is proposed to deal in the present review.

From the time of Schlegel (15) and Duméril and Bibron (5) down to the present day, the Hydrophids, or poisonous marine snakes, have almost universally been regarded as a distinct and perfectly natural family. Their affinity to the Elapidæ, or poisonous terrestrial colubrine snakes had not been overlooked, nor could have been, considering the very striking resemblance which one of them, *Platurus*, bears to *Elaps* and *Bungarus*. But the whole classification of snakes was most artificial, especially when, as has been so often the case, the division into poisonous and non-poisonous groups primed all others. In the recently-published volume of Blanford's Fauna of India dealing with the Reptiles (3) I have abandoned the character of the dentition in the definition of families, and the great group Colubridæ is divided into three parallel series:—The Aglypha, in which all the teeth are solid; the Opisthoglypha, with posterior grooved fangs; and the Proteroglypha, with anterior grooved fangs. The Hydrophids naturally find their place in the latter division, and, although they are placed together as a sub-family near the Elapinæ, I am far from satisfied that such an arrangement

answers to the aim of modern classification, viz., that it shall correctly express their blood-relationship.

A glance at *Platurus*—and, better still, a comparison of its palato-maxillary arch with the same in the other Opisthoglypha—shows it to agree so perfectly with some of the terrestrial Elapines, that we cannot resist the conclusion that it should be regarded as one of the latter, which has become adapted to marine life, in the same way as some of the Aglypha and Opisthoglypha have become modified into such forms as *Chersydrus* and *Hipistes*. For it is a fact frequently overlooked, though ascertained by Cantor (4), Stoliczka (16), Theobald (17), and other naturalists in India, that these harmless snakes are as much marine as the Hydrophids, to which they bear so striking a superficial resemblance as to be almost undistinguishable from them when seen wriggling in the fishing nets, or floating or gracefully swimming on the surface of the water. But their tail, although compressed, does not assume the oar-shape characteristic of the Hydrophids, nor are the neural and hæmal processes so strongly developed as in the latter. An examination of the scaling of the head also

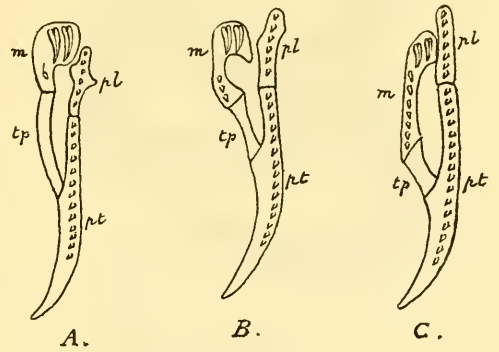

FIG. 1.—Views of right palato-maxillary arch. A. *Platurus*; B. *Enhydris*; C. *Hydrus*. *m*. Maxillary; *pl*. Palatine; *pt*. Pterygoid; *tp*. Transpalatine. (From the "Fauna of British India.")

reveals important differences which show these snakes not to have been derived from the same stock as yielded the Hydrophids, and, therefore, that the resemblance between the two types is merely due to convergence, the result of adaptation to a similar mode of life.

Then, again, the true Hydrophids, and particularly the genera *Hydrus*, *Hydrophis*, *Distira*, although allied to the terrestrial Elapidæ, are, with regard to the structure of the palato-maxillary bones, so much less specialised that it must be assumed that they arose from some no longer existing colubrine type, of which both the terrestrial and marine forms are the descendants. In this instance, therefore, as in so many others, what appears to form a natural group would be merely an association of converging forms.

The snakes that live in the sea may be referred to four natural groups:—

1. Aglyphous, harmless: *Chersydrus*, one species, inhabiting the mouths of rivers and the coasts of Southern India, Burma, the Malay Peninsula and Archipelago, and New Guinea.

2. Opisthoglyphous, harmless, to man at any rate: *Cerberus*, *Hypsirhina*, *Fordonia*, *Cantoria*, *Hipistes*, altogether about a dozen species found in rivers, and on the coasts of India, Burma, the Malay Peninsula and Archipelago, the Papuan Islands, and North Australia.

3. Elaps-like Proteroglypha: *Platurus*, two species.

4. True Hydrophids: *Aipysurus*, *Acalyptus*, *Pelagophis*, *Hydrus*, *Enhydris*, *Hydrophis*, *Enhydrina*, *Distira*, about fifty species altogether.

That *Platurus* is not so absolutely aquatic in its habits as the other sea-snakes, was suspected by Günther in 1864 (9), and this supposition has been verified by Jagor (9, p. 209), Bavay (1), and

FIG. 2.—*Chersydrus granulatus*, a harmless Marine Snake. (From the "Fauna of British India.")

Hagen (in van Lidth de Jeude, 13). The former traveller found a large specimen on a rocky island in the Philippines, at an elevation of 60 feet above sea-level, and one was obtained by Hagen in the forests of Serdang, Sumatra, at a distance of nearly a day's journey from the sea. It has also been repeatedly observed that *Platurus* may be handled with impunity, never attempting to bite, notwithstanding the deadly poison-apparatus of which it is possessed. Its gentle disposition is well known to the colonists of New Caledonia, and the confidence they have in it led to a fatal accident, which is related in an interesting pamphlet by Dr. Forné (7), one of the medical officers at Noumea. A convict, trusting to his experience of *Platurus*, handled a sea-snake which afterwards proved to be a

Hydrophid (*Distira ornata, var. ocellata*); bitten in the hand, the man died nine hours later.

The Hydrophids, on the other hand, are most irascible, and not known ever to leave the water; their movements, when out of that element, being very awkward. In their attempts to bite they will even turn round to wound their own bodies, as observed by Cantor (4), this being partly due to the fact that they are nearly blind in the daytime. S. Kneeland (12) relates his experience with some Hydrophids in Manilla Bay: "We were anchored about three miles from land, in water twenty feet deep. To while away the time we fished, but either our tackle was too clumsy or our bait unsuitable, for we had not even a bite all day. As night came on we kept our lines in the water merely for experiment's sake, without the remotest

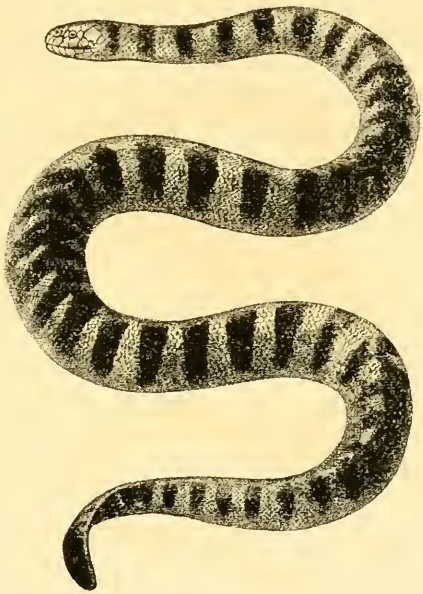

FIG. 3.—*Enhydrina valakadien*, a poisonous Marine Snake. (From the "Fauna of British India.")

idea of catching anything. The sea was calm, and the darkness intense. Between 9 and 10 p.m. we caught six water snakes on hooks of large size, baited with salt pork, and resting quietly on the bottom. They were of about the same size, 3 feet long, ... and very savage, snapping at everything within their range." That their bite is very dangerous, in some cases even fatal to man, has been established by the experiments of Russell (14), Cantor (4), Fayrer (6), and others. The largest size attained by any of them is about twelve feet.

The home of the marine snakes, whatever their affinities, is in the Indian and Western South Pacific Oceans, being found in abundance from the Persian Gulf, along the coasts of India, Burma, and the Malay Archipelago to North Australia and New Caledonia. Some

species have a very wide distribution; *Hydrus bicolor* occurring throughout the Indian and tropical Pacific Oceans, and the extreme points from which it is on record being the Cape of Good Hope, Posiette Bay (Mantchuria), New Zealand, the West Coast of Mexico, and Guayaquil. Several species extend from the Persian Gulf to New Guinea and North Australia, or from the Bay of Bengal and the Chinese Sea to New Caledonia. One species is stated to be confined to fresh water—*Hydrophis semperi*, from Lake Taal, in Luzon (Garman, 8).

The Hydrophids being most abundant in the Arabian Gulf and the Bay of Bengal, it is in the works dealing with the Fauna of India that the greatest amount of information respecting them is to be found. Günther's "Reptiles of British India" (9), contains a complete account of the species known in 1864; Fayrer's (6) "Thanatophidia" gives effective coloured figures of several species; excellent figures appear in Jan's "Iconographie" (11); whilst a revision up to date of all the species known from the coasts of India, Ceylon, and Burma will be found in my volume of the "Fauna of India" (3). Boettger's notes on secondary sexual characters in *Hydrophis* (2) is another important recent addition to our knowledge of this most interesting group of Snakes.

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VII.

The Exploration of Coral Reefs by Borings.

AFTER the years of controversy upon the respective merits of the opposing theories of the origin of coral reefs, an effort is now being made to apply the test which is generally considered to be the only final one. Considering how much is involved by the coral question, with its bearings on the geological history of the whole Central Pacific area, on the permanence of oceans and continents, on zoological distribution, and on the relation of subsidence to volcanic action, it seems surprising that no attempt has previously been made to secure an adequate series of borings. An influential committee has, however, now been appointed by the British Association to arrange for this to be done. The subject having thus been brought within the range of practical science, one is led to consider the questions connected with the choice of a site that would be accepted by all parties as a good test case, and with the mechanical difficulties that have frustrated previous attempts to apply this method of investigation.

In the selection of a test locality, it is obvious that the Darwinians should have the main voice, for there are many cases where reefs occur on submerged banks and shoals, and where a boring would be an absolute waste of money. It is, therefore, for the believers in the subsidence theory to point out a reef where this chance is reduced to a minimum. Fortunately, there are plenty of data with which to do this. Darwin himself remarked that the smallness of many coral islands is an argument for subsidence, and Dana has clearly indicated the characters by which a sinking reef may be distinguished from one that is stationary. In the latter, the *débris* from the reef would accumulate as a beach around the island and thus increase its size; the slopes around the beach-line would be made steeper; storms would throw material into the lagoon and fill it up; and the island would tend to become well wooded. On the other hand, a sinking reef would decrease in size; the lagoon would rarely be more than partially filled up; the surface of the island would rise but slightly above the sea; and it would be largely tide-washed, and thus kept free from trees. Further, Dana has shown that the islands of these two classes are distributed on a definite plan, which affords, probably, the strongest argument for the subsidence theory.

To take one case: at the east end of the Paumotu Archipelago the coral islands are well-wooded, have broad reef grounds, and their lagoons are filled up; that is to say, they possess the characters of stationary reefs. Going westward, the islands are seen to decrease in size and elevation: the lagoons persist, and the islands become less and less wooded. At Bellingshausen Atoll the last stage is seen, and beyond this there is nothing but open sea for nearly 900 miles; then some low banks of tide-washed coral commence the Samoan Archipelago, in which the order is reversed, till it culminates in the great wooded coral island of Savaii. The inference is that the central open sea represents an area where subsidence was too rapid for the reefs to keep pace with it, and so they have been submerged, while the gradual increase of the reefs on either side marks the decrease in the rate of subsidence. If this deduction be correct, then, by the application of this method, one can map out the whole of the Central Pacific into subsiding or stationary districts. Broadly speaking, there are two main lines across the Pacific, along which the land is either rising or is at least stationary. The first runs as a continuation of the longer axis of New Zealand, north-eastward through the Kermadec and Tonga Islands, to the volcanoes of the Samoans, and thence past the Phœnix group and the Kingman Shoals to the Sandwich Islands in the extreme north-east. Along the whole of this line there occurs a chain of volcanoes, a submarine ridge, occasional shoals, and islands with raised reefs, and some plutonic rocks. The second line starts from the coast of South America, as the submerged plateau that extends for a great distance north-west of Patagonia, and runs thence through the Paumotu group, crossing the other line almost at a right angle at the Samoans, and thence it continues westward as the main chain of Malaysia. Parallel to these two lines, and on both sides of them, are areas of deep and open sea, which certainly seem to represent areas of subsidence. It is in one of these that the greatest expanse of radiolarian deposits occurs, stretching as a great elliptic sheet trending north-east and south-west; where this crosses the second line it is completely cut across by a band of a less abyssal globigerina ooze, as is well shown in the beautiful map of the distribution of the deep-sea deposits which accompanies the last volume of the "Challenger" Reports.

It is manifest that any boring on one of the stationary or rising lines would be quite valueless, as shallow reefs might occur anywhere along them. The islands that are most likely to yield a conclusive result are the low atolls on the very margins of the great coral archipelagos, such as Enderbury Island at the northern end of the Phœnix, Rose Atoll at the eastern end of the Samoans, or Bellingshausen Island at the western end of the Society group. But these atolls are so very inaccessible that some more convenient locality would naturally be preferred. Diego Garcia, for example, or any of those in the Indian Ocean, would be much more easily reached. But it is

not at all improbable that the Maldives and Laccadives occur along a submerged ridge that runs parallel to the line of the Western Ghauts, and the southernmost peak of which is marked by Diego Garcia. Hence the selection of this atoll might easily mean the failure of the experiment, for if the reef in this case should be found to be only a thin cap, it would simply prove the existence of this submerged chain, and not that the subsidence theory is unnecessary for the atolls of the Pacific. The Central Pacific is the typical coral reef area, and it is to be hoped that it may be possible to select this region for the experiment. Its proximity to the Australian mining fields is also in its favour, as machinery might easily be obtained thence.

The mechanical difficulties in the way of a deep boring are, however, especially serious on the small low atolls. The upper crust of the coral rock is sure to be more or less rotten and perforated by irregular cavities and channels. Hence, as soon as the work has been well started, the weight of the machinery and the vibration of the boring cylinder are not unlikely to cause a slight subsidence and the breaking of the tool. The attempt made by the Tuscaroa failed in this way. But the difficulties, though serious, do not appear to be insuperable, and if the site be selected with sufficient care, and a proper foundation be erected, there seems no reason why a 500 or a 1,000 foot core should not be obtained.

Finally, in regard to the nature of the core: to judge by many of the remarks made upon this question, some writers seem to expect that, if the subsidence theory be true, the whole core should be of solid coral. But an examination of the structure of raised reefs clearly shows that this cannot be expected. The extensive excavations in the Barbados reefs, made by the Barbados Water Co., has thrown much valuable light upon the varying composition of coral rocks. A large collection of these rocks has been presented to the British Museum by Mr. E. Easton, and this has been supplemented by donations from Professor Harrison and Mr. Jukes-Browne. Microscopic examination of the specimens in this considerable collection shows that the reefs consist mainly of coral-sand and *débris*, with beds of limestone formed by nullipores, foraminifera, mollusca, &c., as well as by corals. In many cases, too, there are masses of rock without any trace of organic structure, though they have been unquestionably formed from corals. The rock of which the atolls are composed is likely to show similar variations; and as the whole foundation of the reef is probably saturated with sea water, it is quite possible that, below a very limited depth, the rock will consist only of a jointed, crystalline limestone. But even if this be the case, microscopic examination will, no doubt, be able conclusively to demonstrate whether the rock was formed at the depth where it was found, or whether it was a shallow water deposit that has since subsided.

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VIII.

Some Recent Researches on Insects and Arachnids.

THE greater part of entomological literature consists of descriptions of new genera and species. To record the names of these would be foreign to the purpose of this Review, which will not endeavour to do the work of the "Zoological Record." "Species making" has often been condemned by morphologists as an unprofitable pursuit; and no doubt the naturalist whose highest ambition is to describe as many new forms as he possibly can, has fallen far short of the ideal which the true student of animal life should set before him. Yet the work of discriminating species is a necessary one; and the generalisations of the philosophic naturalist must largely depend on the technical labour of the systematist.

Only newly described forms which have an evident biological interest will be referred to in this notice. Our object will be to gather from the mass of current entomological literature those results which throw light on the history of the various insect groups. The latest discoveries in anatomy, development, habit, and adaptation to environment will, as far as possible, be recorded.

A contribution to Arachnid anatomy has lately been made by Dr. P. Bertkau ("Zool. Anzeiger," vol. xv., p. 10), who describes sense organs found on the upper side of the last joint of the palps and first pair of legs of two species of Solpugidea (*Solpuga flavescens*, C. L. Koch, and *Galeodes barbarus*, Luc). These organs are of two kinds, and resemble those already described as found on the antennæ of ants and other insects. On account of their shape, they have been styled "champagne-cork" and "flask-shaped" organs. They are in communication outwardly with small openings in the chitinous body-wall, and inwardly with ganglion cells and nerve-cords. Dr. Bertkau thinks it probable that their function is that of smell.

Some notes of interest have appeared in the English entomological journals on the effect of environment on the development of Lepidoptera. Mr. R. Adkin ("Entomologist," vol. xxv., p. 25) states that the wet and cold summer of last year caused a prolongation of the larval and pupal stages in several species over the normal periods. Mr. F. Merrifield ("Entom. Monthly Mag." [2], vol. iii., p. 20) gives the results of four years' experiments in rearing spring-

feeding caterpillars of *Selenia tetralunaria*, some on trees in the open air and others on cut food indoors. Those treated in the former way completed their development in the summer as usual; but the house-fed larvæ, though they pupated in good time, did not become moths until the following spring. Thus, a normally double-brooded species was made single-brooded by being fed indoors, whereas the high temperature of a house should have had just the opposite effect to this. Mr. Merrifield finds in the condition of the food the explanation of this anomaly. He believes that feeding on cut branches wanting moisture, and with little circulation of sap, must have a retarding effect on the development of the insects.

An interesting account of the way in which the caterpillar of *Acronycta alni* makes its cocoon is given by Mr. M. Fitzgibbon ("Entomologist," vol. xxv., p. 40). The habit of this larva is to select a rotten stick, which it proceeds to hollow out, in order to form a shelter wherein it may winter in its pupal stage. The noteworthy point of the description is the use of the caterpillar's strong bristles in sweeping away the sawdust formed by excavation. These bristles are flattened at the tip, and seem specially adapted for this function.

In the same paper the colouring of this caterpillar is discussed. In its early stages, it is supposed to be imitative of a bird-dropping, but after the last change of skin it becomes conspicuously adorned with warning-colours. Like many other larvæ, it hides from its enemies until it is too large to be hidden any longer, and then it tries to frighten them away.

An important contribution to Insect Embryology has been lately made by Dr. R. Heymons, of Berlin (" Zeitschr. für Wissensch. Zoologie," vol. liii., p. 434, &c.). This is a paper entitled " Die Entwicklung der weiblichen Geschlechtsorgane von *Phyllodromia* (*Blatta*) *germanica*, L.," which gives a very exhaustive account of the origin and formation of the ovaries and oviducts in that household pest known as the German Cockroach. This insect is a smaller and paler species than the Common Cockroach (*Periplaneta orientalis*), which it is said to be supplanting in many places in Britain, where it has succeeded in establishing itself. In his introduction, Dr. Heymons points out that technical difficulties have hitherto prevented a complete study of this subject. The eggs of the cockroach are enclosed in a chitinous capsule, and the yolk-mass contained in each egg is of a nature unfavourable for microscopic investigation of the early stages of development. These obstacles have now, however, been successfully overcome. The cockroach is an insect of a primitive and generalised type, and its development is not complicated by secondary metamorphic adaptations; hence it is specially valuable for embryological research.

The primitive germ-cells arise from the mesoderm, from the ordinary cells of which they may be distinguished by their larger size. They first appear towards the hinder end of the blastoderm, before the formation of the somites. They are also formed, however,

from the walls of the somites as these are developed, and, in the younger stages of the embryo, are always to be found in the second, third, fourth, and fifth abdominal segments. Thus, whether formed from the primitive layer, or from the cœlomic epithelium, the origin of the germ-cells is clearly mesodermic.

This result is contradictory to that lately obtained by Cholodkovsky, who believes that the germ-cells are probably formed from small yolk-cells. Dr. Heymons, however, has no doubt that the Russian observer fell into error owing to the imperfection of his method of preparation, especially as he was unable to distinguish the germ-cells before the stage at which the somites begin to form.

Certainly the mesodermal origin of the germ-cells seems by far the more probable of the two alternatives, and, after a review of the results obtained by workers in other insect groups, Dr. Heymons concludes that, among insects generally, though sometimes in a modified way, the mesoderm gives rise to the generative organs. In the origin and segmental arrangement of the germ-cells he sees a similarity to what is observed in the Annelids.

As development proceeds, the somites become largely filled up with the fat-body, and the germ-cells begin to take up a dorsal position. The differentiation of sex is now evident. Epithelial cells of mesodermal origin arrange themselves around and amongst the germ-cells. The epithelial cells ventral to the germ-cells form the oviduct, those dorsal to them arrange themselves in linear series to form the thread-like ends of the ovarian tubes, whilst others enclose the germ-cells and so form the ovarian tubes. Special attention is called by Dr. Heymons to the fact that, throughout the course of the development, the germ-cells are always quite distinct from the epithelium of the ovary.

During the larval stages, changes take place in the nuclei of the germ-cells, and they begin to assume the characters of ova.

The thread-like prolongations of the ovarian tubes appear to be of use only during the embryonic and larval periods, and their function is to enable the ovaries to change their position in the body of the insect. They are all continuous with a single thread of cells which stretches to the pericardial septum.

Dr. Heymons states that the contents of the ends of the ovarian tubes are ordinary epithelial cells and germ-cells. These portions of the tubes in different insects have been called "Endkammer" by various authors who have described a mass of undifferentiated cells as found in them. In *Phyllodromia*, however, the germ-cells are, as stated above, all along distinct from the epithelial cells.

Some statements have lately been made with regard to the effect produced by the parasitic Stylopidæ on their hosts, which are Bees, mostly of the genera *Andrena* and *Halictus*. Mr. R. C. L. Perkins ("Entom. Monthly Mag." [2], vol. iii., p. 1) gave as the result of his investigations on stylopised bees that the internal organs of the hosts

were hardly affected by the presence of the parasites, and that the males certainly, and the females probably, were still capable of reproduction. These results are extremely surprising, being in direct contradiction to those obtained by Shuckard, Westwood, and Perez. The last-named of these, a few years ago, gave a very full account of stylopised bees ("Actes Linn. Soc., Bordeaux," vol. xl., p. 21). Mr. F. V. Theobald, however, confirms, to a great extent, the work of these older observers ("Entom. Monthly Mag." [2], vol. iii., p. 40), and makes it likely that the specimens examined by Mr. Perkins were of a very exceptional nature. The *Stylops*, lying above the viscera, obtains its nourishment by osmosis through the abnormally thin walls of the alimentary canal, which, being forced down on the reproductive organs, renders them abortive. They do not, as a rule, contain ripe ova or spermatozoa. Indeed, Mr. Perkins admits that he could not find the former in any stylopised female.

The habits of another class of parasites are described by Herr C. Verhoeff ("Zool. Anzeiger," vol. xv., p. 41). These are bees of the genus *Stelis* which have long been known to lay their eggs in the nests of other bees *(Osmia)*. Former observers had noticed the eggs both of the parasite and of the host in the food-mass in the cell, and at a later period had found only the larva of the *Stelis*. Hence it had been inferred that the *Osmia* grub died of starvation. It seems, however, that its death is really due to violence. The *Stelis* lays her egg before the *Osmia*, and at the further end of the food-mass, whilst the *Osmia* lays her egg on the surface of the food-mass. Hence the two eggs are at opposite ends of the food; and both grubs feed for a time without conflict. Later, however, the *Stelis*, which, being older, is larger and stronger than the *Osmia*, attacks the latter, digs its mandibles into the head of its victim, kills, and devours it. The time occupied by the *Stelis* larva in eating up the *Osmia* is one to two days.

It seems doubtful if this habit is one to which the term parasitism can properly be applied.

G. H. Carpenter.

IX.

The Relationship of the Carboniferous Plants, Sigillaria and Stigmaria.

THE true nature of *Stigmaria* has long been a fruitful subject of discussion among palæophytologists. Brongniart, who appears to have first established and defined the genus, describes the specimens so named as stems bearing leaves, which ultimately disarticulated and left the surface marked by scars arranged in a quincuncial order.[1] He seems to have had some knowledge of the internal structure of *Stigmaria*, and on the strength of this and other features placed it, with some reservation, amongst the Lycopodiaceæ, pointing out that it had many points of analogy with the existing genus *Isoëtes*, and suggesting that it might be considered as a gigantic aquatic Lycopodiaceous plant, in fact, a sort of arborescent *Isoëtes*. The subsequent discovery by Binney and others of fossil *Sigillariæ* and *Lepidodendra*, whose trunks were continued downwards at the base into large, spreading, and bifurcating structures recognisable as *Stigmariæ*,[2] soon gave rise to another view of their morphological nature, viz., that they were in reality the roots of these trees, and not the stems of independent plants as had been generally supposed. From that time to the present, opinions have been divided as to which of these two views was the correct one, and as new facts have been brought to light, the advocates on either side have sought to interpret them in accordance with the opinions they hold. After carefully weighing the whole of the evidence available five years ago, Count Solms-Laubach came to the conclusion[3] that the best explanation of the known facts was to be found in the hypothesis that the *Stigmariæ* were rhizomes adapted to a soft pulpy environment, whose appendages, though probably foliar in their nature, were functionally organs of absorption.

On the assumption that this conclusion was correct, the further question presented itself as to how the aerial *Sigillaria* with its Stigmarian rhizomes was developed. Was it to be supposed that

[1] "Prodrome d'une Histoire des Végetaux fossiles," 1828.

[2] For details of these discoveries, &c.; see Williamson, *Stigmariæ ficoides*, *Palæontographical Society*, 1887; also Solms-Laubach, "Fossil Botany," Clarendon Press.

[3] "Fossil Botany," pp. 291–293.

the *Sigillaria* gave rise to the *Stigmaria*, or *vice versâ?* To this question, so far as I am aware, no satisfactory answer was given, and its solution was left over until further investigations should throw fresh light upon it.

Some such light appears to have been obtained by Professor Grand' Eury, who has recently written an elaborate memoir on the *Géologie et la Paléontologie du Bassin houiller du Gard*, in which the subject is dealt with. Unfortunately, this memoir will only be accessible to a very limited number of workers. Printed under the auspices of the colliery companies of the district dealt with, only 125 copies have been struck off, and these, it is understood, will not be obtainable by purchase, but disposed of in another way. M. Zeiller, however, in presenting, on behalf of the author, a copy of the memoir to the *Société géologique de France*, has given a careful and somewhat lengthy summary of the palæontological portion, and has thus ensured for the main results a much wider publicity than they would otherwise have obtained. So far as these bear upon the morphological character of *Stigmaria* and its relation to the aerial stem of *Sigillaria*, they are embodied in the following paragraphs, for the substance of which I am indebted to M. Zeiller's excellent summary.

In the first place, it may be noted that Professor Grand' Eury, like Count Solms-Laubach, has come to the conclusion that *Stigmariæ* are rhizomes which floated in water or spread themselves out on the surface of mud. In this condition they were able to live on indefinitely without giving rise to an aerial stem. But his observations have led him much further than this, and have enabled him to indicate, at least in a general way, the mode in which these creeping rhizomes gave origin to aerial stems. The first step in the process is the knotting up of the *Stigmaria*, so to speak, in order to form a large bulb. M. Zeiller does not state whether this bulb-formation takes place at the growing tip of *Stigmaria* or at some other point, or whether both cases are possible. However this may be, the connection is said to be of such a nature that the bulbs are united to the *Stigmaria* by a vascular axis. At first, the bulbs present four swellings at the base, but these afterwards elongate, and ultimately show the cruciate disposition so often recorded as characteristic of the bases of the stems of *Sigillaria*. At first, neither the upgrowing stem nor the root-like branches carry any appendicular organs, and consequently their surfaces are not marked with the usual characteristic scars. Thus the lower part of the bulb, in developing and branching, takes the form of *Stigmariopsis*, while at the same time the stem begins to raise itself vertically. At the base of the stems arising in this way, and which are often swollen into the shape of a bottle, M. Grand' Eury has observed neither true foliar scars nor any trace of a vascular bundle, but merely a number of glands arranged in pairs. Thus, the base of the aerial stem has the characters of the fossils known as *Syringodendron*, or rather those properly so-called, viz., those of the type of *Syr. alternans*, which

M. Grand' Eury now regards, not as decorticated *Sigillariæ*, but the part of the stem which stood in the water or mud, and was destitute of leaves. This part, according to circumstances, might remain short or attain a height of several metres. Higher up the trunk, the leaves made their appearance, but the first to be developed were very short, and were perhaps rather of the nature of scales.

In this account of the development of *Sigillaria* with its Stigmarian rhizomes, there are obviously many lacunæ, which may be due to the condensation necessary in M. Zeiller's paper. Nothing is said, for instance, of the fate of the *Stigmaria* when the bulb formed upon it has developed into a *Sigillaria*. This is certain to attract the attention of palæophytologists, and the more so since, in the stumps of *Sigillaria* described in the literature, no trace of such a structure is met with. The four primary rhizomes seen from below have merely a cruciate depression between them, and show no signs of having been attached to a parent *Stigmaria*. On the other hand, the absence of such signs is capable of an easy explanation, and appears to afford no substantial ground for rejecting the views put forward by M. Grand' Eury.

Subject, then, to the needful reservations, it will probably be conceded that the mode of development of *Sigillaria*, as outlined above, is not an unlikely one, and would be more or less consistent with what has long been known of the relations of *Sigillaria* and *Stigmaria*. It will be observed that the account furnishes a more definite conception than has hitherto been possible of the nature of the fossils known as *Stigmariopsis* and *Syringodendron*, and makes more precise the distinction between the former and *Stigmaria* proper. That distinction has not hitherto been received with much favour by palæophytologists generally, but M. Grand' Eury appears to be convinced of its reality. As this conclusion is based upon observations of wide extent—what are termed forests of fossil *Sigillariæ* having been opened out in the district investigated—it is, no doubt, entitled to, and will receive, the respectful attention of other authorities. Still, it may be pointed out that in several examples figured or described in the literature, there is no absence of the characteristic scars, even at the proximal ends of the four primary Stigmarian branches into which *Sigillaria* divides at the base. This being so, the possibility is suggested that the *Stigmariopsis* stage may not be a constant feature of the development of *Stigmaria*, but may be in some way dependent upon circumstances, as appears to be the case with the height of the Syringodendroid part of the Sigillarian stem.

THOMAS HICK.

X.

The Mammals of India.[1]

NO more important faunistic zoological work has ever been attempted than the "Fauna of British India," of which the present fasciculus concludes the volume devoted to the Mammals. The need for such a comprehensive work, as we are informed in the preface issued with the part before us, was strongly urged on the Government of India by a representative body of zoologists in a memorial signed in the autumn of the year 1881; and it reflects no little credit on those in authority at the India Office that the considerations urged in that memorial were so promptly answered. The whole work was placed under the able editorship of Mr. W. T. Blanford, whose reputation as a proficient in Indian zoology generally, and as a specialist in several branches thereof, is a sufficient guarantee that it could not have been confided to better hands. At first there was some doubt whether it would be found practicable to extend the work beyond the limits of the Vertebrates, but we are now glad to learn that such limits have not been imposed, and that the volumes descriptive of the Moths are now actually in hand. The promptitude with which the volumes allotted to the Vertebrates have been issued is worthy of all praise, six out of the whole seven being completed by the issue of this second fasciculus of the one devoted to the mammals. We thus have the Mammals, by the Editor; two volumes on Birds, by Mr. E. W. Oates; one volume on Reptiles, by Mr. G. A. Boulenger; and two on Fishes, by the late Surgeon-Major Day. The issue of a third volume on Birds will complete the Vertebrate series.

The first fasciculus of the volume on mammals, which appeared in 1887, contained the description of the various representatives of the orders Primates, Carnivora, and Insectivora; while in the present issue we have the orders Chiroptera, Rodentia, Ungulata, Cetacea, Sirenia, and Edentata, the two remaining groups being unrepresented in the Indian area. We are informed in the preface that the total number of species of mammals now recognised from British India, Ceylon, and Burma, as well as the adjacent seas, just exceeds 400. In Jerdon's "Mammals of India," published in 1867, the number was only

[1] The Fauna of British India, including Ceylon and Burma.—Mammalia. By W. T. Blanford, LL.D., F.R.S. Part II., 8vo; London, 1891; pp. i–xx, and 251–617; illustrated.

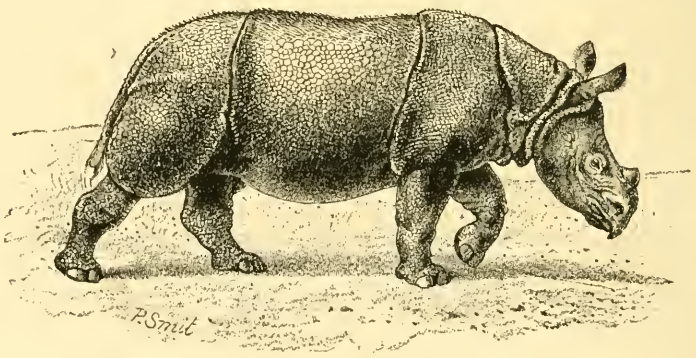
242; but then we must recollect that Jerdon's area was a much more limited one, excluding Ceylon and the regions north of the main axis of the Himalaya, as well as those to the westward of the Indus, and eastward of the Bay of Bengal. In Mr. Sterndale's volume on the same subject, issued in 1884, the number was, indeed, raised to upwards of 482; but some of the species there recorded do not occur within the limits of British India, while fuller observations have shown that others are not entitled to rank as distinct. The greatest additions to the number of valid species living within the strictly British Indian area since Jerdon's time occur, as might have been expected, among the so-called Micro-Mammalia, viz.: Bats, Insectivores, and Rodents; while, as might likewise have been predicated, the group of which our knowledge is least satisfactory is that of the Cetaceans. As regards the main scheme of mammalian classification, Mr. Blanford practically follows the one adopted by Professor Flower in his later works. We shall find, however, as we proceed, that some important changes have been made in the limits of genera upon those which have been adopted by several recent writers; while there are certain not less noteworthy emendations in the specific names applied to some of even the most well-known Indian mammals.

Not the least important feature of Mr. Blanford's work is the happy blending of strict technical description and detail with readable and interesting notices of the habits and mode of life of the various animals described. This ought to make the book as acceptable to the amateur lover of natural history and the sportsman as it must be indispensable to the professed zoologist. Since, moreover, the author has paid great attention to the dimensions and weights of the larger Indian mammals, we trust that, for the future, we shall hear no more of 13 ft. tigers, and other similar monsters, which frequently repel the scientific zoologist from studying sporting works with the attention that, in other respects, they may merit.

Another feature to which we are fain to draw special attention in the work before us is the excellence of the illustrations. With the exception of a certain number reproduced from other works, these have been executed by one of the numerous photographic processes now coming so largely into use; and their fidelity to details proclaims the superiority of this mode of illustration over the ordinary woodcut. Among the figures of the larger mammals which strike us as worthy of special commendation we may refer to those of the two species of Rhinoceros, to that of the Bharal, and to those of the horns and heads of the Markhor. Four of these, by the courtesy of the author, we are enabled to reproduce.

As the larger terrestrial animals are those which are likely to be the most interesting to the majority of our readers, it is to these that we shall mainly confine our remarks. Under the title of *Elephas maximus*, we fear that our old friend *Elephas indicus*—the Indian elephant—will scarcely be recognised; and we cannot help regretting

that the author has felt himself compelled to make this change in nomenclature, seeing that the name *E. indicus* was published by Linnæus in 1754, although in the "Syst. Nat." of 1766 both the Indian and African species were included under the common title of *E. maximus*. Apart, however, from such questions, we may venture to hope that the emphatic statement by Mr. Blanford that elephants are furnished with deciduous milk-tusks, which precede the large permanent tusks, will for the future spare us from sentences like the following, which we extract from Mr. Sanderson's "Thirteen Years among the Wild Beasts of India," where it is stated in reference to the occurrence of these milk-tusks, that "the error—as it undoubtedly is—has arisen through some *savant's* diagnosis of the elephant's dentition, based on analogy, or the confounding the teeth and the tusks, as the same word is used to denote either in several languages." With regard to the question of the height attained by the Indian elephant, Mr. Blanford observes that adult males do not, as a rule, exceed nine feet; he quotes, however, Mr. Sanderson as having measured one



The Javan Rhinoceros (*Rhinoceros sondaicus*).

individual of 10 feet 7½ inches, while the late Sir Victor Brooke is reported to have killed another of upwards of 11 feet in height. A mounted skeleton of an Indian elephant in the museum at Calcutta measures, however, as much as 11 feet 3 inches in height, and if correctly articulated would indicate a height of some 12 feet in the living condition; but Mr. Sanderson states that he found the thigh bone in that skeleton was scarcely longer than that of an elephant of less than 10 feet, so that further investigations are required before these extreme dimensions can be definitely accepted.

Passing on to the Perissodactyle Ungulates, we may note that Mr. Blanford regards all the wild asses of Asia as referable to a single species—*Equus hemionus*. Of the Rhinoceroses, Mr. Blanford figures two out of the three Indian species; and, as we have already mentioned, these figures—of which we reproduce one—may be cited as excellent examples of illustration by "process." Although the author mentions that the lower tusks of the rhinoceros are regarded

by some zoologists as corresponding to the canine teeth of other mammals, we regret to see that this view is not adopted in the text, since it appears to us that the evidence of certain fossil Ungulates renders it indisputably correct. Some interesting observations are given as to the habits and former distribution of the Great Indian Rhinoceros (*R. unicornis*) among which we observe that the time-honoured tradition of its enmity to the elephant is finally disposed of. In accordance with the views of most zoologists, Mr. Blanford regards the small Hairy Rhinoceros of Chittagong merely as a variety of the two-horned Sumatran species (*R. sumatrensis*).

The true Ruminants, or Pecora, from their numerical abundance, necessarily occupy a considerable portion of this part of the

The Bharal, or Blue Sheep (*Ovis nahura*).

volume. Among these, the five Indian species of wild cattle are included in the single genus *Bos*; a peculiar pale-coloured variety of the Buffalo (*Bos bubalus*) receiving a new name. The difficult question as to the real number of species of wild sheep in India and Central Asia receives new light from Mr. Blanford's study of the magnificent series of heads of these and other Ungulates recently presented to the British Museum by Mr. A. O. Hume. In the first place, the specific distinctness of the Tibetan Argali (*Ovis hodgsoni*) from the true Argali (*O. ammon*) of Siberia, is, at least, provisionally admitted. Next comes the mighty *Ovis poli*, in which Severtzoff's *O. karelini* is included, according to observations previously published by the author. The definite identification of the Urial of the Punjab—commonly known as *O. cycloceros*—with the Tibetan Sha (*O. vignei*) now appears, however, for the first time, although it

has already been hinted at by several writers. The Punjab Urial must, accordingly, be regarded merely as a local race of the Sha, modified by the low elevation and the hotter climate of the regions which it inhabits. We reproduce Mr. Blanford's excellent figure of the Bharal, or Blue Sheep of Tibet (*O. nahura*) which forms a connecting link between the more typical sheep and the goats.

In the latter group, Mr. Blanford's observations on the extraordinary range of variation presented by the horns of the different races of the Markhor (*Capra falconeri*) will be read with interest. In spite of the extreme difference in the horns of the two varieties of

A B

Head and Horns of the Kashmir (*A*) and the Suleman (*B*) varieties of the Markhor (*Capra falconeri*).

which the figures are reproduced, there appears to be no doubt but that they both belong to one and the same species, as there are intermediate forms, showing a complete transition from the one to the other through a third variety from Cabul. In separating the Himalayan Tahr and the so-called Nilgiri Ibex from *Capra*, as *Hemitragus*, we think Mr. Blanford is fully justified, since the differences between these animals and the true goats are very considerable; but we are not quite so fully assured as to the advisability of regarding the Goral as generically distinct from the Serows (*Nemorhædus*) although there can be no doubt that if that view be adopted the name *Cemas* is the proper title for the former.

The Antelopes and Gazelles calling for no special remark, we pass on to the consideration of the Cervidæ. Here we note that the author includes all the Indian species, with the exception of the

Muntjacs and the Musk-deer in the Linnæan genus *Cervus*. In connection with the Kashmir Stag (*Cervus cashmirianus*) some important observations are recorded on the other elaphine deer of Asia, which do not properly come within the scope of the work. Among these it is very interesting to note that the great Thian Shan deer (*C. eustephanus*) appears to be so closely allied to the American Wapiti that it is even a question whether it should be regarded as anything more than a variety of that species; thus affording another striking instance of the close relationship of the fauna of the northern regions of Europe and Asia to that of North America. For most of the Indian deer Mr. Blanford retains the specific names by which they are generally known, although in case of the Sambar he has felt compelled to substitute the title of *C. unicolor* for *C. aristotelis*. He further considers that the so-called Equine Deer (*C. equinus*) of the Malayan region is really inseparable from this species. Nothing calls for special notice in the pages devoted to the Chevrotains. In the Swine the author regards the Indian Wild Boar (*S. cristatus*) as entitled to be distinguished from its European congener on account of the more distinct mane on the neck, and the larger size of the last grinding tooth, more especially in the lower jaw. It is, however, suggested that the wild swine of Baluchistan will prove to belong to the European species.

In treating of the Cetaceans, the author mainly follows Professor Flower's arrangement, although, as a provisional measure, certain Indian whales are regarded as specifically distinct from the northern forms to which they are severally allied.

We regret that space does not permit of our making extracts from Mr. Blanford's admirably condensed accounts of the habits of some of the animals of which he treats in such a pleasant, but, at the same time, such an exact style. All our readers who are in any way interested in the subject of the volume before us will, however, be well advised if they consult the original. And, in conclusion, we can only say that the entire work is in every respect worthy of the high reputation of its author, both as a technical zoologist and as an observer of the habits of animals in the field.

R. Lydekker.

SOME NEW BOOKS.

THOMAS SOPWITH, M.A., F.R.S. With Excerpts from his Diary of Fifty-seven Years. By BENJAMIN WARD RICHARDSON, M.D., F.R.S., &c. 8vo. London Longmans & Co., 1891. 8vo. Pp. 400. Price 6s.

AS a worker in Applied Science, the name of Thomas Sopwith takes a distinguished position. Born in 1803 at Newcastle-upon-Tyne, and brought up as a land surveyor, he was in early life much occupied with surveys of mines and roads, in planning the construction of bridges and other architectural works. Indeed, in 1829 he was successful in competing with McAdam—then in the zenith of his fame—for the construction of a road in the North of England. In course of time he was engaged on many surveys for railways in different parts of the country, and in connection with these labours he was brought into contact with the Stephensons, with William Smith, Telford, and others. He made an important mineral survey of the Forest of Dean, and the large models which he constructed of this and other districts won him, in 1842, the honour of a Telford Medal at the Institute of Civil Engineers. Throughout the greater part of his life, Mr. Sopwith was more or less intimately connected with the mining district of Alston Moor, of which he published plans and geological sections, and in 1833 a small volume, in which the Physical features, Geology, and Antiquities were described, together with a particular account of the lead mines. From 1838-41 he acted as a Commissioner on behalf of the Crown in the Dean Forest, and from 1845 to 1866 he held the chief agency of Mr. Beaumont's lead mines in Northumberland and Durham, residing twelve years at Allenheads.

Throughout his career the careful record of facts was a constant aim with Mr. Sopwith. He advised the publication of accounts of railway sections, and, in conjunction with Dr. Buckland and Sir Charles Lemon, was instrumental in gaining the establishment of the Mining Record Office. He was naturally interested in the formation of the Museum of Economic [now "Practical"] Geology, and published in 1843 the first guide to the Museum. He also published "A Treatise on Isometrical Drawing, as applicable to Geological and Mining Plans," &c., a work which met at the time with marked success, and of which a second edition afterwards appeared.

To geologists, Sopwith's Geological Models—brought before the Geological Society in 1841—are familiar. Constructed of various kinds of wood, they served admirably to represent the nature of stratification, of outcrops, faults, and mineral veins. Figures of them were published by Lyell in his "Elements of Geology."

The character of the man is illustrated by the remarkable diary which he kept for the long period of fifty-seven years. This diary has furnished the chief materials for the biography. It contains records of all the leading events in Mr. Sopwith's life, of individuals he met, of occurrences that came under his observation, together with extracts

from MSS. and rare books, pedigrees, plans, and notes relating to antiquities, &c., and many statistics.

Among the records we find reminiscences of Dr. John Dalton, John Buddle, John Taylor, William and Robert Chambers, Dr. and Mrs. Somerville, Faraday, Babbage, Ruskin, Buckland, Murchison, John Phillips, Lord Armstrong, and others.

Mr. Sopwith visited Dr. Buckland in 1837, and notes that, "Dr. Buckland's house is one of those venerable fabrics which form the principal quadrangle of Christ's College. As soon as the old-fashioned door is opened, abundant evidence is presented that the residence is that of a zealous disciple of geology. A wide and spacious staircase has its floors, and even part of steps, covered with ammonites, fossil trees and bones, and various other geological fragments, and in the several apartments piles upon piles of books and papers are spread upon tables, chairs, sofas, bookstands, and no small portion on the floor itself."

Some account is given of a visit, in 1839, to Mr. Babbage, when Mr. Sopwith inspected the famous calculating machine: "After thirteen or fifteen years' labour, and an expenditure of twenty thousand pounds, the engine was suspended for lack of further funds five years ago, and it is yet uncertain whether it will be completed. I saw a portion of it which was placed in the drawing-room, and performed the operation of cubing eighteen in *thirteen seconds* by merely *grinding*, or rather by moving a handle backwards and forwards twice."

In October, 1841, Mr. Sopwith accompanied Dr. Buckland to North Wales, and in the following year to Northumberland, in search of rounded and furrowed rocks, the work of glaciers. A humorous memento of these explorations is an etching of the Professor—not mentioned in the volume—which was reproduced by Sir A. Geikie in his Life of Murchison, vol. i., p. 309. It represents Buckland equipped for an Arctic climate, and with bags and rolls of maps, standing on a surface scored by "prodigious glacial scratches," and alongside are two blocks, one "scratched by a glacier 33,333 years before the Creation," the other "scratched by a cart-wheel on Waterloo bridge the day before yesterday," the whole "scratched by T. Sopwith."

Dr. Richardson tells how he first met Mr. Sopwith, in 1856, at Hartwell House, the residence of Dr. John Lee, where from thirty to forty visitors, including many eminent in different branches of science, were at various intervals entertained. The acquaintance then made, ripened into a life-long friendship, and Dr. Richardson was ultimately asked by members of the family to prepare this biography.

The task of selecting materials from so copious a diary must have been a difficult one; the excerpts given are mostly those which have a popular interest.

Considering that Mr. Sopwith's life-work was connected with engineering, and especially with mining geology, more particulars of his scientific work would have been of value. We read, for instance, on p. 127, that "a very interesting geological survey about Newcastle, carried out by himself and Dr. William Smith, in connection with the new lines of railway then springing up, occupies a good space in the journal, and introduces us to Sir William Jackson Hooker," but no further record is given. Hence, we feel that with the ample material before him, the advice of someone especially interested in Sopwith's work would have been of service to the biographer.

H. B. W.

THE HORSE: A Study in Natural History. By W. H. FLOWER. London: Kegan Paul, Trench, Trübner & Co., 1891. 12mo. Pp. 196. Price 2s. 6d.

A WELCOME may be given to this volume—the second in the "Modern Science Series," edited by Sir J. Lubbock—as tending to popularise the idea that the animals of the present day can only be properly studied with the aid of the light thrown by their extinct allies; this intimate connection between living and extinct forms being in no case more marked than in that very specialised animal, the modern horse. Professor Flower divides his subject into four chapters; the first treating of the horse's place in nature, and its ancestors; the second of its existing relations; and the third and fourth of the structure of the horse itself.

The tracing of the ancestry of the horse from the five-toed *Phenacodus* of the lowest Eocene to the one-toed creatures with which we are familiar, is in the main admirable, although we notice some points where it is scarcely up to date. For instance, it is surely hardly fair to refer to a paper by Madame Pavlow published in 1887, and to ignore the work of Scott and Osborn in 1889. Had the latter been referred to, we should not have seen *Hyracotherium* appearing on one page and the identical *Eohippus* on another; neither should we have met with any uncertainty as to the rights of *Pachynolophus* and the identical *Orohippus* to represent a well-marked genus. We should not, moreover, have had matters complicated by any suggestion of the typical *Hyracotherium*, with its three-toed hind foot, being identical with the five-toed *Phenacodus*. Then, again, we miss any reference to the genera *Systemodon* and *Epihippus*, which should find a place in the series.

In the second chapter we have a good description, and excellent illustrations of the various existing species of Perissodactyle Ungulates; but it seems rather strange to find the extinct *Hipparion* included under the heading of the horse's existing relations. We might, too, have been surely spared, in a work of this nature, a discussion as to whether or no the name *Hipparion* ought to give place to *Hippotherium*, as this cannot much interest the general reader. Again, when discussing the slight importance of the presence of a lachrymal gland in the *Hipparion*, and its absence in the modern horse, in regard to the descent of the one from the other, it would surely have added force to the argument to mention that such a gland actually existed in an extinct horse from India, and also in another from South America.

The two chapters devoted to the structure of the horse are beyond praise, and especial interest will attach to the author's description of the so-called "false nostril," and still more to his discovery that the "ergot" of the horse's foot represents the foot-pads of less specialised mammals.

Space prevents our calling attention to other points of interest, but we may conclude our notice by saying that if the natural history of every animal were treated as exhaustively as is that of the horse in the work before us, naturalists would have but comparatively little to do in the future. R. L.

THE Royal Society has issued the first part of its new *Catalogue of Scientific Papers* for the years 1874–1883, inclusive. The volume comprises 1,016 pages, and extends to the name of Gissler. It is not complete, but the Council promises to deal with publications not hitherto catalogued in a supplementary volume.

Catalogue of the Specimens Illustrating the Osteology of Vertebrated Animals, Recent and Extinct, contained in the Museum of the Royal College of Surgeons of England. Part iii., Class Aves. By R. Bowdler Sharpe, LL.D. Pp. lvii., 468. London: Taylor and Francis, 1891.

This new volume of the Osteological Catalogue of the Royal College of Surgeons is arranged upon the same plan as the previous volume by Professor Flower relating to the Osteology of the Mammalia. An important innovation, however, is the addition of a series of good figures of skulls, feet, and other anatomical points of value in the systematic arrangement of the class to which the work refers. With slight modifications, Dr. Sharpe adopts Mr. Henry Seebohm's classification of birds as propounded in 1889, and he acknowledges his indebtedness to Dr. R. W. Shufeldt during the passing of the sheets through the Press. Apart from its value as an index to the collection of which it treats, the volume will form a most useful work of reference for ornithologists, who too frequently ignore the osteological characters of birds in their systematic writings.

Geologie von Bayern. By Dr. K. Wilhelm von Gümbel. Vol. ii., Part i. Pp. 176. Kassel: Theodor Fischer, 1892.

The second volume of Dr. von Gümbel's Geology of Bavaria is accurately described by its title. The first volume of the work, it will be remembered, forms a general treatise on Physical and Stratigraphical Geology. After a general sketch of the formations represented in the country, the various districts are now to be treated separately in detail, and the text will be illustrated by many good sections and views of scenery. The description of the Bavarian Alps in the first part now before us leads to the discussion of many facts of great interest.

Annals of British Geology, 1890. A Critical Digest of the Publications and Account of Papers read during the Year—with Personal Items. By J. F. Blake, M.A., F.G.S. 8vo. Pp. x., 352. London: Dulau & Co., 1891. Price, 5s. net.

This most useful work is decidedly an advance in the right direction, and Professor Blake's wide experience in many departments of Geological and Palæontological research makes him specially fitted to inaugurate the new era in "Records" that we hope will dawn with it. As the Professor remarks, the annual summaries of progress in Science would be of much greater value if they could be made readable; and, so far as British Geology is concerned, the present volume amply realises the author's ideal. Each work, or paper, is noticed separately, while the entries are numbered and classified by subjects. Professor Blake's own comments and criticisms are placed in square brackets, to distinguish them from the abstracts they help to enliven. Some of these interpolations are likely to arouse—indeed, have already aroused—the scientific wrath of the authors reviewed; but many are extremely good, and will prove an incentive in some cases to more careful work. Professor Blake's "Annals" ought to find a place in every working geologist's library, and we hope the reception of the volume for 1890 will justify the continuance of the publication.

BRITISH EDIBLE FUNGI: How to Distinguish and how to Cook them. With coloured plates of upwards of forty species. By M. C. Cooke, M.A., LL.D. Pp. 237. London: Kegan Paul, Trench, Trübner & Co., 1891. Price 6s. net.

"FUNGUS-EATING is on the increase, thanks to field clubs and their fungus forays; but the complaint has been heard for many years that no efficient handbook for the guidance of young or inexperienced mycophagists could be found in the English language." With this apology for the present work, Dr. Cooke, as one of the oldest fungus-eaters, yielding at last to the importunities of his fungus-eating friends, proceeds to give the results of his experience. In this he does not confine his remarks to a mere description of the different species, their habitat, and season, but adds copious notes on their preparation for the table.

A preliminary chapter is devoted to proving that fungus-eating is "ancient in its origin, and of wide geographical distribution." Martial says Boleti were so prized by the Romans that it was unsafe to send them by a messenger, though one might trust him with silver or gold; because, explains Dr. Cooke, he would eat them by the way. Dr. Curtis enumerates "130 excellent species" in the United States. In India "natives eat fungi promiscuously, chopping up the different species together, without any ill effects." In the Malay Peninsula, China, and Japan, dried fungi are an article of commerce, and largely used in soups. Lastly, in Tierra del Fuego they form for several months the staple food of the country. Moreover, we must not regard the many edible species as a mere substitute for Mushrooms, but as having an endless variety in flavour.

Chapter II. is "explanatory," containing a few general hints. The author can give us "no rule which shall be of universal application in the discrimination of dangerous fungi. The only safeguard is to become acquainted, by means of well-defined features, with some of the best of the esculent species," and to avoid rash experiments in doubtful cases. "There is no difficulty in recognising all the best kinds by means of ordinary intelligence and care." "Good Fungi," we are told, "have usually a pleasant mushroomy odour, a smell of new meal, a faint scent resembling anise, or no particular odour at all." A fragment of the freshly-gathered fungus should have "an agreeable nutty flavour, with no acridity, sharpness, or tingling upon the tongue." Furthermore, it is suspicious "if a fungus, when broken, cut, or bruised, speedily turns of a deep blue or greenish colour." Avoid, therefore, all fungi with a disagreeable odour, a pungency of flavour, and a tendency to become blue when bruised. A species with salmon-coloured spores is rarely worth eating, and may be poisonous. When we have collected our fungi we must "cook without delay, and remember that there is as much art in the cooking as in the selection."

The next 182 pages are devoted to a description of the various species which Dr. Cooke has either eaten himself, or knows or believes to be eaten "without inconvenience" on the continent or in America. We are told when and where to look for each species, and how best to prepare it for the table. The description is written in language as simple as the circumstances admit, and the characters given are all external, so that with the help of the plates the amateur fungus-hunter should, after a little practice, be able to recognise at any rate the most striking examples. There are twelve plates, including figures of forty-four species, but, unfortunately, they are scattered through the book without any relation to the descriptions,

and no reference is made to them in the text. The figures on each plate are only indicated by numbers, and we must refer to the list at the beginning of the book to find whether a species is figured at all, and then search through the book for the particular plate. No description of the figures is given, and it is not even stated whether they are natural size drawings. In some cases, sections, or parts of sections, through the cap are included without any explanation, and these are not in every case all that might be desired, as, for instance, that of the Common Mushroom on Plate 6, Fig. 19. The work is evidently meant to be a popular handbook, and as plates appeal most to the popular mind as a means of identification, we cannot but regret these defects in their arrangement and execution.

At the end of the book there is a list of nearly two hundred edible fungi. About seventy are marked with an asterisk, indicating that Dr. Cooke has himself eaten them. One cannot help thinking that the list is too long, for there must surely be a wide difference between edible fungi which may be eaten "without inconvenience," and those which would form a welcome addition to a meal. Thus, *Lentinus tigrinus* and two species of *Panus* are characterised as "tough when old, and never very delicate or digestible," and we might cite similar or even more doubtful cases. Dr. Cooke's object is evidently to make converts, and we venture to think he would do this more effectually by merely indicating those species which may not only be eaten "without inconvenience," but also whet the appetite, and induce those partaking to go further and try others.

The Tenth Annual Report of the United States Geological Survey for 1888-9, in two parts, dated 1890, has just been received in England. It contains the Report of the Director, Major J. W. Powell, with plates illustrating Standards of Colours for Geological Cartography. The accompanying papers include:—(1) A General Account of the Freshwater Morasses of the United States, with a Description of the Dismal Swamp District of Virginia and North Carolina, by N. S. Shaler; (2) The Penokee Iron-bearing Series of Michigan and Wisconsin, by R. D. Irving and C. R. Van Hise; (3) The Fauna of the Lower Cambrian or Olenellus Zone, by C. D. Walcott; and (4) The First Annual Report of the Irrigation Survey, by the Director. The *Bulletins* of the same Survey issued during 1891 comprise:—(1) The Texan Permian and its Mesozoic Types of Fossils, by C. A. White; (2) Correlation Papers—Devonian and Carboniferous, by H. S. Williams; (3) Correlation Papers—Cambrian, by C. D. Walcott; (4) A late Volcanic Eruption in Northern California and its peculiar Lava, by J. S. Diller; (5) The Minerals of North Carolina, by F. A. Genth; and some good Bibliographies.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE University College of Wales, Aberystwyth, is soliciting subscriptions to provide a suitable and permanent Hall of Residence for the women students of the College. There are at present 40 women in residence, temporarily provided for in lodging-houses superintended by the LadyPrincipal.

The second Semester of the new Leland Stanford Junior University, Palo Alto, California, began on February 2. During the first Semester there were 490 students, 110 being women, and no less than 329 being residents in the State of California. The Engineering classes seem to have been most successful, while the classes for Natural Science were very small, except in Physiology. The President of the University is the well-known zoologist, Prof. David S. Jordan.

Mr. J. J. Lister, M.A., has been appointed Demonstrator in Animal Morphology in the University of Cambridge, in succession to Mr. S. F. Harmer, M.A., who is now Superintendent of the Museum of Zoology.

The official opening of the "Laboratory for Lacustrine Biology," founded by Dr. Otto Zacharias at Plön, between Kiel and Lubeck, is announced for April 1st. We hope next month to give some account of the Institution.

Subscriptions towards several memorials are being solicited. At Cambridge, a portrait of the Professor of Physiology, Dr. Michael Foster, Sec. R.S., is to be presented either to Trinity College, or to the University, by a committee of which Dr. Lea (Gonville and Caius College) is Secretary. At Cambridge, also, the Professor of Geology, Mr. McKenny Hughes, F.R.S., is collecting a fund for the benefit of the widow of his late assistant, Mr. Thomas Roberts, F.G.S. The Royal College of Science, London, will shortly decide upon the form of a memorial to the late Sir Warington W. Smyth, F.R.S., for which nearly £300 has already been subscribed. At Breslau, geologists will honour the memory of the late Professor Ferdinand von Roemer by placing a marble bust in the Geological Institute of the University.

According to the *Geological Magazine*, the Earl of Ducie has just erected at Churchill, Oxfordshire, an appropriate roughly-hewn monument of Oolitic ragstone to the memory of William Smith, the "Father of English Geology," who was born at that village in 1769.

We have often heard from specialists expressions of disapproval of the meagre facilities afforded them for the use of the great collections in the Museum of Comparative Zoology at Harvard College, Cambridge, Mass. Both on personal visits and in correspondence we, too, have met with some disappointment. We cannot, therefore, do better than quote from Dr. Alexander Agassiz's latest Annual Report (1890-91), which shows how the Curator himself appreciates these difficulties,

and is desirous of effecting a change that shall be beneficial both to the course of University teaching and to the progress of Natural Science:

"We are unfortunately compelled for want of funds gradually to restrict the "work of the assistants of the Museum to the mere care and maintenance of the "collections, and to limit their use by specialists. Our inability to engage a "sufficient number of assistants compels us to refuse many of the constant demands "made upon the administration of the Museum for an opportunity of working up or "of examining parts of our collections, the value of which to science is thus greatly "diminished. It is true we have a number of volunteer assistants, but we cannot "expect these gentlemen, who have kindly undertaken the general supervision of "special departments, to spend their time in the drudgery necessary to meet the "legitimate demands which every fairly organised Museum ought to be able to "meet, and which are naturally made upon our collections and assistants. With "the exception of Dr. Hagen and Mr. Garman, there is no assistant upon whom I "feel at liberty to call for work of that kind. The annual expenditure of a com-"paratively moderate sum would go far to remedy the unfortunate attitude we are "compelled to assume towards specialists. But I need not dwell upon the unsatis-"factory condition of the Museum finances. As I have stated in former Reports, it "is no longer practical for me, in connection with other plans I have in view, to "continue to give to the Museum the support, or devote to its interests the time it "has hitherto received from me. It is becoming self-evident that an attempt to "establish at Cambridge a Museum devoted to original investigation, in addition to "its other functions of providing a University Museum and Laboratories, is not "destined to be carried out. We cannot compete with the general government or "municipal institutions without an endowment or resources far greater than any "University can hope to obtain, and the sooner the future plans of the Museum are "modified to meet the existing state of things, the less will be the waste, and the "sooner shall we adapt our organisation to the comparatively limited field of a "University establishment."

Notwithstanding all difficulties, the Cambridge Museum Bulletin still continues to hold its important position among American scientific journals; and the exhibition rooms are being enriched with many valuable additions. A magnificent series of skeletons of Edentata from the South American Pampas, collected and mounted by Professor H. A. Ward, was added last year.

It is satisfactory to learn that negotiations are in progress for the acquirement of the Bristol Museum by the city. Subscriptions to the Museum and Library have of recent years decreased so much as to prevent the committee from making adequate provision for the custody of the Natural History Collection. Some departments of the Museum—notably those of Geology and Palæontology—are so rich in unique specimens that all interested in scientific work will unite in good wishes for the success of the new scheme. That the efforts of the Curator, Mr. Edward Wilson, are appreciated by ordinary visitors, is indicated by the fact that no less than 3,000 copies of his "Guide to the Bristol Museum," issued in September, 1890, were sold within fourteen months. A second edition of this admirable little handbook has just reached us.

We are glad to state that during the political strife of the past year in Chili, the French residents in that country have succeeded in founding a Scientific Society which promises to contribute largely to our knowledge of the Natural History of South America. The new organisation is entitled the *Société Scientifique du Chili*, and its General Secretary is Professor Fernand Lataste, the well-known French zoologist, who has resided in Chili for the last three years. The membership of the Society is composed of persons of every nationality residing in the Republic, and the contributions are presented either in French or Spanish, at the discretion of the respective authors. The Society will shortly issue the first part of its *Actes*, with an appeal for support to the principal Scientific Societies of Europe and North America. We have been favoured with an advance copy of the *Procès-Verbaux* of November 16th, 1891, in which M. Lataste refers to his work on the dentition of the Mammalia, and to a novel method of capturing Petrels. M. P. Germain communicated to the same meeting some observations on the habits of monkeys, and the President, M. A. Obrecht, referred to the lunar eclipse of November 15, as also

to an earthquake of the following day. The address of the new Society is at Santiago, Chili, and our fellow-naturalists in the Republic have our best wishes for success.

We understand that a Committee, with Dr. Josua Lindahl as chairman, is organising another International Geological Congress to meet in Chicago during the Columbian Exhibition in 1893.

The Proceedings of the Dorset Natural History and Antiquarian Field Club for the session 1890-91 were published in January, forming vol. xii. of the series. In addition to Mr. Mansel-Pleydell's presidential address, there are illustrated papers on the stone implements, &c., in the Dorset County Museum, by H. J. Moule, M.A.; on new and rare spiders found in 1889-90, by Rev. O. Pickard-Cambridge, F.R.S.; on the occurrence of *Tinea subtilella* at Portland, by Nelson M. Richardson, B A.; and on ancient British urns, by Dr. Wake Smart. Notes on some of the rarer forms of *Rubus* lately found in Dorset, are contributed by Rev. R. P. Murray, M.A.; and there is a new list of the land and freshwater mollusca, by C. O. P. Cambridge. A short paper on the Portland Stone Quarries, by A. M. Wallis, gives much detailed information. The volume concludes with a tabulated statement of meteorological and phenological observations, by the hon. secretary, Morton G. Stuart, M.A.

The members of the Dorset Field Club are regretting the retirement of their president, Mr. J. C. Mansel-Pleydell, F.L.S., who is, unfortunately, prevented by ill-health and advancing years from taking so active a part in scientific work as formerly. Mr. Mansel-Pleydell has been a central figure among the naturalists of Dorsetshire for the last 25 years, and has presided over the Field Club since its foundation in 1875.

In his Presidential Address to the Entomological Society of London on January 27, Mr. F. Du Cane Godman, F.R.S., referred to the difficulty in obtaining a sufficient number of specialists to deal with the enormously increasing lists of genera and species of insects. The number of forms recognised by entomologists is already apalling, and if satisfactory progress is to be made in the future, there must be a further division of labour among the systematists.

At its annual meeting on February 5, the Geologists' Association of London adopted a new procedure in electing a lady, Miss C. A. Raisin, B.Sc., to the office of vice-president. Miss Raisin, a well-known petrologist, had already been a member of Council for three years. Professor J. F. Blake was re-elected president, and delivered his first annual address. Choosing for his subject "The Evolution and Classification of the Cephalopoda," Professor Blake discussed the changes that had taken place in the study of this group of fossils during the last twenty-five years, and pointed out the wide-reaching significance of these changes. The legitimate outcome of the progress would be, the abolition of Linnean nomenclature in the study of successive races of extinct organisms: they would be arranged, instead, in "mutations" and "lineages."

At the annual meeting of the Geological Society of London on February 19, Mr. W. H. Hudleston, M.A., F.R.S., was elected president for the ensuing year.

To all who are interested in the progress of Technical Education, we heartily commend Professor Ayrton's Presidential Address to the Institute of Electrical Engineers, reported in *Nature* for February 4. Professor Ayrton gave an interesting sketch of the growth of technical education in this country, showing how much has been done, but how far we still are behind Germany and the United States. His address contained, moreover, many valuable warnings as to the dangers ahead. We wish that all those who are responsible for the future development and working of the new institutes would take them to heart, as well as gain fresh stimulus from the enthusiasm that animated the whole address.

At a meeting of the Royal Irish Academy on February 8, Dr. J. K. Ingram was elected to the Presidency, vacant by the death of the late Bishop of Down.

OBITUARY.

JEAN LOUIS ARMAND DE QUATREFAGES.

BORN FEBRUARY 10, 1810—DIED JANUARY 12, 1892.

BY the recent death of Professor De Quatrefages, the scientific literature of France has lost a contributor whose graceful pen had been active for more than half a century. Jean Louis Armand de Quatrefages de Bréau, of Protestant descent, was born on February 10, 1810, at Berthezème, near Vallerangue, in the Dep. of the Gard. At the University of Strasbourg he took a double degree, graduating as a doctor both of science and of medicine. Physical science seems to have attracted his early attention, as one of his theses, written when only nineteen, was entitled "Théorie d'un Coup de Canon," and the following year he published an essay, "Sur les aérolithes." Among his early writings on natural history was one on the Anodonts, which attracted attention, and probably led to his appointment, shortly after its publication, as professor of Zoology at Toulouse. Dissatisfied, however, with provincial life, he resigned this appointment, after a tenure of only two years, and migrated to the capital, where he was befriended by Milne-Edwards.

In 1842, De Quatrefages commenced a series of scientific expeditions to the Mediterranean and the Adriatic, which he described in a number of articles contributed to the *Revue des Deux Mondes*. These essays were afterwards collected in two volumes, which were published in 1851 under the title of "Souvenirs d'un Naturaliste." An English translation, by E. C. Otté, appeared in 1857, as "The Rambles of a Naturalist on the Coasts of France, Spain, and Sicily."

At length a professorship in the metropolis rewarded the active naturalist. In 1850, he was appointed to the chair of Natural History at the Lycée Napoléon, and five years later he succeeded Serres as professor of Anthropology and Ethnology, at the Musée d'Histoire Naturelle. Although the Science of Man henceforth became his favourite study, he continued for some years to carry on his work on the invertebrata: witness his "Histoire naturelle des Annelés marins et d'eau douce," which appeared in 1865. The diseases of the silkworm also engaged his attention, and as early as 1859 he gave to the world his "Etudes sur les maladies actuelles du Ver à soie," followed the next year by his "Nouvelles Récherches" on the same subject. He was likewise the author of an "Essai sur l'histoire de Sericulture."

Of his purely anthropological works, special mention should be made of the *Crania Ethnica*, in the preparation of which he secured the co-operation of Dr. E. T. Hamy, the keeper of the ethnographical collections in the Trocadéro. It was also in conjunction with Hamy that he projected a great series of works, *Les Races Humaines*, to which he contributed the introductory volume in 1887, when he was 77 years of age. Ten years earlier he had written a popular work, *L'Espèce Humaine*, of which an English translation appeared in 1879, as a volume of the "International Scientific Series."

As an anthropologist he conceded a very high antiquity to the human species, and, like so many other Frenchmen, accepted with little hesitation the evidence on which it has been sought to establish the existence of "Miocene Man." On the other hand, he adhered to a rigid monogenism, after the old controversy had lost to many naturalists its former interest. It was a prominent feature in his teaching that the differentiation of the several races of mankind from a single species had been brought about by the varying influences of the environment. In assigning to Man his systematic zoological position, he held that too much weight had been given to his structural resemblances to the lower animals, and too little to his psychological differences; hence, he held to the last that Man must constitute by himself a distinct kingdom—the *Règne Humain*.

In 1852, Professor De Quatrefages was elected a member of the French Academy of Sciences, replacing Savigny in the section of zoology. In 1863 he was made an officer of the Legion of Honour. But the highest recognition of his merit was reserved until 1879, when he was elected a Foreign Member of the Royal Society. At the second meeting of the *Association Française pour l'Avancement des Sciences*, held at Lyon in 1873, De Quatrefages presided, and delivered an introductory discourse admirably suited to a time when his country was emerging from the shadow of its great misfortune. "Dans ce Siècle de la Science," said the President, "une nation ne saurait être grande et forte qu'en s'inspirant du génie du temps, en s'imprégnant de l'esprit scientifique." F. W. R.

THOMAS ROBERTS.

Born 1856—Died January 24, 1892.

GEOLOGICAL science has suffered a severe loss by the death, at the early age of 35, of Mr. Thomas Roberts, M.A., F.G.S. He was educated at the University College, Aberystwyth, and there his attention was attracted to Geology by Mr. F. W. Rudler, who, at the time, occupied one of the professorial chairs. After a successful career at this Welsh college, Mr. Roberts proceeded, in 1879, to Cambridge, where he entered St. John's College. He took his B.A. degree in 1882, being placed in the first class in the Natural Science

Tripos. In 1883, he succeeded the late E. B. Tawney as assistant to Professor Hughes, the Woodwardian Professor, and gave many lectures and demonstrations in Palæontology. In 1886, he gained the Sedgwick Prize for an essay on "The Jurassic Rocks of the Neighbourhood of Cambridge"—a work which, unfortunately, has not been published. To the Jurassic rocks he had devoted especial attention, having, in the summer of 1884, been sent out by the University of Cambridge, with a grant from the Worts Fund, to study the Jurassic rocks of the Jura. The results of this journey, so far as regards the correlation of the Upper Jurassic Rocks of the Swiss Jura with those of England, were communicated to the Geological Society of London, and published in 1887. Another important paper by Mr. Roberts, on the equivalents of the Corallian beds in Lincolnshire, was published by the same society in 1889.

In conjunction with Mr. J. E. Marr, in 1885, Mr. Roberts had described the Lower Palæozoic rocks of the neighbourhood of Haverfordwest, and he had also written some papers on special Palæontological subjects, describing a species of *Conoceras*, and some abnormal Cretaceous Echinoids. He died on January 24, after an attack of influenza, and in consequence of failure of the heart. He was buried at Tenby, his old home.

JAMES AUGUSTUS GRANT.

Born April 11, 1827—Died February 11, 1892.

BY the death, on February 11th, of Col. J. A. Grant, the well-known East African explorer, we seem almost entirely severed from what may be called the Livingstone epoch in the opening out of the Dark Continent. Col. Grant was born at Nairn in 1827, and, after serving with distinction in India, both at the sieges of Multan and during the Mutiny, entered, in 1860, that field of African exploration with which his name will ever be associated. In that year he joined the expedition led by Speke to explore the Victoria Nyanza, which had then been but recently discovered by the latter in his previous expedition with Burton. The two travellers, starting from Zanzibar, journeyed to the north-west side of the lake, and after sojourning in Uganda with the now celebrated late King Mtesa, had the satisfaction of verifying the prediction of the older explorer that the main source of the Nile would prove to be the northern side of the Victoria Nyanza. It was not, however, to be supposed that a mere preliminary reconnaissance, like that which Speke and Grant were enabled to make, could give us an accurate map of the lake, and it is, therefore, no discredit to them that this task ultimately fell to the lot of Stanley. The prolonged absence of the two explorers led to the dispatch of a relief expedition, under Sir Samuel Baker, who met them, in February, 1863, at Gondoroko, on the upper waters

of the Bahr-el-Jebel, the main affluent of the White Nile. For these services to geographical science Col. Grant received the gold medal of the Royal Geographical Society and a C.B.-ship, his C.S.I. being subsequently earned in the Abyssinian expedition of 1868. In biological science Col. Grant will be long remembered by his important memoir on the botany of his African expedition, published in the *Transactions of the Linnean Society*, while his memory will also be kept green in men's minds by that most beautiful of all African gazelles, which the late Sir Victor Brooke so appropriately named *Gazella granti*.

WILHELM JUNKER.

Born April 14, 1840—Died February 14, 1892.

ALMOST immediately after that of Col. Grant, the death of another eminent African explorer is announced. At the comparatively early age of 52, Dr. Wilhelm Junker died on the 14th ult. at St. Petersburg. Born at Moscow in 1840, Dr. Junker left his native country at an early age to study in the Universities of Göttingen, Berlin, and Prague, and his first African journeys were in Tunis and Lower Egypt. In 1876 he visited the Soudan, and in 1879 advanced further into the interior of Africa, where he was hindered by the insurrection of the Mahdi, and prevented from returning until 1886. Vols. II. and III. of Dr. Junker's work descriptive of these later travels have already been published in Vienna.

HENRY WALTER BATES.

Born February 8, 1825—Died February 16, 1892.

ANOTHER pioneer in the investigation of the Natural History of the Tropics has just passed away in the person of Mr. H. W. Bates, F.R.S., the esteemed Assistant Secretary of the Royal Geographical Society. Born at Leicester in 1825, Mr. Bates was educated for a mercantile career ; but his mind was from the first imbued with a love of science that led him to devote all his leisure to the practical study of the Natural History of the country round his native town. Eventually, at the age of 23, he decided to relinquish business for purely scientific pursuits ; and in 1848 he left England with Mr. Alfred Russel Wallace, for the exploration of the Amazons. For four years in the company of Mr. Wallace, and for seven years subsequently alone, Mr. Bates was occupied in studying the people, geography, and natural history of that region of South America. He not only made large collections, which were forwarded to the British Museum, and other European museums, but also studied Tropical Nature with the closest attention, and obtained results of the highest philosophical importance. To Entomology, especially, Mr. Bates

devoted his energies, and it was he who first recognised the principle of mimicry in the animal world. On the first publication of Darwin's Theory of Natural Selection, Mr. Bates became a firm adherent of the new school of Biology, and he regarded the doctrine as the only satisfactory explanation of the phenomena of the world of life. Though he returned to England in 1859, Mr. Bates did not complete his classic work, "The Naturalist on the Amazons," until nearly four years later; and his only book of later years was his "Central America, the West Indies, and South America," contributed to Stanford's Compendium of Geography, in 1878. He wrote part of the section on Coleoptera in the "Biologia Centrali-Americana," and in the Royal Society's List of Scientific Papers he is credited with nearly 80 contributions on entomological subjects to various serials and publications of societies. In proportion, however, to his wide knowledge, Mr. Bates wrote little; and in matters relating to the broad questions of Natural History, he was ever ready with brilliant suggestions for anyone who might consult him. As Assistant Secretary of the Royal Geographical Society for a period of 27 years, Mr. Bates was placed in a congenial sphere of influence, that enabled him to make the best use of his philosophic learning and long experience; and British travellers of the present generation will feel that in him they have lost one of their best friends and most valued counsellors.

THOMAS STERRY HUNT.

Born September 5, 1825—Died February 12, 1892.

AFTER a painful illness, which had long incapacitated him from active work, Dr. T. Sterry Hunt, F.R.S., passed away, at New York, on the 12th of February, in his 67th year. Though most of his scientific work was done in Canada, he was a citizen of the United States by birth, having been born in the picturesque little town of Norwich, in Connecticut. He received most of his early education at Hartford in the same State, but obtained his first scientific appointment in New Haven, where, at the age of 20, he became the assistant to Professor Silliman, in the Chemical Laboratory of Yale College. He, however, only remained here two years, being appointed in 1847 Chemist and Mineralogist to the Canadian Geological Survey. In this country he spent nearly the whole of the remainder of his life, for though he resigned his Survey office in 1872 in favour of the Chair of Geology in the Technological Institute of Massachusetts, he soon returned to Montreal. His first scientific publication, containing a notice of the titaniferous minerals, was issued in 1846, and for many years afterwards he poured forth short papers upon various branches of Mineralogy and Chemistry. His work was noted rather for daring originality than for accuracy of thought or observation. He belonged

to the early school of Canadian geologists, whose theories upon rock metamorphism, the deposition of the Laurentian rocks, and the nature of crystalline schists once exercised a wide-spread influence both in Europe and America. He was one of the first describers of Eozoon, and wrote many papers to explain away the chemical anomalies which the acceptance of its organic origin involved. His best known works were his Chemical and Geological Essays (1875 and 1879), of which a second series was issued in 1886, under the title "Mineral Physiology and Physiography." These give a good summary of his speculations upon the primæval chemistry of the earth, the cause of volcanic action, the origin of the crystalline schists, and others of the most vexed questions of geology.

On the 15th of December last died, at Amboise, Louis François Héron-Royer at the age of 56. Devoid of education, and earning his living as a mere artisan, Héron-Royer nevertheless succeeded in making himself a name by his investigations on the habits and development of the European tailless Batrachians, a subject at which he worked with great enthusiasm from 1877 to the time of his death. His numerous papers were mostly published by the Société Zoologique de France (of which he acted as treasurer for several years) and the Société d'Etudes scientifiques d'Angers.

Almost at the same time, another Zoologist who has done good work in the study of European herpetology was carried away: Count Alexander P. Ninni, director of the Civic Museum of Venice, who died on January 7th, at the age of 55. Ninni's papers, dealing principally with the Vertebrates of Italy, appeared mostly in the *Atti del R. Istituto Veneto*, and as separate pamphlets published at Venice. A complete list of his works has just been issued by Professor Camerano in the *Bolletino* of the Turin Museum.

We also regret to record the death of the veteran Geologist and Palæontologist, Baron Achille de Zigno, of Padua, who passed away on January 15, at the age of 79.

Observations and Correspondence.

Under this heading we shall be glad to receive short comments on the broader questions of Natural Science, and on new facts of wide significance. Numerous notes of news with which we have been favoured are unavoidably held over, and Mr. Rendle's botanical contribution, announced for this number, is deferred to No. 2.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

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NOTES AND COMMENTS.

The Teaching University for London.

THE Government must be congratulated on its action in regard to the proposed Gresham University for its reference of the Charter back to the Privy Council, with the understanding that it is to be submitted to another Royal Commission. To some of the opponents of the proposed Charter this has not given complete satisfaction, though it is difficult to see what more could have been expected. The new Commission is to be made even stronger than the last, and to it (in the words of Mr. Balfour) is to be "referred the question of establishing a teaching university for London *upon a broad basis*." This remark shows that Mr. Balfour and the educationalists in the Government fully realise the weak point in the late scheme, and it is to be hoped that the friends of University teaching will be content to work for the improvement of the scheme in this respect, instead of continuing the theological protest against King's College, which has already done so much to handicap the agitation against the now rejected Charter.

The Water Supply of London.

The subject of Water Supply to towns and villages becomes every year more important, and as local sources fail in amount or in quality, the selection of sites for new supplies must engage serious attention. It is often impossible for villages to undertake deep borings or expensive reservoir works, and the time will probably soon come when the whole country must be divided into separate and co-operative districts to furnish drinking-water to groups of villages or towns that need new or more copious supplies.

London itself is provided with water only for present needs, and it is necessary to be prepared for the further growth of its vast population. It is well-known that the amounts of water that can be drawn from the Thames and Lea, and from various wells, have nearly reached their maximum. The average daily amount taken from the Thames above Teddington Weir is about 100 million gallons; the minimum daily discharge of water over the Weir is sometimes no more than 153 millions of gallons, although the maximum discharge may exceed 3,000 million gallons, even in the month of August, and is sometimes more than double this amount. The daily maximum amount of water taken from the Thames by the several water companies should not exceed 130 million gallons; for if more were abstracted, the navigation might be impeded, while the sewage in the lower Thames would not, in dry seasons, be adequately carried away. The Lea is even more seriously drawn upon than the Thames.

The Chalk under London can no longer be looked upon as a source of great additional supplies of water. This is already utilised by upwards of 200 wells, and while at the commencement of the century the water, when tapped, rose in many places to the surface, it stands now at a level of about forty feet below Trinity high-water mark. It is considered that the rate of depression now varies from one to two feet per annum.

The effect of the continuous pumping of water from the Chalk has thus been to lower the plane of saturation, whereby the flow of distant springs and streams has been more or less seriously affected. Practically, the amount of water pumped from the Chalk under London is so much taken from the streams that are fed by the natural overflow from that formation.

Various schemes have been propounded to supplant and supplement the present sources of water supply for London. It has been suggested that a number of reservoirs be constructed in the upper Thames Valley to retain much of the surplus water in flood seasons, so that it might be sent down, as occasion required, during periods of drought. There has also been a proposal to obtain water at a distance, by means of lakes or reservoirs among the Cumberland or Welsh mountains, or on Dartmoor.

There is no question but that the present supplies of water for London must be maintained, for their quality is good, and no distant sources could be considered satisfactory in times of disturbance. Additional water will, however, ere long be urgently needed, and it is very important to consider the remote sources that might be available, as Birmingham and other large towns and cities are selecting sites for supplies from our mountain regions.

Birmingham has been provided with water from wells in the New Red Sandstone, and from surface-water stored in reservoirs. It is now proposed to construct five reservoirs along the rivers Elan

and Claerwen, tributaries of the Wye, and to form the main compensation reservoir at Caban Coch, just below the junction of the two tributary streams. The sites for these reservoirs are on highly cleaved slates and thick massive grits belonging to the Ordovician or Lower Silurian system. The proposal is, in the first instance, to convey 27 million gallons of water daily to Birmingham, but the area will yield much more than that amount if required. The top of the dam of the Caban Coch reservoir would be 820 feet above Ordnance Datum, and the water could be delivered to a distributing reservoir at Frankley, at a height of 600 feet. As Birmingham stands at a high level, near the centre of England, the importance of the gathering ground that has been suggested is manifest. With regard to London, sites for reservoirs might be selected at a lower altitude.

The question of supplementary supplies for London has been partially considered in connection with the London Water Commission Bill, the Special Report on which was printed in July, 1891. It remains, however, for special and practicable schemes to be considered in detail by a committee of experts, and we understand that a Royal Commission has just been appointed for the discussion of the subject. This includes the well-known names of Sir G. Barclay Bruce, Professor James Dewar, F.R.S., Sir Archibald Geikie, F.R.S., Mr. George Henry Hill, Mr. James Mansergh, and Dr. William Ogle, F.R.C.P.

Proposed Marine Biological Station in Jamaica.

The important results which have accrued from the establishment of marine biological stations, like those of Naples and Plymouth, in temperate regions, lead us to hail with pleasure the proposal to found a similar station in the West Indies. The scheme, which is connected with the approaching celebration of the fourth centenary of the discovery of America, has been ably advocated by Lady Blake, wife of the Governor of Jamaica, in a letter to the *Times*, and has already received cordial support from many of the most eminent men of science in this country, among whom Professor Huxley has emphasised its importance in another communication to the same journal.

It is proposed that the new establishment shall be called "The Columbus Marine Biological Station," and it is hoped that it will receive support not only from this side of the water, but likewise from our trans-Atlantic cousins. No such station yet exists anywhere within the tropics, and, judging from the accounts which have been given of explorations with the surface-net, and by other means, Jamaica seems to be one of the most favourable localities for such an institution that could possibly be chosen. As Professor Huxley observes, "Animal life is indescribably abundant and varied in the intertropical seas; but the mere fringe of it has, as yet, been skimmed

by naturalists under difficulties; literally cribbed, cabined, and confined on board ship, and at best unable to do more than collect and observe. Systematic investigation of the more difficult problems —for instance, those of development—which are of such vast importance to the modern biologist, has been out of the question." The report of Professor W. K. Brooks in the *Johns Hopkins University Circular* for February, alone suffices to show what success may be expected in Jamaica. With fourteen colleagues he spent last summer at Port Henderson, in Kingston Harbour, opposite Port Royal, and materials were obtained for many important researches. Among other novelties, Professor Brooks secured a good series of the successive stages in the development of the minute crustacean *Lucifer*, sufficient for the preparation of an elaborate memoir on the embryology of that interesting organism.

Lady Blake estimates the expense of the undertaking at £15,000, and such a sum ought surely to be raised with no great difficulty if the two English-speaking nations on both sides of the Atlantic will make a united effort to raise a monument worthy to commemorate the discoverer of the New World.

No definite details are given in Lady Blake's letter as to the precise plan on which the establishment is to be founded, although we presume from the estimate of the cost that these have been carefully thought out. In a third letter on the subject, Professor E. Ray Lankester calls attention to the need of such details, and lays especial stress on the necessity of an adequate scientific and working staff, as well as on the importance of having a good cruiser attached to the establishment. With these observations we cordially agree, although we venture to think that those who have had experience of a tropical climate will attach more importance to the need for good substantial buildings than he is inclined to give. We shall, however, doubtless soon be furnished with further information from Jamaica on these points as the scheme matures, and in the meantime we wish it every success.

Field Clubs.

The spirit of Gilbert White and Darwin appears to be sadly on the wane among some of our British Field Clubs. There is plenty of vitality, it is true, in these admirable organisations, and they are all doing a good work by fostering the love of nature in their respective districts. Something more, however, is expected from the Field Clubs beyond the rousing of interest in the minds of those who would otherwise look upon Natural Science with apathy. Every club has a certain number of enthusiastic naturalists who devote all their leisure to the pursuit of their favourite studies; and this work results in many cases either in valuable collections or original papers, or in both. Sometimes the results are of so much interest and permanent

value, that the local worker eventually finds himself in constant communication with the leaders in that branch of science to which he gives attention, and his researches then become directed to the most profitable channels for further development; but in most instances there is an unfortunate want of aim and purpose in the work. Instead of making systematic observations or experiments, guided by the theories and partially established principles of modern science, the botanist or zoologist, confined to a limited area, is contented too frequently with poring over descriptive handbooks, identifying his captures, and then publishing endless lists and notes of "occurrences." There is a total absence of philosophy, and these lists and notes are only too often the melancholy signs of the approaching extinction of rare types of life at the hands of eager collectors or thoughtless "sportsmen." Notwithstanding the innumerable little treatises on animal life, practical biology, microscopy, zootomy, and practical botany, there is still room for some widely-gifted naturalist, or naturalists, to produce a book, or series of books, for the guidance of would-be observers. Modern problems ought to be stated in such terms that any ordinary student of nature, dwelling at a distance from towns and libraries, could turn his observations to some profitable account. The mere recording of "occurrences" might thus be replaced by valuable advances in our knowledge of the life-history of some of our commonest animals and plants, by the study of variations in accordance with surroundings, and also by many facts concerning what Semper terms the "Physiology of Organisms" at present almost ignored.

Pre-Glacial Man in Britain.

The discussion of man's antiquity in Britain has, by common consent, been allowed to slumber for some years. It was felt that much of the evidence brought forward was scarcely such as to command the respect of geologists, and it seemed also probable that before long facts would be discovered that would definitely settle the question. The subject has now been re-opened through the publication of three papers by Professor Prestwich.[1] We are afraid, however, that many weak links exist in the chain of argument by which he attempts to prove that man existed in this country prior to the denudation of the Thames Valley and of the Weald. Neither geologists nor anthropologists will be quite prepared, without better evidence, to accept his conclusion "that not only was the plateau

[1] "On the Relation of the Westleton Shingle, &c. Part iii.—The Southern Drift." *Quart. Journ. Geol. Soc.*, vol. xlvi., p. 155 (1890).

"On the Age . . . of the Valley of the Darent . . ." *Ibid.*, vol. xlvii., p. 126 (1891).

"On the Primitive Characters of the Flint Implements of the Chalk Plateau of Kent, with reference to the question of their Glacial or Pre-Glacial Age." *Journ. Anthrop. Inst.*, vol. xxi., p. 246 (1892).

race not contemporary with the valley men, but also that the former belonged to a period considerably anterior to the latter—either an early Glacial or a pre-Glacial period."

The plateau race referred to by Professor Prestwich in the above quotation, is supposed to have manufactured certain extremely rude implements found at high levels on the Chalk Downs, near Sevenoaks. These implements are thought to be as old as the "plateau drift" which caps the higher part of the Downs. The plateau drift is considered to be older than the oldest of the river-terraces, and older than the Boulder Clay.

If all these statements are allowed to pass without question, no doubt man is pre-glacial in Britain; but, unfortunately, many of the supposed flakes yield but doubtful evidence of human agency, and none of them have yet been found more than 2½ feet below the surface, though the Drift is said to be from 5 to 20 feet thick. At present, also, there is no evidence that the plateau drift, as we now see it, is in its original state; but it rather seems that, what with solution of the underlying Chalk, flood action, and other causes, it is almost impossible for the Drift to have remained undisturbed during a long period.

Professor Prestwich concludes that "the rude implements would appear to have been carried down, with the southern drift, on to the plateau, from those Central Wealden uplands which [he has] estimated might, in pre-glacial times, before the denudation of the Weald, have formed a low mountain range 2,000 to 3,000 feet in height." We should be inclined to doubt the existence, during any period, of such a mountain range, formed of soft strata like those of the Weald. Even granting the existence of this mountain chain, and of the floods which swept the Greensand material northward on to the Chalk plateau, it does not necessarily follow that the implements were transported in the same way, or are of contemporaneous date. They may be of much later date, and only accidentally associated with fragments of Greensand, such as may have been moved and re-deposited, time after time, before coming to rest in the now-existing red clay.

Professor Prestwich's earlier papers form a most valuable contribution towards a study of Palæolithic man and his tools, but we do not feel satisfied that in these latest memoirs he has advanced our knowledge of the antiquity of the human race in this country. The question still remains as it stood several years ago.

Two Zoological Fables.

In 1719, Leuwenhoek first started the idea that Rotifers, or "Wheel-animalcules," could be completely dried and kept for years in that condition, and then restored to life by being moistened with water. His conclusions were supported by Spallanzani in 1776

after a long series of experiments, and the theory of "palingenesis" passed into the rank of accepted truths: thus the name of *rediviva* was frequently applied to new species by authors who might be supposed to possess especial knowledge of the group. Hence it is not surprising that the idea that Rip-van-Winkledom was a normal condition of things among Rotifers should have gained widespread popularity, and have done duty as one of the principal articles of the stock-in-trade of successive generations of lecturers and writers on the romance of natural history.

So improbable a theory, however, was not allowed to pass unchallenged, and Pouchet, in 1859, subjected it to very severe adverse criticism. Frédéricq, in 1889, and Zaccharias a year later, also opposed it, while now Dr. F. Faggioli, of Genoa, has completely demonstrated the erroneous character of the whole story.[1] He has carried out a series of experiments conducted with the precise methods of modern research, and has shown that a Rotifer, if once properly dead or dried, cannot in any way be brought to life again. Dr. Faggioli has also pointed out the nature of the mistake made by earlier experimentalists: they had identified a new generation that had developed from eggs as the old generation again risen from the dead. It was found necessary always to dry the Rotifers on sand, preferably that from the tank in which the Rotifers grew. Dr. Faggioli has shown that the eggs are not destroyed by drying, and that when the experiments were successful, eggs must have been introduced with the sand or in the bodies of the Rotifers. On placing these eggs in suitable conditions development proceeds, and a new crop of Rotifers is the result.

There can be no doubt that the supposed recovery of the Water-Bears (Tardigrada) affirmed by Doyère, admits of the same simple explanation.

Another oft-quoted marvel that has recently been further exposed is the wonderful adaptability of the freshwater Polype, *Hydra*. It has long been said that if *Hydra* be turned inside out it accepts the situation, and lets its skin act as its stomach, and its stomach as its skin. This and many other antics were observed by Trembley, and described in a memoir issued in 1744; they were very widely believed in an age which knew little or nothing about the differences between ectoderm and endoderm, and which never troubled whether the spermatozooids were formed from the former or the latter, and which had never heard of mesoglæa. With the growth of knowledge of Hydroid anatomy, however, the story became discredited, and now some Japanese biologists have repeated the experiment, the results suggesting that Hydras have changed their habits rather than their skins, or that the worthy Abbé's observations were not as careful as they might have been. The Hydras may live for a little time under

[1] "De la prétendue réviviscence des Rotifères," *Archiv. ital. Biol.*, vol. xvi., pp. 360–374 (1891).

these altered conditions, but they make every effort to get right side out again; if this is impossible, a new formation of ectoderm takes place around the base of the tentacles, and may spread further over the animal. There seems no reason to believe that the endoderm ever functions as the ectoderm, or *vice versâ*, while the stories about the reproduction of Hydra from small parts have also been exaggerated.[1]

Hence, though it is very sad to have to part with two such old friends as the reversible Hydra and the resuscitated Rotifer, biology will gain more than it will lose by this substitution of common sense for paradox and puzzle.

Extinct Vertebrata in the Grecian Archipelago.

In a special chapter of a work on the natural productions of Samos,[2] Dr. Forsyth Major gives us further information on the wonderful extinct vertebrate fauna of that island which he himself has been the means of bringing to light. We are first told how the author was led to believe in the existence of such remains from passages occurring in the writings of Euphorion and Plutarch, and how he was finally rewarded by their discovery near the village of Mitylene. Dr. Major has determined the existence of over forty species of animals from these deposits, of which all but four are mammals. Many of these are common to the equivalent deposits of Pikermi in Attica, Baltavar in Hungary, and Maragha in Persia; and the relationship of the fauna to that of modern Africa is exemplified by the number of species of antelopes, by the occurrence of Ethiopian types of hyænas and rhinocéroses, and likewise by the occurrence of an ostrich and an aard-vark.

The most interesting of all the animals noticed by Dr. Major are those of which the teeth were originally described as referable to an ungulate (*Chalicotherium*), and the claws as those of an edentate. It is concluded that, although these animals present a most remarkable resemblance to some of the Odd-toed or Perissodactyle Ungulates in the structure of their teeth and limbs, yet that they have really no direct relationship, but represent a distinct order—Ancylopoda. The three-toed feet, with their enormous curved claws, differ from those of the Perissodactyles in that the middle toe is smaller than the others; while in their powers of prehension these limbs are, of course, quite unlike those of all modern Ungulates. Dr. Major considers that while the representative of these animals found at Samos and

[1] For the latest literature on this subject, see Engelmann, *Zool. Anzeiger*, vol. i., pp. 77–8 (1878); W. Marshall, *Zeitschr. für Wiss. Zool.*, vol. xxxvii., pp. 664–702, and especially p. 682 (1882). For an account of the experiments by Mitsikuri and others, see *American Naturalist*, vol. xxi., pp. 387–8 and 773 (1887).

[2] *Samos.—Etude géologique, paléontologique, et botanique*, by C. de Stefani, C. J. Forsyth Major, and W. Barbery. Lausanne, 1892. 4to.

Pikermi constitutes one genus (*Chalicotherium*), that from the somewhat lower beds of Sansan, in the Gers, must be referred to another (*Macrotherium*), distinguished by the structure of its foot.

When discussing the age of the Samos, Pikermi, and Maragha strata, the author refuses to admit the view of their Pliocene age, and continues to regard them as Miocene. We fail, however, to see how he has accounted for the presence of beds with marine Pliocene shells among and below the Pikermi strata, or for the alleged upward passage of those at Maragha into the Pleistocene. The author proceeds to consider the relations of these beds to the Siwaliks of Northern India, which he concludes to be somewhat newer, and more nearly related to the mammaliferous strata of the Val d'Arno, in Tuscany. We may, however, point out that the Siwaliks embrace an immense thickness of strata, which may represent the whole period during which both the Pikermi and Val d'Arno beds were deposited; and this would well harmonise with the occurrence in the Siwaliks both of the three-toed horses (*Hipparion*) of the former and the true horses of the latter beds. Moreover, in such correlations something must be allowed for zoological regions, which probably existed to a certain degree even in those early days.

The discovery of the extinct mammals of Samos, and of those of Maragha a few years previously, has largely increased our knowledge of the relations of the ancient vertebrate faunas of Europe and Asia; but a full exploration of Baluchistan is still required before we can obtain definite information as to the connection between the fauna of Maragha and the Siwalik fauna of the Punjab. Eastwards of India, the extension of the extinct mammals of the Siwaliks into the islands of the Malayan region and China has been gradually made known of late years, and a small collection of remains from Mongolia recently described by Mr. Lydekker (*Records Geol. Survey, India*, vol. xxiv., 1891, p. 207) now indicates that the same fauna ranged far into the heart of Central Asia.

A fine skull of the new variety of the extinct Irish Deer (*Cervus giganteus*, var. *ruffi*) is described and figured by Dr. A. Nehring in the *Deutsche Jäger-Zeitung* for February 7. This variety was first indicated in 1891 from a shed antler obtained near Kottbus in Brandenburg, and the new "find" was dredged from the Rhine near Worms. The peculiarity of this variety is that the antlers, instead of extending nearly horizontally outwards, are directed upwards like those of a fallow deer, and have a similar inclination of the plane of their expanded portion. Although they agree with the ordinary Irish deer in their palmated brow-tine, they indicate a decided approximation to the fallow deer type, this being especially shown by the

position of the posterior tine, which clearly represents that of the latter. Professor Nehring has some doubts whether this deer ought not to be regarded as a distinct species.

An important contribution to the ancestral history of the Horse is afforded by Professor W. B. Scott's memoir on *Mesohippus* and *Leptomeryx* in the December number of the *Journal of Morphology* (vol. v., pp. 301–406, pls. xxii., xxiii.). *Mesohippus*, from the White River Miocene of the United States, was an animal about the size of a Newfoundland dog, and closely allied to *Anchitherium* of the Miocene of Europe. According to Professor Scott it was distinguished from the latter by the absence of any rudiment of the infolding of the crowns of the upper incisor teeth (comparable to that occurring in those of the horse), and by certain details in the structure of the other teeth and the limbs. *Mesohippus* is considered to have occupied a position near the middle of the ancestral "phylum" of the horse, while it is suggested that *Anchitherium* may have been a side branch.

The other animal described in the same memoir is *Leptomeryx*, which Dr. Scott regards as closely allied to the modern chevrotains, but showing certain resemblances in the form of the incisors and the auditory bullæ of the skull to the true Ruminants or Pecora. The latter features are, however, considered to have been independently acquired.

In the last number of the *American Journal of Science* (vol. xliii., pp. 249–262, pls. v.–xi.) Professor O. C. Marsh gives a further instalment of his researches on the fossil mammals of the Laramie Cretaceous beds of the United States. It is considered that the fauna indicates a marked break between the Laramie Cretaceous and the Puerco, or Lower Wahsatch, Eocene beds. The mammalian remains obtained from the former are now very numerous, although, unfortunately, very fragmentary, consisting mostly of detached teeth. Professor Marsh describes a number of these specimens, of which many are regarded as indicating new species or genera. We consider this reckless multiplication of names much to be deplored, as there is little doubt that far too many species and genera have already been made upon the evidence of specimens from these deposits. That the number of so-called species and genera will eventually have to be reduced materially, Professor Marsh himself seems to have a suspicion, when he attempts to warn off criticism by saying that, in comparison with describing species on the evidence of imperfect specimens, "it is a matter of much less importance if such discoveries should prove that two or more specimens, described as distinct, really pertained to one animal."

A large proportion of the remains described belong to the group generally known as Multituberculata (Allotheria of Professor Marsh);

and it is stated that the new specimens serve to indicate more conclusively than hitherto the relationship of that group to the Monotremes, although this is not very apparent from the descriptions. The other remains belong to Marsupials, which are considered to be most nearly related to the living opossums.

It is not often that ordinary text-books are in advance of current thought, but Mr. Arthur Thomson's *Outlines of Zoology*, which we review elsewhere, contains one bold statement that we commend to the notice of those interested in the classification of vertebrated animals. The author remarks: "What the 'worms' are among Invertebrates, the Reptilia are among Vertebrates—an assemblage of classes." Unfortunately, nearly all the more important forms of reptiles are extinct, and they are thus known only by skeletons; but that they are a medley of widely distinct groups is becoming very evident. As shown more especially by the researches of Cope, Marsh, and Seeley, the early extinct "Reptilia" (including the Labyrinthodonts) form the base of at least four widely divergent lines of evolution—the modern Batrachia, the modern Reptilia, Birds, and Mammals. The formal dismemberment of the "class" may be only a question of time.

Although known now for twenty years, it is remarkable how little information has been added to our knowledge of the Australian Mud-fish, *Ceratodus*, since its original description by Dr. Günther, and the subsequent notes by Professor Huxley. At the meeting of the Australasian Association in Hobart Town last January, Professor Baldwin Spencer returned to the subject, describing some of the habits of the animal; but, judging from the brief report to hand, he had little new to communicate. It is suggested that the lung-like air-bladder of *Ceratodus* is of special use to the fish at times of floods, when the water in which it dwells is charged with much sediment; and there is no evidence that *Ceratodus* ever leaves its native element, being in this respect unlike its African ally, *Protopterus*, which lives coiled up in a "cocoon" in dry mud during the hot season. It is of the greatest importance from the philosophical point of view that we should have some detailed memoirs on the embryology of *Ceratodus*, and to us it is inexplicable that none of the able biologists in Australasia have yet accomplished so important a research. Nearly ten years ago Mr. Caldwell, of Cambridge, made the collection of the embryos of *Ceratodus* one of the special objects of his mission to Australia, towards the expenses of which both the Royal Society and the University of Cambridge contributed funds; yet, so far as we are aware, there are at present no scientific results. Specimens collected at public expense ought to be public property, and if the naturalist who does this service loses his enthusiasm or undertakes

other duties that prevent the carrying out of his agreement, the material ought to be at once handed over to some competent colleague who will render it available to science.

Of the extinct allies of the recent *Ceratodus* we have learned a little more during the last twenty years. In 1891, indeed, the greater part of the skull of the true Triassic *Ceratodus* was described in Austria[1]; and some years previously a tail had been found in the Upper Trias of Würtemberg. Teeth of the same genus have also been discovered in Western North America and South Africa. It now appears from Teller's memoir on the Austrian specimen, that, although the true *Ceratodus* of the Triassic period was very similar to the fish still living in Queensland, there are generic differences; and it is proposed that the recent fish should henceforth be termed *Epiceratodus*. The name *Neoceratodus* would have been more appropriate, but that has been already used for an ally in New Guinea. The fossil, so far as known, differs from the recent fish in the greater extent of the ossification of its skeleton.

Speaking of the extinct Dipnoan Fishes, reminds us of the remarkable change in the ordinary conceptions of this sub-class that certain palæontologists are inclined to introduce. In the second part of the *Catalogue of the Fossil Fishes in the British Museum*, published last year, Mr. Smith Woodward hazarded the suggestion that in early Palæozoic times some of the tribes of Dipnoan fishes were as highly specialised in comparison with the *Ceratodus* type as are the modern Teleostei compared with the Fringe-finned Ganoids from which they are doubtless descended. In a new number of the *Proceedings of the United States National Museum* (vol. xiv., pp. 449-456) just received, Professor E. D. Cope describes the skull of the Devonian *Macropetalichthys*, and is in favour of the same theory. Indeed, so far as the facts are known, the race of Devonian armour-plated "ganoids," of which *Coccosteus*, *Homosteus*, and *Dinichthys* are the best preserved genera, have the cartilages of the upper jaw fused with the skull, and are comparable only with the Dipnoi in the arrangement of their head-shield and dentition. The supposition that the Dipnoan fishes attained to an extreme degree of specialisation at the time when they were a dominant type of life is quite in accordance with known laws; and the fact that none but the more generalised forms survived until later periods, is also what might have been expected. We await with interest further discoveries on the subject.

Professor A. Giard has just published a brief account of a small turbot, in which the original bilateral symmetry of the fish is

[1] F. Teller, "Ueber den Schädel eines fossilen Dipnoers, *Ceratodus sturii*, sp. nov.," *Abhandl. k.k. geol. Reichsanst.*, vol. xv., pt. 3, 1891.

partially retained, the eye of the right side not having quite passed over the top of the head, and both sides of the body being pigmented (*Comptes rendus Soc. Biologie*, Jan. 16, 1892). The Professor concludes that the coloration of the upper exposed side in the flat-fishes is due to the direct action of the light, and arrives at some other interesting results which we hope to notice on a future occasion.

For some years Dr. R. Blanchard (of 32 Rue du Luxembourg, Paris) has been occupied with the preparation of a *Monograph of the Hirudinea*. He has now almost completed the section on Gnathobdellidæ, and appeals for the help of naturalists and museums who can supply him with leeches of which the respective habitats are definitely known.

The interesting new Crinoid from the Galapagos Islands, *Calamocrinus diomedæ*, has at last been fully described by Professor Alexander Agassiz (*Mem. Mus. Comp. Zool. Harvard*, vol. xvii., No. 2, 1892). The monograph, which has just arrived in England, enters very minutely into the structure of the skeletal system of this form, and also contains notes on the apical system and the homologies of Echinoderms generally. There are thirty-two plates, and rarely has any Crinoid had its hard parts so magnificently illustrated. Since, however, the author has given no diagnosis, no succinct description, and no synopsis of its relations to other genera, we must defer a fuller account of this remarkable form until we have digested the large mass of facts now published.

A study of the nervous system of organisms so lowly in the scale of animal life as the Alcyonarian polypes (*e.g.*, the common "Dead-men's fingers" of our British coasts) presents many features of interest, and has just been undertaken by Dr. S. J. Hickson. At the meeting of the Cambridge Philosophical Society on February 22, Dr. Hickson communicated some preliminary results of his researches. In fresh specimens of *Alcyonium* stained with osmic acid, a plexus of very fine nerve-like fibrils, connected with a number of minute uni-, bi-, or tri-polar ganglion-cells, could be distinctly recognised in the dense, transparent gelatinous material between the several polypes of the colony. In their normal state the polypes of the *Alcyonium* contract regularly twice every twenty-four hours; and even when the colony is removed to an aquarium, where there are no tides, the regularity of the periods of contraction and expansion continues for two or three days. After such a period in an aquarium the polypes begin to remain expanded or contract only irregularly; but a new rhythm may be easily induced by subjecting the *Alcyonium* to the action of an artificial tide of any given duration. There is much analogy between this phenomenon and the opening and closing of flowers.

THERE are many advantages in comprehensive handbooks prepared by a single author. They present a uniformity of plan and treatment that is nearly always lacking in composite works. In these days of specialisation, however, the production of such handbooks is becoming almost impossible; and most of the larger treatises are the result of the labours of many compilers. According to the *Zoologischer Anzeiger* of February 1, another of these indispensable works of reference is being prepared, and we may hope within the next few years to have a great Manual of Zoology written by a number of the most eminent specialists in Europe. The respective authors desire that they may be promptly furnished with reprints of all new papers and memoirs relating to their various subjects, as enumerated below:—

Rhizopoda, Ciliata, and Suctoria—Dr. Fabre-Domerque, Paris. Sporozoa—Professor Moniez, Lille. Flagellata—Professor Künstler, Bordeaux. Porifera—Professor Vosmaer, Utrecht. Anthozoa—Professor Künstler. Hydrozoa, Siphonophora, Acephala, Ctenophora—Professor Lang, Zurich. Dicyemidæ—Professor Van Beneden, Liége. Orthonectidæ—Professor Julin, Liége. Trematoda, Cestoda—Professor Moniez. Turbellaria—Professor Lang. Rotifera, Gastrotricha—M. De Guerne, Paris. Archi-annelida, Sternaspida, Phoronida—Professor Roule, Toulouse. Hirudinea—Professor De Nabias, Bordeaux. Annelida—Professor Yung, Geneva. Bryozoa—Professor Cuénot, Nancy. Brachiopoda—Professor Joubin, Rennes. Mollusca—Professor Pelseneer, Ghent. Protracheata—Professor Vayssière, Marseilles. Nemathelminthea, Acanthocephala—Professor Köhler, Lyon. Nemertea, Chætognatha—Professor Joubin. Linguatulida, Tardigrada, Pycnogonida, Arachnida—Professor Barrois, Lille. Myriopoda, Insecta—Professor Vayssière. Crustacea—Professor Giard, Paris. Echinodermata—Professor Cuénot. Enteropneusta—Professor Köhler. Tunicata—Professor Van Beneden. Amphioxus, Cyclostomata—Professor Julin. Pisces, Amphibia, Reptilia, Aves—M. L. Dollo, Brussels. Mammalia (except Primates)—Professor Weber, Amsterdam. Primates, including Homo—Dr. Deniker, Paris.

Some time ago we heard rumours of a similar Manual projected at Cambridge, to be undertaken by British naturalists; but there is as yet no definite announcement.

THE supposed discovery of pseudopodia in diatoms, announced by Mr. J. G. Grenfell in the *Quarterly Journal of Microscopical Science* for October last, is still being disputed by experienced observers. In the January number of the *International Journal of Microscopy and Natural Science*, issued late last month, Mr. Jabez Hogg adds his testimony to that of other critics, and briefly disposes of the so-called evidence of "pseudopodia." Mr. Hogg confirms the view of Mr. G. H. Bryan that the processes in question "are, in no sense of the word, pseudopodia, neither are they, nor can they be, regarded as organs of locomotion; and their discovery—which, by the way, is a very aged one—throws no light whatever on the debatable question: the movements of diatoms. These spinous processes have been described over and over again. Their composition is simple enough: Organic matter, carbonate of lime, and a slight admixture of a silicated peroxide. They are apparently secreted by the diatom for the purpose of affording a rigid support to

the gelatinous or protoplasmatic material outside the framework or skeleton. Very many genera besides *Melosira* and *Cyclotella* are known to possess similar spinous processes, which are stiff, non-retractile, and arranged symmetrically around the valves. The same kind of perfectly rigid spines are also a characteristic formation of many desmids—*Micrasterias*, *Arthrodesmus*, *Epithemia*, *Stephanosia*, and *Achnanthidium*. These appendages, however, bear no resemblance to the well-known pseudopodia of the lower Protozoa, which are most undeniably organs of locomotion and prehension." Mr. Hogg continues with a readable review of the theories of the movement of diatoms, which we commend to the notice of all interested in the subject.

THE *Journal of Botany* makes the welcome announcement that Messrs. J. Britten and G. S. Boulger are preparing to issue in separate form the Biographical Index of British and Irish Botanists, which has for several years been running through the journal and has lately been completed. The Index is to appear in June, and will extend to the end of 1891.

A STUDY of the Speeton Clay and its Russian equivalents has led Professor A. Pavlow to publish a long memoir on the various forms assumed by the "guards" of Belemnites in the new number of the *Bulletin de la Société Impériale des Naturalistes de Moscou* (vol. for 1891, Nos. 2, 3). Many good figures and detailed descriptions of Cretaceous species are given, and the memoir concludes with some general observations on the classification of these fossils. The "guards" of Belemnites seem to be of much value to the stratigraphical geologist—"medals," as Mantell would term them—but it is to be regretted that they furnish so slight an idea of the characters and evolution of the cephalopods to which they originally belonged. We are glad to notice that our countryman, Mr. Lamplugh, is co-operating with Professor Pavlow in his researches.

WE regret to learn from recent letters from Dr. J. W. Evans, the geologist with the Matto Grosso Expedition, that the party has just returned to Europe after a somewhat disappointing journey. This seems to have been mainly due to the action of one of the recent Governments of Brazil, which refused to ratify the concession made by the late Emperor. The district in which Dr. Evans spent most of his time was not a profitable locality geologically, consisting of enormous tracts of crystalline schists, clay slates, and sandstones; these have so far not yielded a single fossil, and igneous rocks are represented only by seams of ashes in the slates. A few bands of limestone were met with, but they are all unfossiliferous. The most interesting part of Dr. Evans' journey seems to have been an ascent of the

lower peak of Urucum, some 2,500 feet high. On the mountain there are massive deposits of pure pyrolusite. The richly fossiliferous Mesozoic Limestones of Bolivia and the Andes of Western La Plata do not seem to extend as far east as the southern part of Matto Grosso. Dr. Evans' fellow-traveller, Mr. Spencer Moore, has made a large collection of plants.

At the meeting of the Zoological Society on March 1, Mr. Graham Kerr exhibited a series of lantern slides prepared from negatives taken during his ascent of the Pilcomayo River in the steamer, "Explorer." These gave a good representation of the various types of scenery met with during that ill-fated expedition, and especially of the salt desert where the steamer was finally abandoned. The most valuable part of Mr. Kerr's collections had to be left with the steamer, but, nevertheless, important additions were made to the knowledge of the fauna of the district.

A list of the fossils at present known from Western Australia has been published by Mr. Harry P. Woodward, the Government Geologist, in his report for 1890 (issued at the close of 1891). It is interesting to find familiar European names among the Devonian, Carboniferous, and Jurassic species; of special interest, too, is the record from the Cambrian rocks of *Olenellus* and *Salterella*.

The excessive rainfall during the early part of the present winter has caused the opening of numerous chasms or "swallow holes." These are found principally along the line where Tertiary clays overlap the chalk. Fortunately, in the southern counties, few houses happen to be situated exactly on the line where "swallows" are likely to occur. Thus far all the subsidences have taken place in woods or open fields.

The *Mediterranean Naturalist*, a monthly journal of Natural Science, edited by Mr. J. H. Cooke, B.Sc., F.G.S., of St. Julians, Malta, does not appear to be so widely known in Britain as it deserves to be. The first part appeared on June 1, 1891, and the journal contains much matter concerning the Mediterranean region of very wide interest.

We are glad to hear that the *American Naturalist*, of which no number has reached England since November, is to be revived almost immediately under the auspices of new publishers.

I.

Factors in the Evolution of the Mammalia.

AT the close of his valuable memoir "On the Osteology of *Mesohippus* and *Leptomeryx*" in the last number of the *Journal of Morphology* (Vol. v., No. 3) Dr. W. B. Scott, of Princeton College, New Jersey, devotes a section to a critical consideration of some of the factors in the evolution of the mammalia. Therein, at the outset, he expresses his dissent on general grounds from Dr. August Weismann's now familiar theory of the continuity of the germ-plasm, according to which it is impossible for any character or modification of structure acquired during the life of the parent organism to be transmitted to the offspring which that parent subsequently produces. Quoting from Mr. W. H. Dall, he says: "The contention of Weismann, that 'not a single fact hitherto brought forward can be accepted as proof' of the transmission of acquired characters . . . reminds one of the familiar statement of twenty years ago, that the Darwinians had not brought forward a single instance of the conversion of one species into another species. If the Dynamic Evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error detected in it."

The special object of Dr. Scott's discussion is to show that the careful examination of certain series of fossil mammals indicates clearly that the Dynamic theory, that is to say, the hypothesis of the inheritance of acquired modifications of structure due to the direct action of the environment, is more probable than the hypothesis of unaided Natural Selection; or, to state the contention in his own words, "that transformation, whether in the way of the addition of new parts or the reduction of those already present, acts just *as if* the direct action of the environment and the habits of the animal were the efficient cause of the change, and any explanation which excludes the direct action of such agencies is confronted by the difficulty of an immense number of the most striking coincidences."

The evidences in favour of this contention are briefly as follows:—

(1.) The changes which are observed to occur in the fossil mammals under consideration are carried out in exact accord with the mechanical exigencies of the case. New facets on the bones and new cusps on the teeth appear in definite ways and in definite places. In the structure of the carpus and tarsus, the bones which answer to

those in the wrist and ankle of man, "we find that in any given phylum [or series in direct descent] very definite lines of evolution are early established and closely adhered to, and the changes are just those called for by the operation of dynamical influences." Moreover, in the matter of modifications of tooth structure, in all cases known to the author, the new points appear in places where, at some stage, there is special abrasion.

(2.) The evidence afforded by the reduction and loss of structures supports that afforded by the growth and development of structures. Such loss and reduction is ascribed by Dr. Weismann to "panmixia," or the *cessation* of that selective process which raised them to a high level of perfection and efficiency, and, so long as it continues, keeps them there. When the selection ceases, those individuals which have these now useless structures poorly developed are no longer weeded out and, breeding with those individuals which have the structures well developed, lower step by step the standard of efficiency. To this explanation Dr. Scott dissents on general grounds; and, with respect to the special field of mammalian palæontology, he says: "When we turn to the series of fossils, and follow out the history of disappearing organs, we find little to support the theory of panmixia. The reduction is steady and sure, if slow, exhibiting, of course, a certain degree of individual variation, but not the fluctuations which we should naturally expect to find were panmixia alone the cause of the reduction. As a matter of fact, when examining an extensive series of fossils reaching through many horizons, it is difficult to escape the suspicion that individual variations are not the material with which Natural Selection works, so steadily does the series advance toward what seems almost like a predetermined goal."

(3.) While the development of useful structures and the loss or reduction of useless structures are thus in close accord with the mechanical conditions, there is no evidence of tentative trials and false starts before the proper line of development is hit upon. If variations in accordance with mechanical conditions were selected from among other variations, we should expect to find evidence of such other non-adaptive variations. These may, however, be sought for in vain. The development of the carpus and tarsus may be traced step by step in the equine series from *Hyracotherium* to *Equus*. We find a definite chain of adaptive modifications. Non-mechanical variations do not occur.

(4.) Under the influence of similar environing conditions similar modifications have been induced in parallel series of animals not closely related to each other by genetic affinity. Thus, in several different groups there appear suddenly and simultaneously (using these terms in their geological sense) prismatic or rootless molar teeth, with very complex enamel foldings, the valleys between the harder enamel ridges being filled with the softer "cement." This development of complex grinding teeth is found to be contemporaneous

with the great extension of grassy plains. It may, therefore, be correlated with this extension of grass lands; and we may infer that the complex grinding teeth arose as a consequence of a change of food-supply presented to the herbivorous mammals from a diet consisting of soft plants and leaves to a diet consisting mainly of siliceous grasses. "Now, what is the probability," asks Dr. Scott, "that such a series of changes in horses, rhinoceroses, pigs, ruminants, elephants, and other families, should be due primarily to the mingling of different hereditary tendencies, especially when it is remembered that none of the ancestors of these groups possessed any such teeth? Or can it be reasonably contended that such parallel variations are due to the direct action of the climatic or other environment upon the germ-plasm?"

According to Dr. Weismann, it must be remembered, variations arise in the main from the combinations of slightly differing developmental tendencies in the elements derived from the parents which unite to form the offspring. Nothing acquired during the life of either parent, or both parents, can affect the germinal or fertilising element. It is, therefore, a matter of chance whether any new variation shall be well-adapted to somewhat new conditions of life, or shall be ill-adapted to such new conditions; but the well-adapted survive, while the ill-adapted succumb. How is it, asks Dr. Scott, that we find only favourable adaptations? Where are the unfavourable adaptations that succumbed? And how comes it that in parallel series of animals similar favourable adaptations so constantly occur, if these similar favourable adaptations be not due to the direct influence of similar environing circumstances?

In a presidential address to the Bristol Naturalists' Society, I briefly considered the bearing and value of such palæontological evidence in the vexed question of "The Nature and Origin of Variations," and I ventured to express a doubt as to whether the evidence could be regarded as conclusive as to "use-inheritance," or the transmission to the offspring of modifications of structure acquired by the parents during their individual life-time. In criticising the position occupied by Dr. Scott and those whose views are in accordance with his, I thought that some stress might be laid on the imperfection of the geological record, and that the number of individuals in our palæontological collections was not sufficient to constitute a truly representative sample. Furthermore, I suggested that, on the hypothesis of selection, the individuals possessed of unfavourable, unadaptive modifications must have been weeded out in the early stages of life. In reply to such criticisms, Dr. Scott says "that the objection drawn from the imperfection of the geological record, and from the small number of individuals contained in the museums, can be allowed little weight. So far as several of the mammalian phyla are concerned, the number of missing links must be very small, the wonderful series of American freshwater

Tertiaries being in this respect quite unrivalled in the known world. Of the better known species, the collections already contain hundreds, and sometimes thousands, of individuals available for study [and by implication, I presume, *actually studied*]. If results obtained from such material point in one direction, it is surely most illogical to assume that specimens yet to be discovered will probably lead to opposite conclusions. The probabilities are all the other way. . . . That individuals in the early stages of life are uncommon as fossils is very far from being the case. They are abundantly represented in the collections, and show no more tendency to indeterminate variation than do the adults. But even were the young stages unknown, before their absence could be allowed weight as an objection, it would be necessary to show that such very slight changes were of 'elimination value.'"

It is not my intention here to urge again my previous criticism. Personally, I also am disposed to question whether such very slight changes would be of what I have termed "elimination value," that is, whether they would be fatal in life's competition; but the thoroughgoing believer in Natural Selection as the sole factor in evolution does not accept my "elimination value." He says *all* unfavourable variations, no matter how slight, are eliminated; and it is he that we have to convince. From his point of view, then, I am still somewhat doubtful whether the material already studied is sufficient to justify the assertion that unfavourable variations in the budding stage (*i.e.*, scarcely recognisable, or deemed of no consequence by the palæontologist, but readily picked out by the "ever-vigilant eye" of Natural Selection) do not occur.

Granting, however, that my criticism was ill-founded—and it should be remembered that I was criticising the view to which I myself lean—it seems to me probable that, on the selection hypothesis, determinate variations in the direction of mechanical adaptations would far outnumber other variations in other directions. Take the case of an organism which has in some way reached harmony with its environment. Slight variations occur in many directions, but these are bred out by intercrossing. It is as if a hundred pendulums were swinging just a little in many directions, but were at once damped down. Now, place such an organism in changed conditions. The swing of one or two of the pendulums is found advantageous; the organisms in which these two pendulums are swinging are selected: they mate together, and in their offspring while these two pendulums are by congenital inheritance kept a-swinging, the other 98 pendulums are rapidly damped down as before.

Let us suppose, then, that the variation of tooth structure, in a certain mechanically advantageous direction, be such a selected pendulum swing. That particular pendulum, swinging in that particular direction, will be the subject of selection. The other pendulums will still be damped down as before, and in that particular

pendulum variations from the particular direction will be similarly damped down. It will wobble a little, but its wobbling will be as nothing compared with the swing that is fostered by selection. In this case, then, selection will choose between the little more complexity that is advantageous and the little less complexity that is disadvantageous. The little less complexity will be eliminated, the little more complexity will survive. The little less and the little more, however, are in the same line of developmental swing. Hence, the variations discoverable in fossil mammals in which tooth-development along special lines is in progress, will, on the hypothesis of selection, be plus and minus along a given line; in other words, the variations will be determinate, and in the direction of special adaptive modifications.

Of course, we may believe—as those who hold the view that acquired characters are transmitted to offspring do believe—that individual use gives the pendulum an added push. But if the considerations above urged have any weight, we cannot prove this by a study of palæontological records. It must be proved, if proved at all, by some crucial experiment still to be devised.

Unquestionably the phenomena of parallelism on which Dr. Scott, as we have seen, lays stress, forces on the believer in Natural Selection as the sole known factor in evolution the assumption that in different groups similar congenital variations occur. In the language of our analogy, each of half a dozen different groups has among its hundred pendulums one which is swinging in such a way that the plane is parallel to that of the other five. Or in the case of prismatic and complex tooth-structure cited from M. Kowalevsky, horses, rhinoceroses, pigs, ruminants, elephants, and others are all ready to develop a certain general type of molar, when the extension of grassy plains renders such development of advantage. It is, however, of the essence of the Darwinian faith to believe that the whole organism is eminently plastic, and, according to Dr. Weismann's view, the ever-varying combinations of germ-plasm give unlimited possibilities of advance, retrogression, or divergence in the congenital modification of structure.

In conclusion, I may, perhaps, be allowed to say that in the general tendency of our opinions Dr. Scott and I are at one. Where we differ is in the estimate of the value of palæontological evidence. By itself, this does not seem to me to be sufficiently convincing or conclusive to win over a single follower of Dr. Weismann. If the transmission of acquired characters were demonstrated as an unquestionable fact, the Dynamic Theory would no doubt be generally accepted as a *vera causa* in the evolution of the Tertiary Mammalia of the North American Continent; but to one who denies the possibility of such transmission, all the facts brought forward by Dr. Scott will still be held to be explicable on the hypothesis of Natural Selection.

C. Lloyd Morgan.

II.

Some Salient Points in the Study of Mammals during 1891.

(*Continued from page* 39.)

Passing on to fossil mammals, one of the most important and interesting memoirs published during the year relates to the extinct allies of the giraffe[1]—an animal which, as most of our readers are probably aware, now occupies a somewhat isolated position, being the sole representative of a distinct family. In the Pliocene epoch there appear, however, to have existed a number of ruminants more or less closely related to the giraffe, and it is the object of the memoir quoted to show that this relationship is even more intimate than had hitherto been considered to be the case.

Giraffes themselves, it need scarcely be observed, are now confined to Africa, but there is abundant evidence to show that they were formerly spread over Southern Europe, Persia, India, and China. One of their nearest extinct allies seems to be the *Samotherium*, from the Pliocene of the Isle of Samos and Persia, in which the males (Fig. 3) had short, upright horn cores, not improbably covered with skin in the living condition, while the females were hornless. In some respects, however, *Samotherium* shows decided signs of affinity with the Deer-family, as represented by the Elk; this being especially manifest by the equality in the length of the fore and hind limbs, and the comparatively straight profile of the middle part of the skull, and we thus have an interesting confirmation of the view which has been long current as to the kinship of the giraffe with the deer. Of especial interest is the proof afforded by this animal that a comparatively small ruminant from the Pliocene of Greece, known as *Palæotragus*, and hitherto regarded as an aberrant antelope, is also a near cousin of the giraffes, of which group it is the smallest known representative.

The connecting link between the Giraffe and the Samothere is formed by the well-known Helladothere, of the Grecian Pliocene, in which the fore limbs were longer than the hinder, while all the known examples of the skull were unprovided with antlers or horns, although the middle of its frontal region has a prominence corresponding to

[1] Forsyth-Major, *Proc. Zool Soc.*, 1891, pp. 315–326.

the so-called median horn of the giraffe. Perhaps, however, the most interesting portion of the whole memoir is that relating to the gigantic ruminants from the Pliocene of India, severally known as the *Sivatherium*, *Bramatherium* and *Hydaspitherium*. These creatures, which equalled a rhinoceros in bulk, were characterised by carrying large and branching appendages on their enormous skulls, which probably partook of the nature both of the antlers of the deer and of the horns of the oxen. There has been much discussion as to the affinity of these animals, some writers affiliating them with the antelopes, and others with the giraffe; but the author of the memoir before us so ably advocates the latter view that the question may now be regarded as finally decided. We may accordingly now look upon the giraffe as the last survivor of an extensive family of ruminants, which, after having included among its members the most gigantic representatives of the whole group, for some unknown reason suddenly disappeared

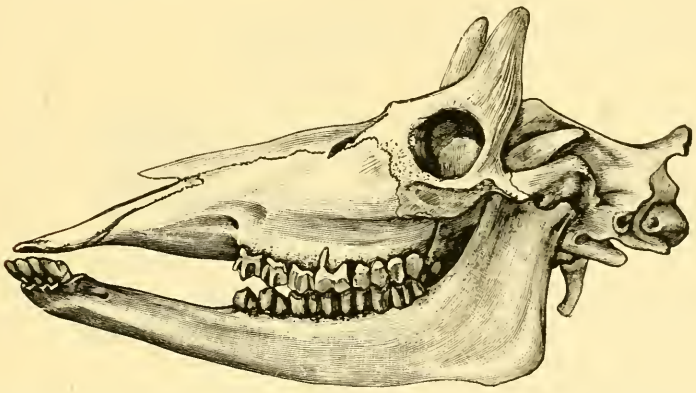


FIG. 3.—Side view of the skull of *Samotherium*. About $\frac{1}{7}$ natural size. The horn-cores are provisionally restored.

from the face of the earth, with the exception of the one species which had obtained a footing on the African continent.

It is a far cry from the mountains of Northern India, where the remains of the Sivathere and its allies are entombed, to the pampas of the Argentine and Patagonia; but it is in the latter area where the next most important advances have been made in the palæontological history of the Ungulate Mammals. For these discoveries we are mainly indebted to Professor F. Ameghino, who has published the results of his observations in a serial[2] started during the past year. In these regions the extinct Ungulates seem to have run riot as regards strangeness of form and dissimilarity of structure to any of those found in the European area. Among the numerous types described in the various papers by Professor Ameghino, great interest attaches to the acquisition of the whole skeleton of an animal described some years ago by Professor Flower from the evidence of the

[2] *Revist. Argent. Hist. Nat.*, vol. i., 1891.

teeth and jaws alone under the name of *Homalodontotherium*. From the new discovery we now learn that this animal was a near ally of that peculiar aberrant Perissodactyle known since the date of Darwin's voyage as *Macrauchenia*. Very important information is also afforded as to the structure of those still more aberrant Ungulates known as *Nesodon*, which are nearly related to the huge *Toxodon*. It is now ascertained that the deciduous and permanent dentitions of these animals were very unlike one another; in consequence of which many species and even genera have been founded upon the remains of one and the same kind of creature. The most remarkable feature about the dentition of *Nesodon* is to be found in the circumstance that, while the first and second pairs of upper incisor teeth are very large and situated in the same transverse line, the third pair are small and placed in the antero-posterior line of the cheek-teeth, such a feature being quite unknown elsewhere among mammals.

We might go on almost indefinitely on the subject of these and other strange Ungulates from the Argentine, but we must pause to

FIG. 4.—Front view of mandibular symphysis and inner side of right mandibular ramus of *Homunculus patagonicus*—natural size.

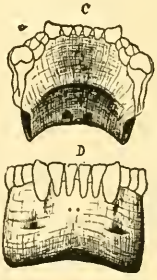


FIG. 5.—Inner (C) and front (D) views of mandibular symphysis of *Anthropops perfectus*—nat. size.

cali attention to the great event of the year in the mammalian palæontology of these regions. Most of our readers are probably aware that, with the exception of the Opossums of America, the Marsupials are confined to Australia and some of the neighbouring islands. Hitherto, no fossil Marsupials of Australian types have been found in Tertiary deposits of any other parts of the world. Now, however, we have the discovery in the Lower Tertiaries of Patagonia of remains of Marsupials nearly allied to the carnivorous Thylacine of Tasmania, and the Dasyures which inhabit both that island and the Australian continent. These have been described under the names of *Amphiproviverra* and *Prothylacinus*.

This remarkable and unexpected discovery must profoundly modify the views hitherto obtaining as to the former distribution of Marsupials; and, taken in connection with other circumstances, seems indicative of a former more or less continuous land-connection between the southern extremities of America, Africa, and Australia. Not less important is the connection exhibited by these Patagonian Marsupials with the primitive Tertiary Carnivores of North America and Europe termed Creodonts; and this leads Dr. Ameghino to conclude that the

Marsupials, Creodonts, and true Carnivores are closely related. We may add that the asserted occurrence by MM. Moreno and Mercerat of a large Monotreme in the Patagonian Tertiaries will, if confirmed, strongly support the connection of the South American Tertiary fauna with the existing fauna of Australia.

Another very important event of the year is the discovery of numerous remains of small monkeys in the Lower Tertiary—? Eocene—of Patagonia, which have been described by Professor Ameghino[3] as *Homunculus*, *Anthropops*, *Homocentrus*, and *Eudiastatus*. The mandibles of these (Figs. 4 and 5) show the three premolar teeth characteristic of the American monkeys, and apparently belong to the family Cebidæ; they are remarkable for the extreme uprightness of the chin. If the Patagonian strata be rightly correlated with the European Eocene, we have now evidence of the existence of Anthropoid Primates at a much earlier date than has hitherto been

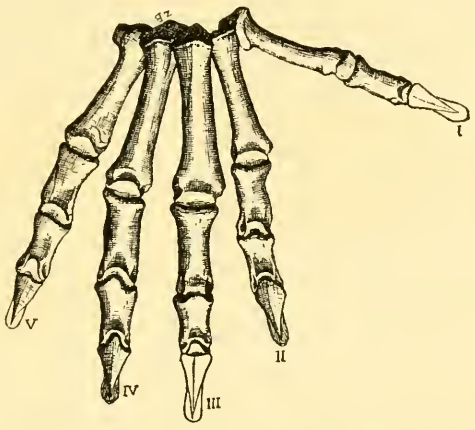


FIG. 6.—Skeleton of the right hind foot of *Icochilus robustus*—natural size.

recorded. Perhaps still more noteworthy are the signs of affinity exhibited by these early Primates to the extinct South American Protypotheriidæ. The latter are clearly related to the aberrant Ungulate *Typotherium* of the South American Tertiaries, which appears to be allied on the one hand to the extinct *Toxodon*, and on the other to the Rodents. *Protypotherium* and the allied *Icochilus* differ, however, by having all their teeth in contact, and by the terminal phalanges of the digits being expanded for flattened nails (Fig. 6) while both the thumb and great toe were opposable to the other digits. If substantiated, such an unexpected relationship as that of the American Primates to the Toxodonts will materially modify some of our present views as to the mutual relationships of mammals. We are indebted to Professor Ameghino for the figures of the remains of these monkeys and *Icochilus*.

[3] *Revist. Argent. Hist. Nat.*, vol. i., pp. 383–397, 1891.

We have yet another interesting discovery to chronicle before concluding our notice of the mammalian palæontology of South America. This relates to the remains of numerous small mammals presenting a more or less marked similarity to some found in the Secondary and Lower Eocene rocks of Europe and North America, and described as the Plagiaulacidæ. We may observe that these mammals (of which the Jurassic *Plagiaulax* and the Eocene *Neoplagiaulax* are well-known examples) are characterised by having a single large obliquely-grooved cutting tooth on either side of the lower jaw, which presents a considerable resemblance to the last lower premolar of the Rat-kangaroo (*Potorous*) of Australia, and also by having an equally large proclivous incisor on each side of the front of the same jaw. It was long considered that these Plagiaulacidæ were near allies of the kangaroos, but later observations have discredited this view, and have suggested that they were more nearly related to the Monotremes. Now, the South American forms (which have been imperfectly known to science for several years) also have a pair of procumbent incisors and a very large cutting premolar-like tooth in the lower jaw (Fig. 7) which may be vertically grooved; and it has

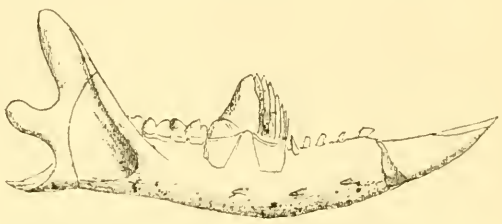


FIG. 7.—The right ramus of the mandible of *Abderites*, $\frac{2}{3}$ natural size.—After Ameghino.

accordingly been presumed that they were closely allied to the Plagiaulacidæ. The last-named tooth is, however, much larger in proportion to the others than is the case in the Plagiaulacidæ; while it is separated from the large incisor by five exceedingly minute, and evidently functionless teeth, which are very different from the two or three grooved premolars occupying the same position in the Plagiaulacidæ. Moreover, behind the large grooved tooth there are three molars, in lieu of the two of the latter. Dr. Ameghino considers that the large grooved tooth of these mammals, as represented by *Abderites* (Fig. 7), is the first molar, and not the fourth premolar; and he accordingly also regards the large secant tooth of the Plagiaulacidæ as a true molar. We are, however, by no means sure that the resemblance between the Abderitidæ and the Plagiaulacidæ is anything more than a superficial one; and, if this proves to be the case, the former family will indicate an entirely new group of mammals, of which the affinities cannot, at present, be even surmised. Whatever be the ultimate result, there can, however, be no sort of doubt as to the great interest attaching to these peculiar

South American mammals, which may some day, when their true affinities are known, afford important aid in constructing the table of mammalian genealogy.

That very similar structural peculiarities may arise separately in different groups is indicated by Dr. W. B. Scott's investigation into the phylogeny of the Camels and their allies;[4] where it is shown that the crescent-like (selenodont) grinding surfaces of the molar teeth of these animals have almost certainly been developed quite independently of those of the true Ruminants, which have a similar general structure. Indeed, it appears that we must go back as far as the small Eocene Ungulate known as *Dichobunus* (represented in North America by the closely allied, if not identical, *Homacodon*) before we reach what can be regarded as the common ancestral type of both the Camels and the true Ruminants.

That the evolution of the Camels took place in the New World has, for some years, been regarded as a fairly well ascertained fact, but it has been reserved for Dr. Scott to work out this phylogeny in a thoroughly satisfactory manner. It would be out of place to detail here the names of the various extinct genera connecting the modern Camels and Llamas with their primitive ancestral types; but we may observe that, as we descend the geological scale, we gradually find a less and less reduction in the number of the teeth below the normal type, accompanied by a steadily decreasing bodily size, till, in the American Miocene *Poëbrotherium*, we have an animal not much larger than a fox, with the full mammalian complement of teeth, and the metapodial bones of the feet not soldered together to form cannon-bones. Although this animal had acquired selenodont teeth, yet it is but a single step to a creature like *Dichobunus*, in which the molar teeth still retained the primitive hillock-like (bunodont) cusps of those of the pigs. Dr. Scott considers that towards the end of the Miocene epoch the ancestors of the modern Camels and Llamas respectively migrated to the Old World, and the southern half of the New; the earliest known Old World Camels being those of Northern India, which retain certain signs of kinship with the Llamas, which are lost in the living species.

The above appear to us to be some of the more striking results of the year's work in mammalogy; but there are, of course, many other topics of nearly or quite equal interest, which we are debarred from noticing by the necessary limits of our article.

R. Lydekker.

[4] On the Osteology of *Poëbrotherium;* a contribution to the Phylogeny of the Tylopoda. *Journ. Morphol.*, vol. v., pp. 1–78, pls. i.–iii., 1891.

III.

The Physical Features and Geology of Borneo.[1]

FROM the day when the companions of the ill-fated Magellan cast anchor before Brunei, in the early part of the sixteenth century, down to the present time, the island of Borneo has ever provoked a lively curiosity, and has been the subject of the wildest speculation. Its vast size and symmetrical position on the equatorial line early attracted the attention of geographers; but the exploration of the interior was long hindered by the trade policy of its first settlers, the Dutch East India Company, who deemed it expedient to confine their operations to the coast.

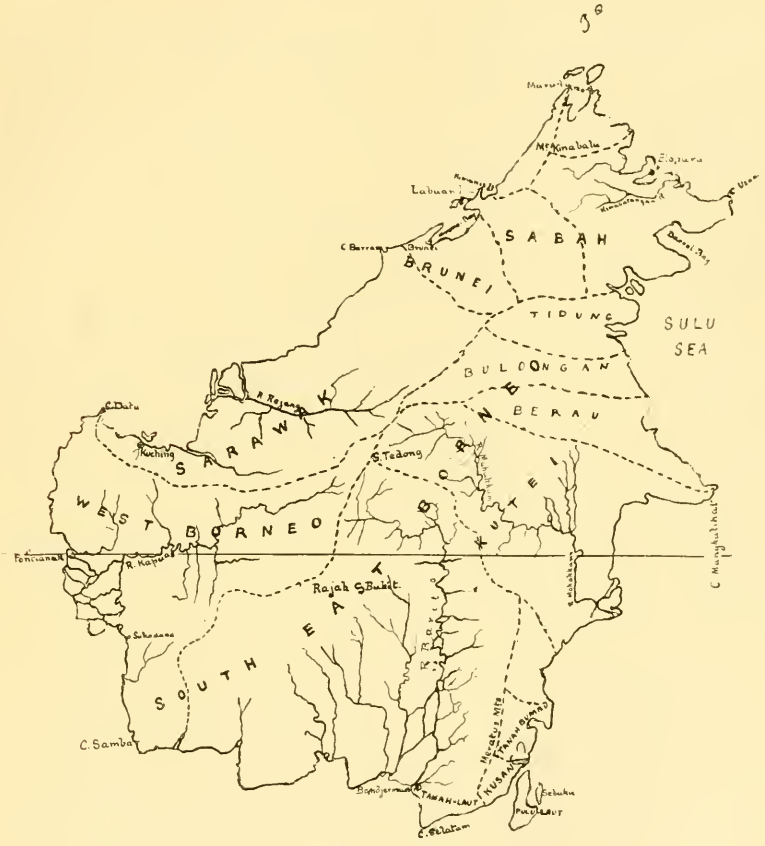
In more recent years, the popular imagination has been kindled by the romantic history of Sir James Brooke. That daring Englishman, arriving off the northern coast of Borneo, partly in search of adventure, partly with the idea of suppressing the Malay pirates who infested those seas, found himself, at the end of a few exciting years, installed as Rajah of Sarawak and ruler of a turbulent crowd of Malays, Dyaks, and Chinese. Strange rumours have also reached us of the ferocity of the head-hunting and cannibal tribes who share the forests of the interior with the orang-outang.[2] Lastly, the enterprise of the capitalist and the energies of the prospector have been stimulated by exaggerated reports of the wealth of the island in minerals and coal, of its wide-spreading gold-fields, and of its diamonds of fabulous size and unparalleled lustre. And yet, in spite of these manifold sources of interest, there is hardly another country of equal magnitude of which we know so little. It is true that the naturalist Wallace, in his "Malay Archipelago," draws a charming picture of the life and vegetation of the dense tropical forests of the island; but of its physical features we have, till recently, been in complete ignorance.

The vacuity of our knowledge of Borneo appears the more remarkable when we contrast it with the exactitude and minuteness of our information with regard to the neighbouring islands of Sumatra and Java; but the explanation is not far to seek. This is embraced in

[1] For the facts on which this essay is based, I have mainly relied on Dr. Theodor Posewitz's "Borneo" (Berlin: Friedländer und Sohn, 1889). I have recently translated this invaluable work, in which the whole of the literature (principally Dutch) has been admirably summarised; and it is with considerable satisfaction that I am able to announce the approaching publication of the English edition.—F. H. H.

[2] *See* Carl Bock's "Head-hunters of Borneo." London: 1881.

the physical and political conditions of the countries. The attenuated shape of Sumatra and Java makes it impossible to travel to any considerable distance from the coast; and they have been for many years under European rule. The configuration of Borneo is quite different. Its compact and rounded form measures 675 geographical miles, both from north to south and from east to west; while only the margin of its enormous area (219,034 square geographical miles)



Map of Borneo.

has been brought under the complete dominion of the Dutch and English.

The island has been traversed from coast to coast on more than one occasion by intrepid travellers, who, following one or other of the great rivers to its source, crossed the dividing range into the basin of another stream, which ultimately led them to the coast. But the main portion of the interior has remained, and, to a large extent, still is a *terra incognita*. The difficulties of transport through impenetrable forests, and over high mountain ranges, and the dangers to be apprehended from the hostility of the native tribes,

have combined to deter the scientific investigator from venturing far into the unknown and inhospitable regions of the interior.

In those districts where European settlements have long existed, for instance, in the districts of Bandjermassin and Tanah Laut in the south-eastern portion of the island, more exact details respecting its physical and geological structure have been obtained, for in these districts the country has been carefully examined in the eager search for useful minerals.

Much of the knowledge thus accumulated in the Dutch part of the island is due to the labours of the Natural History Commission, a scientific institution established in Batavia in the year 1820. Its members were scientific men, comprising, in the first instance, a zoologist, a botanist, and a geologist; and they were appointed for the purpose of investigating the resources of the as yet almost unknown island. At a later period, the geology of some portions of the island was elucidated by the work of the Batavian mining engineers, who were sent by the Dutch Government to search for useful minerals and for coal.

On the northern coast, our knowledge has received its greatest increment in Sarawak, by explorations carried on at the instigation of Rajah Brooke and his successor, Charles Johnson Brooke, and in Sabah, where the British rule has been inaugurated by the exertions of the North Borneo Company. Since 1881, when the company acquired its charter, our geographical and geological information of this part of the island has increased with every year. A band of courageous pioneers and explorers, defying every danger, have traversed these unknown regions in all directions, and scientific results of great value have been thus obtained.

Physical Features.

Proceeding from an elevated area in the centre, the radiating lines of water-parting divide the island into a southern, a northern, a western, and an eastern catchment-basin. The divides, however, are not, as usually represented on the maps, formed by continuous mountain chains, but consist of a series of short ranges and abrupt ridges, or of isolated groups of mountains and single peaks, which are turned in the same general direction. Surrounding the mountains is a low undulating country in which the separate and outlying masses lie like islands in a sea.

This structure is the dominant feature of the orographic systems of Borneo, and is cleverly epitomised by Posewitz when he names the detached portions of the high land "mountain-islands." The gently rolling country which laps round the foot of the "mountain-land" he describes as "hill-land." The hill-land, in its turn, is bordered by dry plains which are often of great extent, especially in South Borneo; and these, again, pass into the swamps and morasses of the coast. Like the mountain-land, the hill-land sends

out spurs into the plains, and tongues of the latter extend into the marshes.

According to Schwaner, the central mountain tract has a north-east and south-west strike, and culminates in two high peaks—the Gunong Tebang and Gunong Apo Borau, estimated at from 5,000 to 6,000 feet high. The four chief ranges that branch off from this *massif* extend towards the south-east, south-west, north-west and north-east, and constitute the chief political boundaries.

The south-eastern chain has a varied character. Its central portion consists principally of hill-land through which only isolated peaks (mountain-islands) project; but its northern and southern parts are more ruggedly developed, and steep ridges and rocky peaks rise to a considerable height. The southern limit of this chain is formed (in Tanah Laut) by the Meratus mountains, the highest peak of which is 4,250 feet above the sea-level.

The south-western chain extends from the central tract to near Cape Sambar, which is the extreme south-western point of the island. With the exception of a few hilly tracts in its central part, this chain retains throughout a rugged Alpine character. Its highest point is reached in the Rajah Bukit—the Olympus of the Dyaks—which is a peak of 7,000 to 8,000 feet. Schwaner describes a portion of the chain as consisting of a high plateau, 20 to 30 miles wide, the surface of which is broken by numerous dispersed peaks.

The northern chain, which comprises a north-eastern and a north-western branch, is less known than the southern chains. Its highest part is reached at its northern limit in the imposing *massif* of Kina-balu (13,698 feet); in its middle portion the peaks vary from 6,000 to 8,000 feet, and still further west, between Sarawak and West Borneo, they decrease to 2,000—3,000 feet. The western branch of the chain terminates in the Datu Mountains, spurs of which reach the sea, forming the promontories of Api and Datu. Isolated peaks of this part of the chain have an altitude of 6,000 feet.

In their general character, the northern chains are similar to the south-western chain, being composed of detached ridges and mountain-islands. Kina-balu is the highest mountain in Borneo, and is held in much veneration by the natives. This fine mass is situated in the territory of the British North Borneo Company, and forms a striking feature in the scenery of this part of Borneo. It consists of about ten peaks in a line, running east and west, while a solitary peak rises on the south, separated from the others by a deep and wide ravine. Several of its peaks have been ascended by English explorers. Spenser St. John describes the view from one of the summits as magnificent: the coast-line of North Borneo is visible as far as the island of Labuan, while to the south and south-east the eye ranges over a multitude of ridges and peaks of from 7,000 to 8,000 feet.

In the central mountains rise all the principal rivers—the

Barito, which flows towards the south, the Kapuas to the west, the Rejang to the north, and the Mahakkam to the east. The Barito is 570 miles long, according to Schwaner. Besides these large rivers, there is a great number of smaller streams, which rise in the different mountain chains.

The rivers of Borneo pass through three distinct phases in their passage from the mountains to the sea. In an *upper course* they rush over a rocky bed with cataracts and rapidly shifting pebble islands. The *middle course* is through a more level country, in which the stream is often replaced by a string of lakes. The extent of these lakes depends on the time of year; during the rainy season the surrounding forests are often flooded for miles, while in the dry season they partially dry up, leaving a black fertile land, dotted with small patches of water. Such, for instance, is the case in the rivers Barito and Kapuas, both of which have an extended lake district. Another feature of this part of the course is the formation of natural canals (*antassan*) which cut across the loops. Similar natural canals (*trussan*) connect neighbouring rivers with one another. They are formed by the floods that arise during the west monsoon, and are very serviceable as water-routes to the traveller.

In the *lower course*, the river winds slowly through a flat swampy country, fringed with mangroves, nipa-palms, and other tangled jungle growth. This lagoon district is inundated daily at high water, and is covered for months during the rains. On retreating, the subsiding waters leave behind a black mud, rich in humus and full of shells. A large amount of this mud is brought down by the rivers. Its accumulation causes the formation of numerous mud islands. Occasionally the accumulation takes place in such a manner as to form a natural embankment, through which the river flows at a higher level than the surrounding marsh-land. A part of the sediment is carried out to sea, where it is deposited in banks which often constitute dangerous bars across the mouths of the rivers. On the east coast there are mud banks extending for 8—10 nautical miles into the sea: even at a distance of 10 miles the sounding line only gives a depth of 33 feet.

The deposition of sedimentary material brought down by the rivers, protected in some cases by the presence of coral-reefs, causes a rapid extension of the coast. This is aided by the growth of the jungle vegetation. On the newly-formed marsh-land mangroves soon spring up, and by their pendent air-roots favour the accumulation of the mud. The forward movement of the mangrove woods has been estimated to amount to be more than a hundred metres in forty years. Another jungle tree which acts in a similar manner is the nipa-palm. Flourishing alike in salt or fresh water, its matted roots serve admirably to retain the flotsam of the rivers; and it is propagated rapidly by its angular fruit, which, on falling, sinks easily into the soft mud. On the coast of Sarawak the annual increase is estimated at

15—18 feet. The formation of new land is further aided in South Borneo (Tanah Laut) by the deposition of the fine mud brought down by the river flowing through the gold-washing districts.

Geology.

Posewitz treats the geology of the island under four principal heads:—(1) The mountain-land; (2) the Tertiary hill-land; (3) the diluvium, or drift of the plains; (4) the alluvium of the marshes.

The mountain-land consists partly of crystalline schists, with which old eruptive rocks (granites, diorites, gabbros, and serpentines) are associated, partly of a slate-formation which appears to be of Devonian age. So little is known of the geology of the mountain-land, that the schists have not been separated from the slates, and even the Devonian age of the latter is only problematical. The slate-formation comprises phyllites, sandstones, conglomerates, and quartzites. The Devonian (?) rocks are in places overlain by a Carboniferous formation consisting of a hard bluish limestone (Carboniferous Limestone[3]), succeeded by coarse white sandstones. This Carboniferous formation is largely developed in North Borneo, where it appears to extend from Sarawak to the Bay of Marudu. It has not been separated from the underlying slates, but is clearly marked off from the Tertiary beds that succeed it.

The presence of Cretaceous rocks in Borneo has been proved by the discovery by Van Schelle, in West Borneo, of fossils which have been referred by Geinitz to the Upper Chalk. This is the first recognition of the presence of Mesozoic rocks in the Indian Archipelago. How far they extend is at present unknown, but recent researches of Martin[4] appear to show that rocks of this age have a wide range in Borneo.

The Tertiary hill-land forms a broad belt round the mountain-land. It consists of a gently rolling country, which near the mountain border rises into hills of 200—300 feet (Eocene), but elsewhere dies away into the common level of the plains (Miocene).

Verbeek divides the Eocene of Borneo into three stages, namely: α, the sandstone stage; β, the marl stage; and γ, the limestone stage.

The sandstone stage (α) is the lowest, and, from a practical point of view, the most important, since it contains the Borneo black coal. It consists of alternating beds of sandstone shales, carbonaceous shales, and coal-seams.

The marl stage (β) comprises shales and marls, some of which are very fossiliferous, containing numerous remains of Crustaceans. One whitish-grey bed of marly limestone is packed with Orbitoides and Nummulites.

[3] Tenison Woods has described the following fossils, derived from this formation in Sarawak: a *Vertebraria*, *Phyllotheca australis*, a *Fenestella*, and a *Stenopora*.

[4] See *Nature*, 1889, p. 121.

The limestone stage (γ) consists of a hard white or bluish limestone, containing numerous fossils (Corals, Sea-urchins, Lamellibranchs, Gasteropods, Orbitoides and Nummulites). This stage appears to be the equivalent of the Nummulitic Limestone of Europe.

Schwaner describes it in the following terms: "The limestones possess the undoubted characteristics of a littoral formation: they consist of enormous aggregates of coral and broken shells, intermingled with the spines of echinoderms; and resting on a bed of hard rock, they follow the boundaries of the high ground, into its bay-like inlets and round its projecting promontories."

Space does not permit me to give details as to the development and distribution of these important Eocene deposits, nor to describe their fauna and flora.[5] Suffice it to say that stage α has been found to reach a thickness of 524 feet; stage β, to 820 feet; and stage γ, to 295 feet. With regard to the fauna, eighteen species of Gasteropods have been determined, all of which are marine types. They indicate a tropical climate, the majority of the species having their nearest representatives in the present fauna of the Indian Archipelago. The Lamellibranchs are better represented than the Gasteropods, and comprise an abundance of characteristic marine forms, indicating both deep and shallow seas. The Nummulites belong to four different species, two of which are peculiar to Borneo (*N. pengaronensis*, Verb.; *N. sub-brongniarti*, Verb.; *N. biaritzensis*, d'Arch.; *N. striata*, d'Orb. var.). The coral fauna of the Nummulite beds consist of species peculiar to Borneo (belonging to the Turbinolidæ, Stylophorinæ, Astræidæ, Madreporidæ, and the Poritidæ) and are of a reef-forming type. The flora has an Indian character, showing a remarkable resemblance to types now living in the East Indies.[6]

The Eocene strata have been pierced in numerous places by the eruption of andesitic lavas. These lavas are bedded, and are accompanied by tuffs. Since they have broken through the highest beds of the limestone stage, they are probably of Miocene age. Above the andesites lies a series of shales and sandstones, which Verbeek describes as Late Tertiary (Miocene). The lower beds consist of soft shales and harder marls. The greenish sandstones which lie above them have, in great measure, derived their material from the andesites and tuff. These Miocene beds contain occasional seams of brown coal, which is very inferior as fuel to the black coal of the Eocene beds.

[5] An admirable summary, with full reference to the literature, will be found in Posewitz's "Borneo."

[6] "The vegetation of the Sunda Islands has retained its Indian character, from the Eocene period down to the present time, although the Tertiary floras of Europe have been considerably modified." Geyler, *Jaarb. v. h. Mynwezen in Nederl.-Ind.* 1879. II.

The diluvium (drift) of the plains occurs chiefly as a zone round the Tertiary hill-land, though it also covers the flanks of the mountains. It is of great practical importance, as it contains the chief deposits of gold, platinum, and diamonds. It may be described as solid flat land in contradistinction to the marsh-, hill-, and mountain-land. Its composition is fairly uniform, its upper part consisting of clays and sandy clays, its lower of pebble beds. It is between the pebbles of the latter that the precious metals and stones are found, mixed with a fine clayey earth. In some places the pebbles are united by a siliceous cement to a hard conglomerate.

The alluvium of the marshes has a wide distribution in Borneo. Its mode of occurrence has been already referred to. The marsh-land rises very slowly from the coast. This is shown in the lower course of the rivers by the great distance from the mouth at which the influence of the tides is still perceptible. The Barito, for instance, is tidal for fifteen geographical miles, while in certain seasons the influence of the tides may be observed at a distance of thirty-five geographical miles. It is also shown by the immense area covered by the diurnal and periodic floods. The area flooded daily in the basin of the Barito is estimated by Schwaner at 160 square geographical miles, or one-twelfth of the whole river-basin, to which 420 square miles must be added in the rainy season. Consequently, during the west monsoon, 580 square geographical miles, or more than one-third of the river basin, are under water.

With regard to the geological evolution of Borneo, Posewitz's ideas are not altogether in unison with those that have been expressed by Wallace in respect to the whole of the Malay Archipelago. While Wallace regards these islands as having been produced by the breaking up of a continental area, Posewitz describes Borneo as resulting from the fusion of an archipelago of small islands. The grouping of the different island-clusters in this archipelago has been preserved in the main features of the present structure. A narrow island, which extended from north-east to south-west, is now the Tanah Laut range. Parallel to it was the mountainous island of Pulu Laut. The Pramassan Alai and Amandit mountains were represented by islands of the same general type. To the north there extended a broad sea, lapping round the foot of the central mountains. A few peaks rose from its surface, marking the direction of the south-east mountain chain. In the south-west was a large cluster of small islands, which are now the mountains of Sukadana and South-West Borneo. A large island occupied the position of the present Chinese districts, and from it a chain of islands extended to the great mass of Kina-balu in the north-east point of Borneo.

In the beginning of the Tertiary period the seas surrounding these islands began to be filled in by the deposition of sedimentary matter. Borneo now acquired a shape resembling that of the neighbouring island of Celebes. Great gulfs and arms of the sea, fringed

by coral reefs, occupied the place of the low-lying plains of the Borneo of to-day.

At the close of the Tertiary period, the gulfs gave way slowly to dry land; the seas became shallower and retreated, thus preparing the way for the advent of the present period. Numerous and powerful streams carved out a passage from the mountains and flowed towards the retiring seas, bringing with them their quota of mud and fine sand with which to increase the extent of the growing island. Thenceforth, down to the present time, no important changes have modified the physical features of Borneo; and the island has enjoyed a remarkable immunity from the fierce paroxysms of volcanic activity that, during the Recent period, have convulsed the other large islands of the Malay Archipelago, and culminated, in the year 1883, in the extraordinary outburst of Krakatoa.

F. H. Hatch.

IV.

Great Lakes.

MUCH has been written on the origin of lakes, and on the character of their inhabitants; but the literature of the subject is buried in so many monographs and scattered papers that it may be useful to linger for a moment to review the present state of our knowledge. Especially is this advisable when we bear in mind the rapid advance in geographical and geological exploration which has taken place in the past decade. It should also be remembered that no one branch of Natural Science can stand alone; each in turn can aid and advance the others. If we wish to understand the fauna and flora which inhabit a lake, it is necessary to learn how that lake originated, and what was the date of its origin. The inhabitants of each existing lake do not include merely such plants and animals as, reasoning from analogy, we should say were most fitted for it. They are always mingled with certain forms which bring before one's mind the bygone history of the lake. The peculiar species may be allied to forms usually confined to salt water; or they may be species incapable of migrating across barriers of land, though now found in several distinct basins. We may find lingering in the depths of certain lakes species which point to extensive climatic changes since the lakes were first inhabited. This evidence of climatic change may also help us to understand why certain species are present, and others, seemingly equally fitted to inhabit the lake, are now absent.

Even a small lake must be studied from many points of view, if we wish to obtain anything beyond the merest catalogue of its inhabitants. Still more is this necessary when we deal with the vast sheets of fresh water which are found in the interior of our continents; for they, like the large oceanic islands, may contain lingerers of antique types, now lost in regions less isolated. We will attempt, therefore, in the following pages to show how lakes originate; and we will give, also, a short description of the great lakes now existing. It must be pointed out, however, that we know little as yet about the origin of great lakes, and the natural history of the most important group is still practically unknown. These notes were partly written to point out this deficiency in our knowledge, and if they help to stir up naturalists to a more thorough study of the Great Lakes of Central Africa, one of their principal ends will be attained.

The regions occupied at the present day by great freshwater lakes

are only two. There is the Central African region, with Victoria Nyanza and other equatorial sheets; and there is the somewhat similar cluster in North America, including Lake Superior. Another group once occupied most of the Mediterranean region and extended far into Asia; it is now represented by the Caspian Sea and other more or less isolated sheets of brackish water, but the western part has again become connected with the ocean and has lost its peculiar fauna. A fourth district to which we shall refer is now the arid region, or Great Basin, of North America.[1] This country was occupied not long since, geologically speaking, by magnificent inland seas, which now are represented by a few shrunken and saline remnants, such as the Great Salt Lake. No doubt other extensive lakes can be recognised by studying earlier geological history, but our present purpose is not to trace the former recurrence of lacustrine conditions; it is rather to study the history of the two great groups of freshwater lakes which still exist.

It is evident that, for the purpose of study, it will be most convenient to select lakes which still remain, but which formerly had a greater extent, and have thus left their ancient history written in the marginal sediments now accessible to our exploration. The two regions which most thoroughly answer to these conditions have been carefully studied, and they represent two distinct types. The Mediterranean has been alternately connected with the ocean or converted into a chain of brackish-water lakes. The Great Basin of North America, on the other hand, has contained lakes changing often in size, sometimes becoming almost dried up, sometimes filled to overflowing, but never forming part of the ocean. Thus we find that the lakes of the Mediterranean basin, even when nearly fresh, contain seals and other marine animals; while the Great Basin is characterised, and apparently always has been characterised, by the poverty of its fauna and the absence of marine forms.

Let us now trace the past history of these two continental basins, as far as our knowledge will permit us. We shall then be in a position to understand the probable mode of origin of the great lakes of Africa and America. We shall then also be able to realise the bygone conditions which led to the formation of such vast sheets of fresh water, and to their peopling with the animals and plants that now inhabit them.

In the long-settled and carefully-studied region between Europe and Africa, we find, at the present day, the western part occupied by the extensive inland sea, known as the Mediterranean. To the east, this is connected with other seas somewhat less salt, known as the Black Sea and the Sea of Azov. Still further east we find the isolated Caspian Sea and Sea of Aral. There may not appear at

[1] The maps illustrating this paper are all drawn to the same scale, so as to indicate the relative importance of the Basins. The figures on the lakes show the height of their surface above the sea.

first sight to be any resemblance between the isolated lakes and seas of western Asia and the Mediterranean; but when we trace back the geological history of the basin to early Pliocene times we meet with quite different conditions. The commencement of the Pliocene period was heralded in the Old World by the formation of brackish-water lakes on a scale unknown at the present day. *Congeria*-beds—so called from the numerous species of the brackish-water mollusc *Congeria* (or *Dreissena*) found in them—extend from Spain eastward into Asia. They are the deposits of the large lakes which in early Pliocene times seem to have occupied most of the Mediterranean basin. The Mediterranean does not seem ever to have formed a single great lake, for at various times during the Tertiary period there was apparently a connection between Europe and Africa, not only at the Strait of Gibraltar, but also across the ridge near Sicily and Malta.

If the movement which transformed the Miocene sea of the Mediterranean basin into a chain of brackish-water lakes had continued, and the barriers at the Strait of Gibraltar and the Isthmus of Suez had remained unbroken, we should have expected the lakes gradually to become smaller and smaller and more and more salt, as long as the inflow was less than the evaporation. But if the rainfall and inflow from rivers exceeded the evaporation, the lakes must increase, and find outlet across the barrier. Thus the sea would become gradually transformed into a series of freshwater lakes, like those of Central Africa. As far, however, as we can judge, the Mediterranean never reached this last stage, for after long ages of brackish-water conditions, and the differentiation of a certain number of peculiar species, the sea again broke in, and a copious marine fauna re-conquered its ancient territory. Thus the Mediterranean region is occupied by basins which may at any time easily be transformed into large freshwater lakes, though, as it happens, the sea has broken in on several occasions when the lakes were gradually forming. The continuity of the process was thereby stopped before the great sheets had become thoroughly fresh, and before a large lacustrine fauna had appeared.

Turning now to the second region of extinct lakes to which we have alluded, we find in the Great Basin of North America a district of which the geology has been worked out quite recently by the Geological Survey of the United States. It would be impossible in a few words to do justice to the valuable monographs by Messrs. G. K. Gilbert and I. C. Russell, but their main conclusions are these. An enormous area in Nevada, Utah, Oregon, and California forms an enclosed basin, now occupied by deserts and by salt lakes with no outlet towards the sea. The basin seems to have originated in late Tertiary times, through irregular earth movements which raised certain tracts and depressed others. The region was apparently altogether above the sea-level when the formation of the

basin took place, or at any rate was saved from the influx of the sea by higher land. We find, therefore, no evidence that the sea ever penetrated into the region after it became a basin, and there is no evidence of the former existence within it of a marine fauna.

Though the sea never penetrated into the Great Basin, there was formerly a much heavier rainfall; so that instead of the existing deserts and salt-lakes, there were large sheets of fresh water, comparable in size with the Great Lakes on the other side of the continent, and, like them, overflowing towards the sea.

It is a most interesting region to the geologist, for the Great Basin well illustrates the formation of lakes through irregular earth-

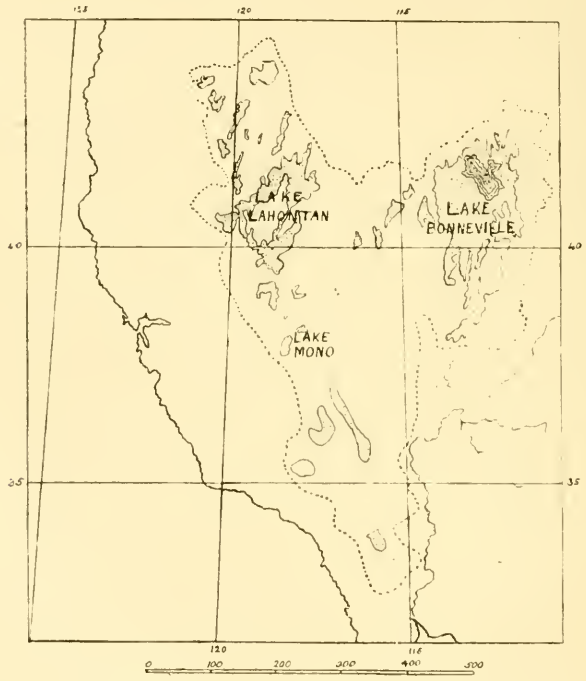


FIG. 1.—The Great Basin and its extinct Lakes. After Gilbert and Russell.

movements. The abandoned beach and shore-lines of the extinct lakes, as Mr. Gilbert has clearly shown, are tilted and deformed in a most remarkable fashion.

The aquatic fauna of the Great Basin, as we should expect in an isolated region such as this, where lakes expand and contract, and their waters change constantly in chemical composition, is an extremely poor one, and seems always to have been poor. From the point of view of the naturalist, these nearly extinct lakes are, therefore, of little importance, for their continuous isolation and the unfavourable conditions have prevented the appearance of a lacustrine fauna and flora of any extent.

The Great Lakes of Canada and the United States form a group too familiar to need description. Their date and mode of origin are as yet unknown, for the glaciation which so influenced the temperate parts of the northern hemisphere has swept away most of the old lacustrine deposits in which was written the ancient history of these lakes. The same glaciation, or the arctic climate that accompanied it, has obliterated the old lacustrine fauna and flora, if a peculiar one ever existed. We thus find that at the present day the inhabitants of the Great Lakes consist merely of the ordinary lacustrine species of North America, with the admixture of a few forms perhaps of marine origin. Too short a time has yet elapsed since the passing away of the arctic climate for a special lacustrine fauna and flora to be evolved in this region, or apparently in any other of the areas which felt the rigour of the cold during the Glacial epoch.

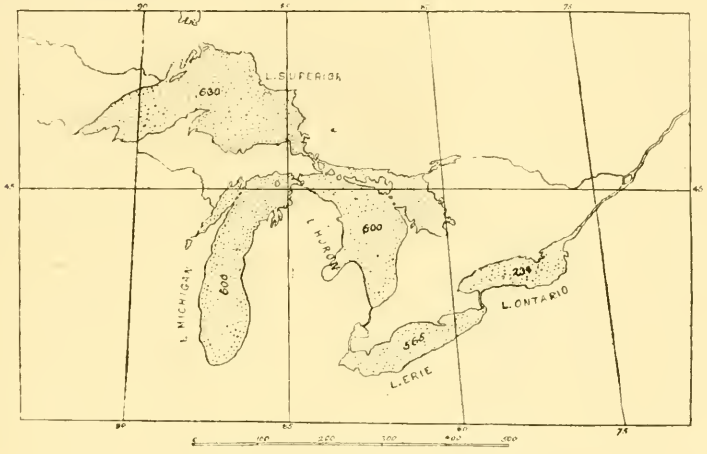


FIG. 2.—The Great Lakes of North America.

From what we have now learnt of the sweeping climatic changes that occurred during the Pleistocene period in temperate regions, it is evident that only in tropical or sub-tropical countries can we hope to find a well-marked and peculiar fauna and flora in each detached basin. Lakes are like oceanic islands. If their fauna and flora has recently been obliterated, they become peopled from the nearest land and will contain no endemic species. Such is the case with the Faröe Islands and with the lakes of Cumberland. If, however, there has been no break in the continuity during long periods, the result is quite different. Accidental introductions will have little chance of establishing themselves in a country already occupied, and each island of a small archipelago, like the Madeira or Galapagos, will contain peculiar species.

The same rule will probably apply to tropical lakes; but, unfortunately, one group only, and that almost unexplored, seems to fulfil the necessary conditions of long-continued isolation and absence

of violent changes. No extensive freshwater lakes are to be found within the warmer regions of the globe, except in Central Africa, and to these we must turn if we wish to study the effects of long-continued and slow change on a lacustrine fauna.

So little is yet known as to the natural history of the equatorial lakes of Central Africa, or of the Tertiary geology of the district, that one cannot speak confidently as to the process of their formation.

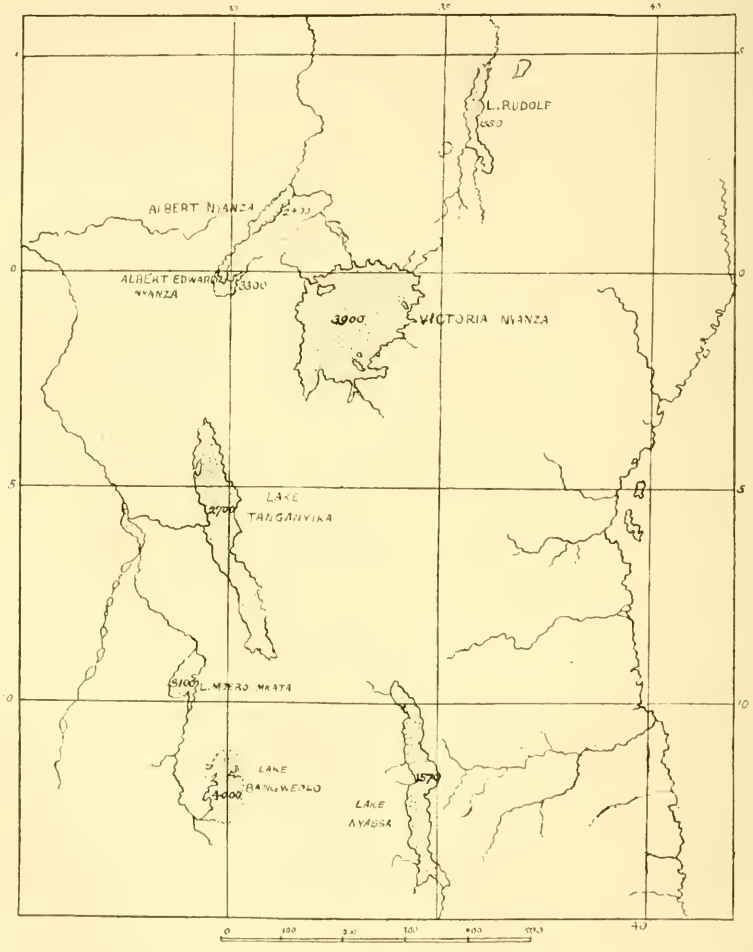


FIG. 3.—The Great Lakes of Central Africa.

Still, however, we have reason to believe that their history has been like that of the Mediterranean lakes before alluded to, except—and the exception is important—that the longer continuance, or greater energy of the uplift in Central Africa, has raised the sea-bottom high above its ancient level and has gradually transformed portions of its area into freshwater lakes.

It will, perhaps, be asked how it is that, notwithstanding our confessed ignorance of the African lakes and their inhabitants, we venture to express any opinion as to their former connection with the sea. Though not much is yet known as to the fauna of these lakes, we are not entirely without information. From the slightly brackish Lake Tanganyika, Mr. Edgar A. Smith has recorded thirty-two species of mollusca, nineteen of which are peculiar to that lake. Among them are two species of a remarkable genus closely allied to the marine *Trochus*, and one or two other shells seem also to suggest marine rather than freshwater ancestors. Lake Tanganyika is now 2,700 feet above the sea-level, but the occurrence in its waters of modified marine mollusca points to the probable elevation of this part of Africa to at least that extent. Till their fauna is better known, we cannot speak confidently as to the origin of the Central African lakes.

The decision of the question How did the African lakes originate? ought to help us to understand various peculiar problems in geographical distribution presented by that continent. Lake Tanganyika, though nearer to the east than to the west coast, overflows into the Congo; the Victoria Nyanza and the associated lakes are connected with the Nile; Lake Nyassa's outlet is towards the Indian Ocean. Thus the African lakes belong to three distinct basins, each probably possessing a peculiar fauna. Most of the great lakes are so near to the watershed that if the higher ones were connected with the sea there might exist an equatorial Mediterranean entirely cutting off South Africa and Madagascar from the rest of the continent. Much has still to be learnt about Africa, and the African lakes evidently contain the answer to many riddles.

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CLEMENT REID.

V.

Life-Zones in Lower Palæozoic Rocks.

THE "Records of the Rocks," which had been assiduously collected for some time past, were first arranged in definite order by William Smith during the close of the last century and the commencement of the present one. It is well known that Smith's work gave an impetus to the study of Stratigraphical Geology, which resulted in the establishment of a chronological sequence of strata in many areas during the early decades of this century. It is a tribute to the acumen of British geologists that many of the terms which they adopted for the major subdivisions of the strata are now in general use in all parts of the world. But the work of correlation, so enthusiastically commenced, received a check. Dr. Whewell and Mr. Herbert Spencer questioned the possibility of assigning the same age to similar deposits in remote areas, and the difficulties of the task were forcibly presented to geologists by Professor Huxley in his Presidential Address to the Geological Society in 1862. Some time previously to this, M. Barrande (1) had called attention to the apparently anomalous distribution of organisms in the rocks of the Lower Palæozoic Basin of Bohemia, on which was founded his well-known doctrine of "Colonies," and in later publications he instanced a large number of re-appearances of organisms in the rocks of the same basin, which he cited in support of his doctrine. Correlation of strata was, after this, looked upon with considerable suspicion. Nor is this to be regretted, for one result was the diversion of the energies of many most able workers into another field, that of petrological research, which has of late years yielded so many brilliant results.

The then comparatively little known Lower Palæozoic Rocks were, perhaps, the special objects of distrust at the time of which I have been speaking, for it was in these rocks that the colonies were asserted to exist, and subsequent research brought to light a number of other cases of which the once-accepted explanation was strongly adverse to successful correlation. The theory of colonies has now but few supporters, and many of the other apparent cases of anomalous vertical distribution of organisms have been proved to be due to errors of interpretation.

A new era in the study of rocks of greatly disturbed districts was inaugurated by the publication, in 1878, of Professor Lapworth's remarkable paper on "The Moffat Series," (2) in which the method

which that author has called the *zonal* (as opposed to the *regional*), was first applied to the Lower Palæozoic Strata in a detailed manner.

The use of two words has probably done much to retard the general acceptance of this "zonal method;" these words are *zone* and *species*. The physical geologist is naturally alarmed by the use of the term zone, and the biologist by that of species. We may pause for a moment to consider these words.

The adoption of the word zone—a girdle—is unfortunate. Small wonder if there are some who have looked askance at the rapidity with which thin zones were identified in distant regions, and have feared that the task of the modern stratigrapher was akin to that of Puck when he said:—

> "I'll put a girdle round about the earth
> In forty minutes."

It will presently be shown that the fear is groundless, for the zones are in reality far from being universally distributed. They are in no sense girdles.

The biologist justly asks how we can tell that a form is a distinct species, when the soft parts are not preserved. May it not be merely a variety? Certainly it may be, but it is of value to the stratigrapher for all that. There are two kinds of variety. Firstly, the departure from the normal form, which co-exists with that form; the number of variations of form presented by the shells of *Terebratula biplicata* in the Cambridge Greensand illustrates this; such variations, though interesting to the biologist, are useless to the stratigraphical geologist for the purpose of defining zones. The other variations are found, by experience in the field, to mark successive periods of time. A palæontologist whose sphere of work was limited to the museum, would see nothing by which he could separate these varieties from the former, but they are all-important to the stratigrapher, and it matters not to him whether they are true species or merely varieties of an earlier species, for they mark definite horizons. Dr. W. T. Blanford some time ago remarked to me that a distinctive term was wanted for such a variety, and I would suggest the term Exallage (ἐξαλλαγη, a variety "from what has previously been").

We may now proceed to consider the manner in which life-zones have been established amongst the strata, and to defend their utility. Though many sets of life-zones have been established, as for instance in the Upper Cretaceous Rocks by the use of echinoids and other forms of life, and in the Jurassic Rocks mainly by the use of Ammonites, I shall refer chiefly to the graptolite-zones of the Lower Palæozoic Rocks, as my practical knowledge of zones is chiefly confined to those of the last-named strata.

Let us consider first the relative value of different groups of organisms in working out zones. Hitherto special use has been made of the graptolites, and they are, when found, extremely valuable on account of their abundance and the frequency with which they are

preserved in a perfect or nearly perfect condition; but they cannot always be obtained, and in such cases corals, brachiopods, trilobites, and other forms will be found serviceable. The trilobites have already been extensively utilised by the Scandinavian and Russian geologists, and they will probably yield more valuable assistance than heretofore in our own country when they have been more fully studied with reference to their distribution in the field. Some of our "species," such as *Cheirurus bimucronatus*, probably contain many "exallogous" forms which are of value for stratigraphical purposes.

The actual utility of different forms of fossils for determining horizons can only be ascertained by working out their vertical ranges in the strata. It is a well-known fact that some fossils have a longer range than others which are closely allied to them; for instance, in the Llandovery Rocks of the Lake District, *Monograptus gregarius* ranges through more than thirty feet of rock, while *Monograptus argenteus* is limited to eight inches of the same set of strata.

When an organism has a long range it by no means follows that it will be found equally distributed throughout all the strata included in its range, and so we get re-appearances of a form. Among the many cases of re-appearance noted by Barrande among the rocks of the Bohemian Basin (3), two of the most noteworthy are those of the trilobites *Æglina rediviva* and *Dionide formosa*, which have been found in the bands *d* 1, *d* 3, and *d* 5, but have not been recorded from the intermediate bands *d* 2 and *d* 4. It is not necessary to suppose that these creatures migrated from the Bohemian area during the deposition of the bands *d* 2 and *d* 4. If they lingered in diminished numbers in the area during a period marked by conditions unfavourable for their existence, they might well escape the notice of collectors.[1] It is evident that recurring forms are not adapted for marking zones. They do not, however, raise any real difficulty when zones are being established, for they are not usually prominent forms, and, what is of more importance, the association of other organisms is different in the beds in which a particular organism recurs, from that which is found in the beds in which it first appears. From this consideration, it follows that a successful establishment of zones will more probably be made if the organisms associated with the form after which the zone is named be also carefully noted, and this is constantly done by those who use the "zonal method."

Another possible cause of suspicion is the frequent thinness of the deposit marking each zone in the area in which such zones were first determined. It is, however, natural that such determinations should be made in areas of exceptionally thin deposits, for there many zones may be studied in the course of a single section; and it is not only easier to establish zones in such an area, but when a thick

[1] In illustration of this I may notice that during the summer of 1876 the butterfly *Erebia cassiope* swarmed on many of the hills of the Lake District. Since that time I have not seen one, though I have usually spent my summers in the district.

group of deposits is studied, there is a tendency to speak rather of series or stages than of zones. Nevertheless, the term is not applied merely to thin deposits, but also to groups of considerable thickness, when a particular form is characteristic of a group of strata. Professor Lapworth, when commenting upon the thinness of the Stockdale shales (4) (divisible into nine graptolitic zones), pointed out "that they were represented by very great thicknesses of deposit elsewhere; thus, the Browgills were represented by thousands of feet in the Bala group and the Tarannon, and the Skelgills by enormous thicknesses in Girvan and Central Wales." The various zones of Llandovery-Tarannon age are traceable also in these expanded deposits, and there each zone is of very considerable thickness. I have elsewhere spoken of the Lower Ludlow Rocks of Bohemia as the zone of *Monograptus colonus*, and there is no reason why the term should not be applied to those beds, though, doubtless, a more intimate acquaintance would show that they might be split into smaller zones or sub-zones. The beds of this age in Bohemia are not very much thicker than some of the recognised graptolite zones of typical areas in Britain; but in the English Lake district we find that these Lower Ludlow Rocks (*i.e.*, the zone of *Monograptus colonus*) have an estimated thickness of about 10,000 feet, and although this may be an exaggeration, there is no doubt that they must be put down at many thousands of feet.

The above remarks lead us to consider the lateral extension of zones as hitherto traced. Using the term zone in the widest sense in which it has been applied, we find zones characterised by a particular genus having a very extended geographical distribution. The zone of *Dictyograptus* has been recognised in Britain, Belgium, Russia, Scandinavia, and America; that of *Bryograptus* in Britain, Scandinavia, and America, and that of *Phyllograptus* in these three countries, as well as in Australia. There seems no reason why strata characterised by particular *genera* should not be spoken of as zones, and indeed we have long been accustomed to speak of the zone of *Micrasters*, and, more recently, of the *Olenellus* zone. At present, however, we may confine our attention to such zones as are characterised by the existence of forms which are usually spoken of as species. These zones vary in different ways, when traced laterally. It has already been pointed out that they frequently expand considerably when followed from one area to another, and, on the other hand, they thin out to such an extent as sometimes to disappear altogether. Thus the *Rastrites maximus* zone of the Stockdale shales is found near Sedbergh, but does not occur in the Lake district, and study of the deposits points to the conclusion that it has actually thinned out, and is not represented by any sediment. In such a case the absence of sediment in an area during the time that the deposits of a zone are being laid down elsewhere, will result in the production of a plane of stratification, marking the position of

the zone. According to Professor Lapworth (5), the fossiliferous zones of the Moffat series "disappear one by one from above as we pass over the Uplands from south-east to north-west." Furthermore, the sediments of a zone may vary considerably when traced laterally, whilst the fossils remain the same. The graptolites of the Lower Ludlow beds are similar in the limestones of Bohemia, the shales of Scandinavia, and the grits of the north of England. At other times, both the lithological characters and the organic contents of corresponding strata are different in two areas. In such cases, as a rule, only general comparisons can be made, and a minute zonal subdivision of the strata cannot be attempted.

The most rigid test to apply to the zonal method, if we wish to ascertain its value, is the examination of the fossils of zones, with a view to discovering whether there is not an inversion of the order of some of the zones in different areas. So far as I am aware, the extensive exploration that has been carried on amongst the Lower Palæozoic Rocks has hitherto brought to light no case of the inversion of order of two zones in any two areas. It would be tedious to give instances of the constancy of order of such zones, but they may be readily found by searching the papers which, during the last fifteen years, have been devoted to the distribution of the graptolites. It is difficult to see how, on any view save that of the contemporaneity of the strata, marked by the inclusion of the same fossils, we are to account for this absence of inversion. It might be produced by the successive zone-forms originating in one, and only one, area, and spreading away from thence; but such a supposition, though perhaps just conceivable, is in the highest degree improbable, and viewing all the evidence, not of the graptolites only, but of other forms also, may be rejected. But all difficulties vanish if we allow sufficient *time* for the accumulation of the strata. Surely the time taken for the spread of an organism by transport or migration would usually be short, as compared with the time during which that particular form existed. Where a species is limited to a few feet of strata, the time during which that species spread may be represented by less than an inch of deposit. In this case, the zones, though not actually begun and ended at the same time, are for all practical purposes contemporaneous. We should speak of a man who was born in 1800, and who died in 1870, as the contemporary of one who was ushered into the world in 1801, and left it in 1871; and in a science like geology, where accurate estimates of time are at present impossible, and seem likely always to remain so, a similar use of language is fully justifiable. If the value of zones depends on the comparative rapidity of dispersal of organisms, a group of organisms should be of more or less value according to its capacity for rapid dispersal; and if this be so, some forms should be more useful than others in marking zones. It is very probable that such freely floating creatures as graptolites were more easily dispersed from their centres

of origin than were animals of other groups, but at present we cannot say much upon this head, for we do not know how far other groups will yield "exallogous" forms, after a further study of the vertical distribution of the "species."

Hitherto I have made no attempt to give a definition of a zone. It is advisable, as in the case of other stratigraphical divisions, not to insist on too rigid a definition. A zone may be defined as a *stratum or set of strata characterised by an organism, which may or may not be the dominant form of that zone, but which is nearly or entirely confined to it, and from which it takes its name; it usually contains an assemblage of other forms occurring in similar association in that zone only; it may be of considerable thickness, and may contain material of different composition in different parts of its extent.* Such a definition permits the use of the term for deposits characterised by genera, species, or even varieties. Some authors speak of a zone when the form after which the zone is named is absent. It is an open question how far this is desirable, but in practice little harm can result, if the absence of the characteristic fossil is noted, and the evidence for the identification of the zone is fully given. It is clear that the assemblage of fossils other than the characteristic one will not be constant in all areas in which the zone is found, so that the stratigraphical geologist must necessarily exercise his judgment in determining how far it is safe to subdivide a group of deposits into zones, and to correlate these with one another in different areas. The success of such correlation will depend upon the cumulative evidence.

I shall devote the latter part of this article to a discussion of the value of the zonal method to the geologist. It is above all things of value as furnishing a chronological index, by which we may fix the relative dates of the different events, which taken together constitute the earth's history. As Sir A. Geikie observes (6), stratigraphical geology "gathers up the sum of all that is made known by the other branches of the science, and makes it subservient to the interpretation of the geological history of the earth." Now it is impossible to read that history aright, unless we have a trustworthy chronological index, and this consideration probably influenced those who, alarmed by the jeremiads of eminent men of science, abandoned the study of stratigraphical geology, to pursue the subject along what seemed to be more promising lines. It is indeed difficult to see what would be the ultimate value of geology if our stratigraphical chronometer were untrustworthy; no detailed study of the mineral composition, organisms, and physical changes of our globe could furnish material for a connected history of that globe. But the institution of the zonal method has given us a brighter outlook, and already we have gained knowledge which has been turned to excellent use. I may refer, in support of this remark, to the discovery of the "Secret of the Highlands," and to the establishment of a horizon—the *Olenellus* zone—which enables us to fix the upper limit of the pre-Cambrian Rocks.

Another use which can be made of the method we are studying is the more accurate estimate of the physical conditions under which groups of strata were deposited. This kind of work has hitherto been carried on in a somewhat haphazard way as far as the Lower Palæozoic Rocks are concerned, but as an example of the results which we may eventually get by careful study of zones, I may point to Professor Lapworth's admirable section showing the lateral changes observable in the Ordovician and Silurian strata of the Southern Uplands, appended to his paper on the Ballantrae Rocks, which appeared in the *Geological Magazine* for 1889.

The study of zones not only throws light upon the physical conditions under which they were deposited, but also gives us additional evidence of the value of these zones. The expansion of thin masses of shale into much thicker deposits elsewhere, and the swarms of fossils occurring in those deposits, has been held for some time to indicate that the thin sets of strata were formed far from land, and this is supported by observation of the nature of the included fossils. It has been shown by Dr. Hinde that deposits of radiolarian chert are associated with the graptolitic shales of Scotland, and these have been compared with similar modern deposits formed in deep sea. Professor Suess has given instances of trilobites amongst the Lower Palæozoic Rocks which strikingly resemble modern deep-sea crustaceans in more than one respect, and such trilobites are found amongst the strata which are divisible into thin zones. It would seem, therefore, that the evidence points to such rocks having been formed in seas far away from land, and in that case we need not be surprised at the very wide geographical distribution of the organisms of the zones.

The inquiry into the cause of zones may possibly throw some light on climatic change, though here we are touching upon a question upon which it would be premature to speculate. Suffice it to say, that the sharp line of demarcation between many zones does not appear to be due to change in the physiographical conditions of the area (apart from climatic change), for we find sediment having the same lithological characters, divisible into several distinct life-zones, and, as already stated, the same zone may be traced among strata of very different lithological characters.

In studying the phylogeny of different groups of organisms, the establishment of zones will prove invaluable. Numerous errors have shown the danger of attempting such a phylogeny by a study of fossils in the Museum, without reference to their order of appearance in the strata. On the other hand, an earnest of what may eventually be done is furnished by the contribution to the study of the descent of the later graptolites given by O. Hermann in his monograph of the Dichograptidæ.

The zonal method has "quietly entered upon what may be regarded as the accepted or orthodox stage"—so wrote Professor

Lapworth in 1889. It now remains to apply that method to the more accurate correlation of the strata of the earth's surface, for hitherto the work has been only commenced. The geologist of the future will gather a valuable harvest from the material that his predecessors have collected. We must be, in the main, content patiently to collect that material, comforting ourselves with the knowledge that brilliant speculations, founded on insufficient data, have usually but a brief existence, while a collection of accurate facts is of service for all time. We need not fear that our energies will be altogether exerted in cataloguing facts; every careful piece of work assists in the establishment of some new truth, or the acquisition of fresh ideas.

The Stratigraphical Geology of the future will not consist almost exclusively of the enumeration of the order of succession of deposits, their thicknesses, their lateral variations, and their fossil contents. The earth-movements, the vulcanicity, the metamorphic changes, the climatic variations, and the life-history of each period will be more fully elucidated, and the elaboration of a true Earth History will be possible. In the meantime, we rejoice to see Stratigraphy once more placed upon a firm basis, and, while still indebted to her early handmaid, Palæontology, having fresh life infused into her by contact with the young and vigorous Petrology.

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VI.

A New Group of Flowering Plants.

QUITE recently the study of a genus of flowering plants, in many respects very peculiar, has led to most interesting results. The genus *Casuarina* occurs chiefly in Australia, but is also met with in Tropical Asia and Madagascar. In Australia it is known as the She-Oak, but it is hard to understand why, unless, perhaps, from its hard and heavy wood. The leaves are reduced to tiny scales, borne in whorls on long green whip-like branches. The male flowers are borne on spikes and consist of solitary stamens surrounded by four bracteoles; the female flowers are borne in capitula, on twigs and branches of a very different age, and may often be found in the old wood of thick branches. So unlike is the genus to any other, that it has hitherto had a natural order to itself, styled Casuarineæ, and placed in the Apetalous group of dicotyledons. Its affinities have always been regarded as most obscure, and the order as very distinct from all other dicotyledons.

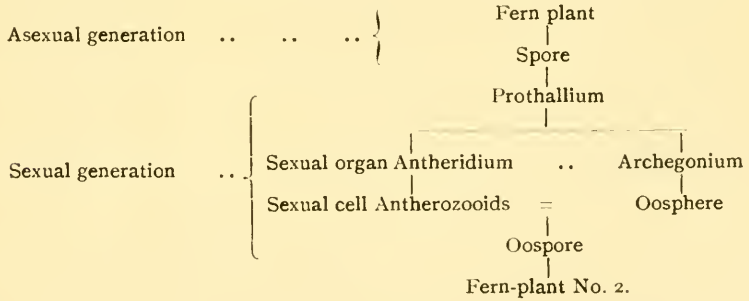
At the close of last year, Dr. Melchior Treub, director of the Botanic Gardens at Buitenzorg, Java, published the result of some detailed researches, "Sur les Casuarinées et leur place dans le Système Naturel." The results are startling, as may be judged from the fact that they have led their author to place the Casuarineæ by themselves in a distinct sub-division of the Angiosperms. This is not the first time that Dr. Treub has made the *Annales du Jardin Botanique de Buitenzorg* the medium of a most important morphological communication. In 1881, he communicated an important paper on the Cycads, including many new facts relating to the development of the macrosporangium, and in 1884 the first complete account of the structure and life history of the prothallium of Lycopodium, which he had succeeded in cultivating from the spore, and also found growing naturally. It may be interesting to give a brief *résumé* of his latest researches.

At the outset, however, we may briefly review the structures which are now generally regarded as of primary importance in the classification of plants into large groups. A true appreciation of the principle of the Alternation of Generations forms the basis of the whole subject.

The alternation is clear enough in the Ferns. It is a matter of common observation that the tiny spores borne in great profusion,

usually on the back of the fertile leaves, produce on germination not a new fern-plant, but a small green expansion showing no differentiation into stem and leaf, and attached to the soil only by a few colourless root-hairs. From this expansion, the *prothallium*, arises a new fern-plant similar to the original spore-producing individual. This new plant is the result of a sexual process. On the under surface of the prothallium are two sets of organs, the male, or *antheridia*, containing the male cells or *antherozooids*, and the *archegonia*, or female organs, containing the female cell or *oosphere*. The antheridium consists merely of a wall of a single layer of cells enclosing the naked unicellular spirally-coiled antherozooids. The archegonium consists of four rows of cells forming a projecting neck, round a row of *canal-cells* terminating at the central cell or *oosphere*. Fertilisation takes place by the rupture of the wall of the antheridium, and the escape of the antherozooids which swim about in the water present and are attracted to the archegonium by a mucilaginous secretion resulting from the disorganisation of the canal-cells. They pass down the passage thus formed and penetrate the naked oosphere, which is at once invested with a cell-wall and becomes the *oospore*. On germination, the oospore produces the young fern-plant.

We may represent the life history in a tabular form :—



In this case the sexual generation represented in the prothallium is quite distinct from the previous asexual generation or fern-plant, and also gets quite free from the spore.

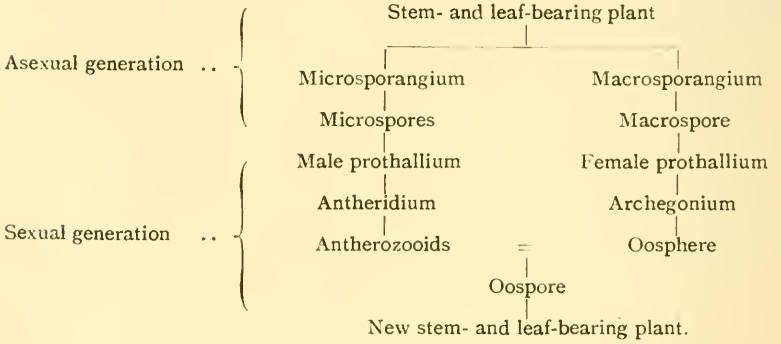
The spores are contained in small roundish capsules, the *sporangia*—there are usually sixteen in each—arising from the division of a single central cell, the *archesporium*. They are set free by the bursting of the sporangium-wall. The true Ferns are *homosporous*, *i.e.*, produce only one kind of asexual spore, and one prothallium bears both antheridia and archegonia, but the nearly allied Rhizocarps, which include *Salvinia* and *Pilularia* (the Pillwort, a local British plant), are *heterosporous*. Two sorts of spores are produced in two sets of sporangia. The smaller *microsporangium* produces a number of small spores, *microspores*, and the larger *macrosporangium* one large *macrospore*. The indication of sex has been carried back into the asexual generation as far as the sporangia, but both kinds of sporangia are

borne on the same leaf, though this has become considerably modified and differs remarkably in appearance from the vegetative leaf.

The prothallium resulting from the microspore is very simple. In *Pilularia*, for instance, the spore divides into three cells, the smallest representing the prothallium, while the other two together form the antheridium, and, by repeated bipartition, give rise to sixteen naked antherozooids. The prothallium and antheridium remain enclosed in the spore, which ultimately bursts to allow the escape of the antherozooids. The macrosporangium, like the microsporangium, becomes separated from the leafy asexual plant, but the macrospore never gets quite free, although in course of growth it ruptures the sporangium wall above.

The female prothallium, like the male, is very simple and contained in the spore, forming a small green structure on which one or more archegonia are developed in the same way as in the homosporous Ferns. The sporangium and spore become ruptured near the apex to give access to the antherozooids.

We may represent the life history thus :—



Besides the homosporous and heterosporous ferns, the group Vascular Cryptogams comprises also the Equisetineæ, or Horse-tails, and the Lycopodineæ, including the Lycopodiaceæ, or Lycopods proper (Club-mosses), and the Ligulateæ, comprising *Selaginella* and *Isoëtes* (the British Quillwort). Of these the Equisetineæ and Lycopodiaceæ are homosporous, while *Selaginella* and *Isoëtes* are heterosporous. The sporangia are produced, not from a single cell, as in the Ferns, but from a group, which forms a hemispherical protuberance on the under margin of the modified, peltate scale-leaf of the Equisetineæ, or in the upper surface of the base of the leaf in the Lycopods, in the hollow of the leaf-sheath of *Isoëtes*, or on the stem just above the leaf-base of *Selaginella*. The archesporium is either a single cell beneath the epidermis, as in Equisetineæ, or a hypodermal layer, as in *Isoëtes*.

If we follow the process in the macrosporangium of *Isoëtes*, we find that the cells of the archesporium elongate and divide by transverse

walls; single longitudinal rows grow less rapidly and form sterile trabeculæ separating the rows of fertile cells. The latter give rise to a few *tapetal* cells by transverse division towards the outer wall and one sporogenous cell, the mother-cell of the macrospores, which becomes rounded off and grows considerably, destroying and using up the tapetal and neighbouring cells, till it thus comes to lie in a cavity of the sporangium, where it divides into four daughter-cells, the macrospores.

The germination of the male or microspore somewhat resembles that of the Rhizocarps.

The macrospore in *Isoëtes* a few weeks after being set free from the decaying macrosporangium, becomes filled with cells, which are at first naked, but when the spore is quite filled become invested with a cell-wall. The prothallium then occupies the whole interior of the spore, which now bursts at the apex, and one or, if necessary, more archegonia are developed on the exposed portion of the prothallium.

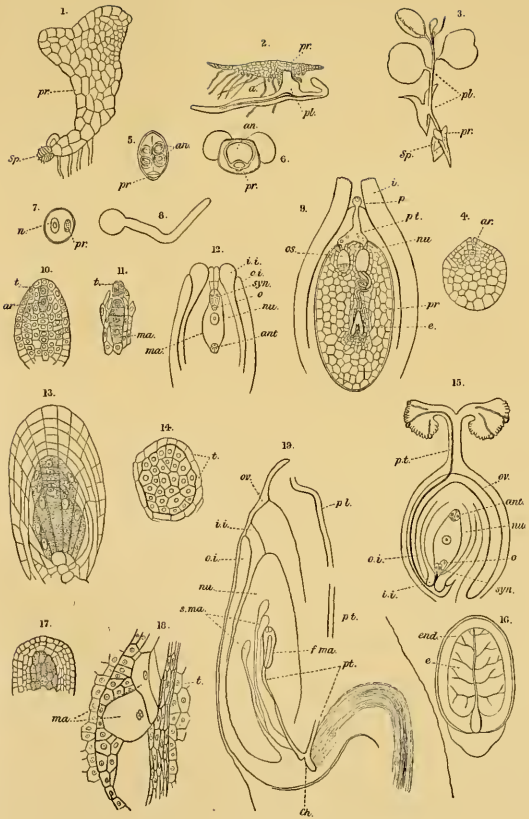
The alternation of sexual and asexual generations is evident in all these families classed together in the great group of Vascular Cryptogams. In every case the sexual generation, though it may not escape from the spore, and sometimes not even get quite free from the sporangium, as in *Salvinia*, is yet distinct from the always conspicuous asexual plant, with its evident differentiation into root, stem, and leaf. It was not until Hofmeister published his "Comparative Researches," in 1851, that the relation became clear between the higher plants, the Phanerogams, or *Seed-plants*, and the Vascular Cryptogams. We now understand that the same principle runs through the life history of all, and the accurate correlation of the organs in the two great classes has thrown a much clearer light on the principle of classification. Phanerogams are characterised by producing a seed, and it is by the proper comprehension of the development of the seed that we are able to understand these relations.

Phanerogams are heterosporous, and the indication of sex has penetrated far into the asexual generation. There are not only two sets of sporangia, but two sets of modified leaves to bear them. Those which bear the microspores are the *stamens*, while the *carpels* have the same relation to the macrospores. Stamens and carpels are commonly associated in the *flower*, a structure unknown in the Cryptogams, but developed in the Phanerogams to ensure the protection of the "essential" sexual parts, and the proper performance of their functions. The method of production of the microspores or pollen-grains follows a general plan similar to what obtains in the microsporangium of Vascular Cryptogams like *Isoëtes*. The pollen-sac, or sporangium, arises from a group of cells sometimes on the underside of a leaf-like stamen, as in Conifers, sometimes, as in Angiosperms, several together, forming the anther at the apex of the

very modified staminal leaf. The sporogenous tissue is the product of an archesporium and is surrounded by tapetal cells. The microspores or pollen-grains are formed by division of the mother-cells into four daughter-cells, but the development of the macrospores is of greater interest.

Let us first consider the Gymnosperms, as it was here that the relation to the Vascular Cryptogams was first pointed out by Hofmeister. The *nucellus*, or group of cells composing the sporangium, is surrounded by a single integument, except at the apex, where an open passage is left, the *micropyle*. The macrospore or *embryo-sac* is produced from a hypodermal archesporium, which is covered above by tapetal cells as in *Isoëtes*. The archesporium may divide by a few shiny transverse walls into a row of cells, the lowest one of which is the macrospore, as for instance in the Larch (*Larix*). In the Cypress (*Cupressus*) and *Callitris*, the archesporium may, however, divide further, forming a group of cells, as happens also in the Cycadeæ. A sporogenous tissue is thus produced; all the cells are, however, sterile except the one which produces the embryo-sac, and are replaced, as in *Isoëtes*, by the growing macrospore. There is, moreover, a difference from the Vascular Crytogams, in that the mother-cell does not divide by a repeated bipartition into four daughter-cells or macrospores, but elongates and divides by superposed transverse walls. In the Cycads, the cell-wall of the embryo-sac thickens and splits into two layers, the outermost of which is cuticularised like the membrane of a macrospore, which will be set free from the sporangium. The embryo-sac remains in the nucellus, almost the whole of which it occupies, and the nucellus remains attached to the carpel, and thereby to the parent plant. The macrospore germinates as in *Isoëtes*. The nucleus, by repeated bipartition, gives rise to a number of free nuclei round which cells are formed; these grow and divide in such a way as to fill the macrospore with the tissue of the prothallium. Archegonia are formed at the apex from single superficial cells of the prothallium in the same manner as in the Vascular Cryptogams.

It is obvious, however, that fertilisation cannot be affected in the same way. In Vascular Cryptogams the prothallium, either free or still contained in the ruptured spore is found on damp ground or in mud at the bottom of water. But free-swimming antherozooids would be incapable of penetrating to the oosphere borne high and dry on the cone of a Fir-tree. Hence the mechanism is different. The pollen-grains when set free from the pollen-sac, contain an indication of a prothallium in one or two cells cut off from the rest of the contents. The light grains, produced in large quantities, are carried by the wind. Some find their way to the carpel and the micropyle of the ovule, and pass through it to the apex of the nucellus. The antheridial cell grows out as a short tube into the tissue of the nucellus, where it remains stationary for a time. Then, when the oosphere is



EXPLANATION OF PLATE.

Fig. 1.—Fern-prothallium (*pr.*) developed from the spore (*sp.*) to which it is still attached.

,, 2.—Fern-prothallium (*pr.*) quite freed from the spore, and bearing the young asexual plant (*pl.*) *a*, an archegonium, the oosphere of which has not developed (longitudinal section).

,, 3.—Young asexual plant of *Salvinia* (*pl.*) borne on the prothallium (*pr.*), which has not become quite freed from the macrospore (*sp.*).

,, 4.—Longitudinal section through a macrospore of *Isoëtes* filled with the prothallium which bears an archegonium (*ar*) at the apex.

,, 5.—Microspore of *Isoëtes*, showing formation of antherozooids (*an.*). *pr.*, prothallium-cell.

,, 6.—Microspore (Pollen-grain) of *Pinus sylvestris*; *pr.*, prothallium-cell; *an*, antheridial cell, which will grow out as the pollen-tube.

,, 7.—Pollen-grain of an Angiosperm; *n.*, nucleus; *pr.*, prothallium-cell.

,, 8.—Ditto, in germination, showing production of pollen-tube

,, 9.—Longitudinal section of ovule of a Gymnosperm, showing contents of macrospore (embryo-sac), method of fertilisation and development of embryo; *i.*, integument; *nu.*, nucellus; *pr.*, prothallium (endosperm) contained in the embryo-sac; *os.*, oospore, showing primary cell divisions as result of fertilisation; *e*, embryo developed from the other oospore with which it is seen to be still connected. *p.*, pollen-grain germinating in the micropyle; *p.t.*, pollen-tube which has fertilised the oospheres.

,, 10.—Longitudinal section through the young ovule of an Angiosperm. Two cells are distinguished by size and contents from the rest of the nucellus; *t.* is the primary tapetal cell, *ar* the archesporium.

,, 11.—Later stage of 10. The archesporium has developed into a row of cells, the lowest of which (*ma.*) will become the embryo-sac.

,, 12.—Still later stage of same, showing the ovule, consisting of nucellus (*nu*) and outer (*o.i.*) and inner (*i.i.*) integuments, and also the contents of the embryo-sac (*ma.*); *o.*, oosphere; *syn.*, synergidæ; *ant.*, antipodal cells.

,, 13.—Longitudinal section of nucellus of *Rosa livida*, showing the sporogenous tissue distinguished by great elongation of the cells, large nuclei, and conspicuous cell-contents. Tabular tapetal cells, also distinguished by nuclei and contents, are seen above the sporogenous tissue.

,, 14.—Sporogenous tissue of a Cycad surrounded by a layer of tapetal cells (*t.*).

,, 15.—Longitudinal section of ovary of Angiosperm at the time of fertilisation; *ov.*, ovary-cavity; *o.i.*, outer integument of ovule; *i.i.* inner ditto; *nu.*, nucellus containing the mature embryo-sac in which are seen *o.*, the oosphere, *syn.*, synergidæ, *ant.*, antipodal cells; *p t.*, pollen-tube making its way from the grain which germinated on the stigma, down the style and through the ovary-cavity and micropyle to the apex of the nucellus.

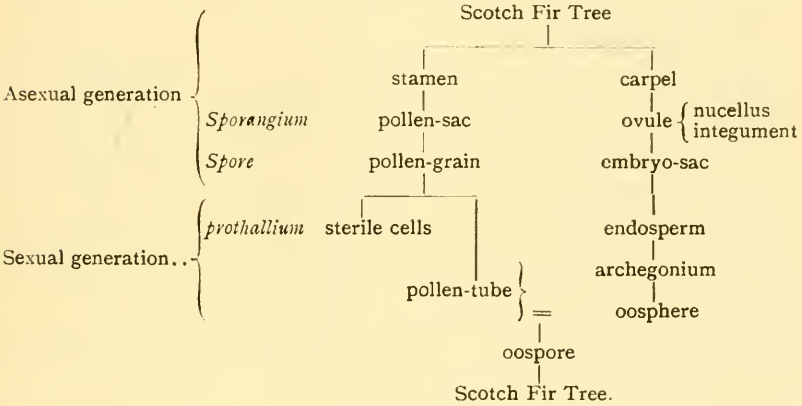
,, 16.—Seed of an Angiosperm containing the embryo (*e.*) surrounded by endosperm (*end.*).

,, 17.—Longitudinal section of nucellus of *Casuarina* showing development of sporogenous tissue (dark-celled). After Treub.

,, 18.—Longitudinal section of sporogenous tissue at a later stage, showing the large macrospores (*ma.*). A tracheid is indicated at *t.* After Treub

,, 19.—Longitudinal section of ovule after passage of pollen-tube; *ov.*, ovary-cavity: *o.i.*, *i.i*, outer and inner integuments; *s. ma.*, sterile macrospores: *f. ma.*, fertile macrospore containing sexual apparatus; *ch.*, chalaza: *p.t.*, pollen-tube passing down the tissue of the placenta to the chalaza, where it branches, the main branch passing up the tail of a sterile macrospore (not indicated) and becoming attached to the fertile macrospore. The constriction of the pollen-tube is shown.

mature and ready for fertilisation, the tube begins to grow again through the nucellus, reaches the apex of the embryo-sac, and thrusts between the neck-cells of the archegonia protuberances which finally reach the oosphere. A number of small nuclei have been formed in the extremity of the pollen-tube from the original antheridial nucleus; these disappear in fertilisation, and their substance in some way or other passes into the substance of the oosphere, which thus becomes the *oospore* and is invested with a cell-wall. From the oospore is produced the embryo. Meanwhile, the integuments of the ovule, together with what remains of the nucellus, have become thickened and hard or leathery, forming the coats of the seed, which is now ready to leave the parent, carrying a miniature of it in the enclosed embryo. The life history of a typical Gymnosperm, say the Scotch Fir (*Pinus sylvestris*), will be as follows :—



The great difference, from this point of view, between the Gymnosperms and Vascular Cryptogams, is the complete merging of the two generations in one individual, and the entire dependence of the sexual generation for nourishment and support on the previous asexual one. This results in the production of the *seed* in which the second asexual generation is finally dispatched to start a separate existence.

Coming now to the highest group, the Angiosperms, the plants with an ovary, we shall find important differences.

As regards the macrosporangium, the nucellus is protected by one or two integuments. In the simplest case a sub-epidermal cell, which is the archesporium, also becomes the macrospore (embryo-sac). This occurs in *Lilium bulbiferum*, but is rare. We usually find the type established by Strasburger. One sub-epidermal cell, the archesporium, divides transversely. The upper daughter-cell grows and divides, giving rise to parietal and tapetal layers, while the lower, the mother-cell of the macrospore, divides by one or several shiny, transverse walls, and one of the resulting series of cells, almost always the lowest, becomes the

embryo-sac, growing enormously, and crowding and absorbing its sister-cells. In rare cases, one of the latter develops further, but never far enough to give it the character of a sterile macrospore, a rank which it certainly holds theoretically. Sometimes the nucellus contains two or even several embryo-sac mother-cells. Thus, Strasburger saw four in *Rosa livida*, and this supports the view that we are dealing with rudimentary sporogenous tissue. In this case, usually one of the daughter-cells from the commencement crowds and absorbs, before they have reached any development, not only its sister-cells, but also those arising by division of the other mother-cells. Occasionally, some may develop enough to assume the appearance of sterile macrospores, but it is very rare that one of them succeeds in reaching to the formation of the sexual apparatus, as Fischer saw in *Triglochin palustre*, and this, too, eventually disappears.

As regards the development of the macrospore, there is a profound difference from what we have hitherto seen. The nucleus of the embryo-sac divides into two; this is twice repeated, and each group of four nuclei retires to one end of the oval embryo-sac. Three at the apex become surrounded with protoplasm and form naked cells—the *oosphere* and two others termed *synergidæ*. Three at the opposite end also form cells which become invested with a cell-wall and are known as antipodal cells; their function is unknown. The synergidæ assist in the process of fertilisation; they may be regarded as "endosperm cells which by an adaptation to a new function have acquired a special form and place." The remaining pair of nuclei again advance to the centre and join, forming a central nucleus. The pollen-tube grows down the tissue of the ovary to the micropyle, passes through this, and becomes attached to the embryo-sac (which has occupied almost the whole of the nucellus) just above the oosphere.

Fertilisation occurs as in the Gymnosperms and the resulting oospore becomes at once invested with a cell-wall. The development of the embryo is on a similar plan to that in Gymnosperms, and differs entirely from the mode of formation in Vascular Cryptogams.

After fertilisation, and during the early stage of embryonic development, the nucleus of the embryo-sac, by repeated bipartition, forms a number of free nuclei, and the sac becomes filled with endosperm tissue. Similarly the ovule becomes the seed.

It is evident that in Angiosperms the female sexual generation is reduced as far as possible. The prothallium is represented by the synergidæ and, perhaps, the antipodal cells. The archegonium has been reduced to the central oosphere. The endosperm formed after fertilisation must probably be regarded as a new formation, and not strictly comparable to the endosperm of Gymnosperms, which is the true homologue of the prothallium of Vascular Cryptogams.

We have thus accounted for the sub-division of the seed-plants as at present accepted.

Seed-plants (Phanerogams).

Division I.	Division II.
Gymnosperms.	Angiosperms.
Ovule naked on the carpel.	Ovules borne in an ovary.
Distinct female prothallium present bearing archegonia as in the Vascular Cryptogams.	Female prothallium extremely rudimentary, and the female organ reduced to the oosphere.

Let us now return to Dr. Treub's recent work. He first gives a clear account of the development of the ovary and ovules on which our knowledge has hitherto been "inexact and incomplete." The ovary is formed of two carpels, which at first include an ovary cavity, but the cavity subsequently disappears almost entirely, and is only indicated again when the ovules begin to develop. One is produced from each carpel, but an inequality is early noticeable between the two protuberances from which they are developed, the smaller forming only a sterile ovule. They are separated by the cellular mass of the placenta and are semi-anatropous.

Section 2 deals with the sporogenous tissue and the macrospores. The archesporium consists of four large subepidermal cells, by the growth and division of which is produced a fairly thick cylinder of sporogenous tissue in the centre of the nucellus, quite distinguished by the large size of the cells from the enveloping layers. An intercalary growth at the base connects the sporogenous tissue with the *chalaza*, where the tissue of the nucellus is attached to the funicle or stalk of the ovule. The large cells of the sporogenous tissue are embryo-sac mother-cells. They divide rapidly by transverse walls, which are thick and shiny like the walls of the embryo-sac mother-cells of other Angiosperms. Three differing elements result from this division:

1. Small cells with a dense protoplasm and big nucleus. These are ultimately, but *not at first*, crushed and absorbed by the macrospores.

2. Large cells or macrospores.

3. Tracheides. These Treub compares with the elaters of Hepatics, though their function is unknown. They occurred in two out of the three species examined.

The macrospores grow rapidly, producing long tails, which often penetrate the chalaza. The number developing may be considerable, in *Casuarina suberosa* as many as twenty; where they are very numerous, some in the region of the micropyle do not produce tails.

As regards the contents of the macrospores, we must distinguish between sterile ovules and those which will be fertilised. In the former, Treub rarely saw tails to the macrospores, the latter increasing

in size without elongating, and often containing two fairly large nuclei. In the fertile ovules, those macrospores which are well developed, that is, the majority, include at the apex two or three cells, rarely only one. These are usually naked, but sometimes have cellulose walls, and their arrangement seems to indicate that they have come from one primary cell. This is the sexual apparatus. In the great majority of cases there is only one macrospore in the nucellus in which the sexual apparatus has the cellulose walls; this is the future fertile embryo-sac. Occasionally, however, there is more than one in which this is the case. The perfect equivalence of the future embryo-sac and the sterile macrospores thus admits of no doubt.

Section 3 deals with the pollen-tube and the fertile embryo-sac. Treub finds that the pollen-tube does not enter the ovary cavity, and does not pass down the micropyle, but enters the nucellus at the opposite end, namely, at the chalaza. Only one pollen-tube enters each ovary. It passes down the style, through the tissue bordering the nucellus, to the end of the vascular bundle which reaches the chalaza. Here it branches, one or two short recurved limbs passing outwards towards the surface of the ovule, while the main portion enters the chalaza by the tail of a sterile macrospore, and passing rapidly up leaves it again to approach and become fixed to the embryo-sac. Sooner or later the pollen-tube contracts at a point in the middle of the nucellus, and the upper portion becomes closed off, and separated from the rest. This is evidently due to the fact that the embryo-sac and nucellus grow considerably after the attachment of the pollen-tube to the former, and this causes the rupture.

The oosphere is distinguished from the one or two "neighbouring cells" produced at the same time by its thicker membrane. In some cases it is alone. Several examples have been noted in other Angiosperms of synergidæ clothed with a cellulose membrane, but there is not a single well-authorised case of an *oosphere* invested with a cellulose wall before fertilisation. The appearance of the cell-wall is the result of fertilisation, and is always regarded as an indication of its accomplishment. Treub never saw antipodal cells in *Casuarina*.

Another curious point is that the pollen-tube never joins the macrospore membrane above the insertion of the sexual apparatus as in other Angiosperms, but always at some point more or less distant, sometimes diametrically opposite.

The embryo-sac continues to increase in size, the number of nuclei in the cavity also increases, while the sexual apparatus still remains in the same state, the oosphere keeping the same aspect and dimensions. The pollen-tube never enters the embryo-sac, but the end applied to the wall contains a very distinct protoplasm, and, probably, nuclei.

If we consider as adult the embryo-sac which has reached the dimensions it will keep till the appearance of the embryo, we find

that up to this stage the sexual apparatus remains unchanged. When this stage is reached, the embryo-sac contains a great number of nuclei, while in the dense protoplasm at the summit, endosperm cells begin to be formed round them, and this formation descends gradually to the base till the sac is filled with solid endosperm. Shortly after the appearance of the first cells, development of the embryo commences, by division of what must accordingly be now the oospore. The embryogeny conforms to the ordinary dicotyledonous type, and Treub saw nothing particular to characterise it.

Treub cannot indicate the precise moment of impregnation, not having seen the male nucleus penetrate to the oosphere. The otherwise universal test of the presence of a cell-wall will not apply, for the oosphere of *Casuarina* has a cellulose membrane from its birth, at a time when the pollen-tube has not even entered the nucellus. Another indirect test fails. In Angiosperms carefully studied for this purpose, the synergidæ have generally quite a different appearance before and after fertilisation, and often the fertilised oosphere elongates considerably before the first division proves that it has been fertilised. It is necessary from the position of the pollen-tube that the male nucleus must (1) traverse the membranes of the pollen-tube and embryo-sac, and enter the cavity of the latter; (2) traverse a smaller or greater part of the cavity; (3) penetrate the oosphere from below. Treub thinks fertilisation takes place much later than the time of junction of the pollen-tube and embryo-sac, in fact, the moment before the first endosperm cells appear in the top of the adult sac, and adduces in support the following facts. The pollen-tube remains attached to the embryo-sac; we might expect if fertilisation had taken place at once that the tube would have perished. There is, moreover, a difference in the contents of the tube while the embryo-sac is still growing, and when it is nearly or quite adult. In the latter, the protoplasm has lost its former distinct appearance, and no trace of a nucleus can be seen. The Casuarineæ being tropical and sub-tropical plants experiencing no intervening inclement season, there is no reason for an interval between fertilisation and division of the oospore, such as occurs, for instance, in the Autumn Crocus, where fertilisation takes place in the autumn, but the oospore remains undivided through the winter till the following May. Treub avers, moreover, to have several times seen in adult embryo-sacs in which, according to his view, fertilisation was on the point of taking place, or even had just occurred, a spot in the base of the oosphere where the membrane was very thin and sometimes drawn out to a point which looked as if it were open. He also cites a case where an embryo-sac had undergone considerable development before the pollen-tube had got even close to it.

This view implies that the production of the numerous nuclei in the sac is not a consequence of impregnation as in other Angiosperms, but as independent of it as in Gymnosperms. It is, in fact,

the commencement of a true prothallium. The production of endosperm cells must coincide almost with the moment of impregnation, which would clearly be rendered impossible by an earlier production of cells, considering the path the male nucleus must take. It may be that the pollen-tube when entering the nucellus stimulates the young embryo-sac to assume the state necessary for it to be fertilised later on, but there is a profound difference between mere accessory stimulation and fertilisation. Is it possible to follow the little male nucleus, lost among the numerous nuclei of the large embryo-sac, on its way to the oosphere, and to determine the precise moment when it enters the bottom of the oosphere? The difficulties are all but insurmountable, and we at any rate cannot but agree that Dr. Treub was justified in his determination to put before us the many interesting and unexpected facts he has elucidated without waiting to settle this doubtless very difficult point.

The Casuarineæ are usually placed in that section of Apetalæ which includes also the Cupuliferæ, Myricaceæ, Juglandaceæ, and other families. It is in these that we should expect to trace affinity with the Casuarineæ, but in none is there any indication of the curious structures and events just described, and Treub concludes that the Casuarineæ occupy a quite exceptional place among the Angiosperms.

For the great development of sporogenous tissue (recalling the Cycads and Cupressus) and the large number of macrospores, the greater number of which arrive at the formation of a sexual apparatus, we must seek comparison with the Vascular Cryptogams. The formation of a large number of prothallial nuclei in the fertile macrospore before fertilisation (supposing Treub's view correct as to the moment of impregnation), recalls what passes in a greater degree in the macrospore of Gymnosperms, and indicates an intermediate stage between the latter and the Angiosperms. The origin of the sexual apparatus from a single cell, the similarity in aspect of the "neighbouring cells" of the oosphere to "canal-cells," indicate an homology with the archegonium of Gymnosperms and Vascular Cryptogams, and that the "neighbouring cells" are not comparable with the synergidæ of Angiosperms. Finally, there is the most anomalous mode of entry of the pollen-tube by way of the chalaza, for which Treub suggests the following explanation.

When Angiospermy originated, the micropyle lost its function of conducting the pollen-grains which now germinated, not on the nucellus as in the Gymnosperms, but at a distance, the pollen-tube having to traverse the intervening space. There were two ways in which the pollen-tube might reach the embryo-sac: the one used before by the grain, through the micropyle, the other through the chalaza. The genus *Casuarina* is, as far as we know at present, the only surviving example which chose the latter mode of entry, the rest of the Angiosperms taking the former.

Believing that he has clearly demonstrated that *Casuarina* constitutes the type, and probably the only representative, of a separate class, Treub proposes the following division of Angiosperms :—

Sub-group Angiosperms.

Sub-Division.	Sub-Division.
Chalazogames.	Porogames.
Class.	Classes.
Chalazogames.	Monocotyledons; Dicotyledons.

This grouping is open to criticism, and we cannot but think that the author has selected the least important difference to characterise his new subdivision; for the route taken by the pollen-tube is surely of little account provided the macrospore is ultimately reached. Until we have some more definite evidence of the relation in time between impregnation and the formation of the endosperm, we cannot lay much stress upon it for purposes of classification. It is quite possible that fertilisation never occurs, and that the oosphere born with its cell-wall already formed is potentially an oospore. There are cases of apogamy quite as striking in the fungi among the Saprolegnieæ. The fact that nuclei were never distinctly seen in the apex of the pollen-tube points in this direction, as also does the comparatively thick membrane of the oosphere, which would afford considerable resistance to the passage of fertilising material. The most striking fact is the number of the macrospores, and the high development they attain. *Rosa livida*, referred to above, shows an indication of the same, though in a much less degree.

Dr. Treub has certainly made out some points of the highest interest, but hardly sufficient, especially considering the perfect similarity of the embryology with the ordinary dicotyledonous type, to separate the genus *Casuarina* as a distinct subdivision of flowering plants.

A. B. Rendle.

VII.

The Naturalist in La Plata.[1]

SELDOM, if ever, have we read with such extreme pleasure and interest any work on the habits and ways of animals in their native state as Mr. W. H. Hudson's "The Naturalist in La Plata," a volume which is in every respect a worthy successor to Mr. Bates's "Naturalist on the Amazons," or Mr. Wallace's "Malay Archipelago." Mr. Hudson is, indeed, in some respects unrivalled as an observer of animate nature, and is equally at home when treating of the ways of large ferocious animals like the puma, or when describing the curious dances of the birds of the Pampas. He has also the rare charm of conveying to his readers the results of his observations in such bright and vivid language as cannot fail to make the volume acceptable to all lovers of nature. Indeed, so striking is his word-painting, that we seem to realise the scenes described, even without the aid of the very excellent figures with which many of them are illustrated.[2]

The greater portion of the work, as we are informed in the preface, has already seen the light in the form of articles in various magazines and journals. This, indeed, would have been apparent of itself, on account of certain repetitions which occur here and there, notably the description of the scent given by the male Pampas deer, in almost identical sentences on pages 16 and 159. The author being essentially an outdoor naturalist, may, perhaps, be excused for not being quite up to date in regard to nomenclature and synonomy. For instance, *Didelphys aurita* (p. 18) is now regarded as identical with *D. marsupialis*, of which *D. azaræ* (p. 102) is only a variety. Then again, the bat alluded to on page 101 as a *Vespertilio* would appear, according to Dr. Dobson, to be an *Atalapha*. There are, moreover, a few misprints, such as *auritur* for *aurita* (p. 18), *Porphyriops* for *Porphyrio* (p. 20), and over-bird for oven-bird, which might have been avoided by a little more care in proof-reading. These are but slight blemishes, and any inaccuracy in the proper names of the opossums of the Pampas is largely discounted by the

[1] THE NATURALIST IN LA PLATA. By W. H. Hudson. London: Chapman and Hall, 1892. 8vo. Pp 388. Illustrated. Price 16s.

[2] We are indebted to the publishers for the opportunity of presenting samples of the illustrations to our readers.

interesting observations on the occurrence of such animals in those treeless wastes.

Passing on to a brief survey of the work, we find in the first chapter an excellent description of the Pampas and its inhabitants —both feathered, mailed, and furred. Among the latter, considerable interest attaches to the account of the large Rodent Coypu, and especially its sudden increase and change of habits when protected by law, and its equally sudden destruction by a pestilence which rendered the species well-nigh extinct. A striking illustration is given of a female coypu swimming with its young. Later on (p. 23), we notice that the author waxes very wroth with those who advocate a northern origin for the life of the globe, and at the same time expresses his belief in the existence of a varied bird-fauna, and perhaps also of mammals, in the unexplored antarctic regions. We fear, however, that as regards mammals at least this sanguine view

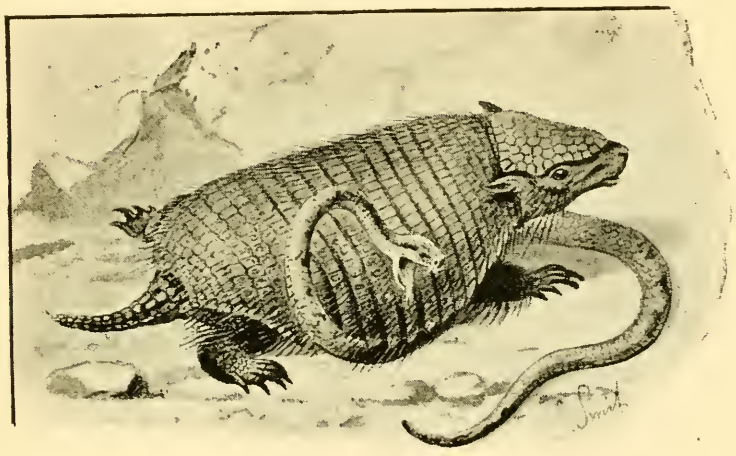


FIG. 1.—Three-banded Armadillo killing Snake.

is not likely to be realised. Near the end of this chapter the author is at his best in his description of the Rhea, or American ostrich, and we regret to learn that this splendid bird, with some other feathered inhabitants of the Pampas, is likely ere long to share the same fate as the bison of the prairies of the northern half of the continent.

In the second chapter we have a full biography of the puma, which, far from being the cowardly creature generally reported, is described as being really bold and courageous. Curiously enough, it appears to be now well ascertained that the puma in South America will not only never attack man, but will not even defend itself against him. It is new to us that pumas are such deadly enemies to horses as to render their existence impossible in regions where the former abound; and it is accordingly suggested that we may here have an

adequate cause for the extinction of the original American horses of which the remains are found in the superficial deposits and caves. Mr. Hudson gives full credit to the statement of the occurrence of the puma in Tierra del Fuego.

In the fourth chapter, entitled "Some curious Animal Weapons," we desire to draw particular attention to certain very interesting observations regarding the hairy armadillo. This animal (of which we reproduce Mr. Hudson's figure) is stated to have a good chance of surviving all the other members of the group (which are rapidly diminishing), owing to the omnivorous habits it has acquired, and the assumption of a partially nocturnal mode of life. The curious manner in which this armadillo burrows after its prey is graphically described, and the illustration reproduced exhibits the mode in which it actually saws the flesh of unfortunate snakes by means of the sharp edges of its carapace.

Omitting the interesting chapter on "Fear in Birds and

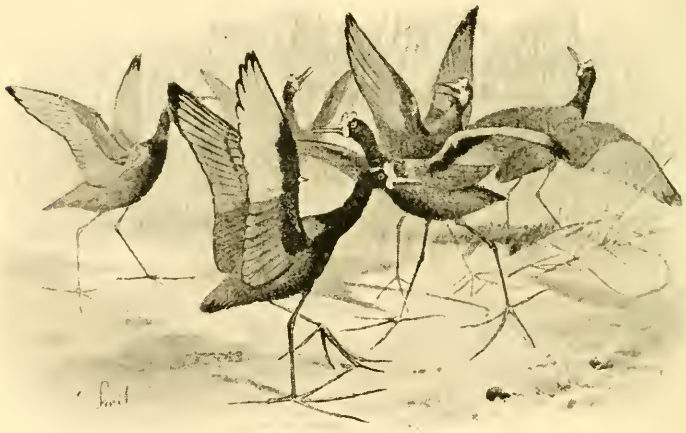


FIG. 2.—Wing-display of Jacanas.

Parental Instinct," we pass on to the seventh, where we find much entertaining reading as to the terrible effects of the secretion from which that noisome creature, the skunk, derives its name; and we confess that until we read the anecdote detailed on pp. 117, 118, we had no conception how persistent and how effectual is even one drop of that fluid. After five chapters devoted to Insects—among which the one on Dragon-fly-storms will be found especially interesting —we have in the fifteenth an account of the death-feigning instinct, which curious mode of protection is developed in such widely different animals as foxes, opossums, and tinamus. The sixteenth chapter is devoted to Humming-birds, where we find it remarked that, after exhausting his powers of admiration on their gorgeous colouring and strange modifications of form, the naturalist finds little scope for

observation among these birds owing to the similarity and mechanical nature of their habits. This chapter is illustrated by a marvellous figure of *Loddigesia*—the strangest in plumage of the whole group, and which was long known only by a single example from Peru, until re-discovered by Stolzmann in 1880. In the chapters on the Horned Screamer, and the Woodhewers (*Dendrocolaptidæ*) space only permits of reference to the extraordinary range of variation presented by the beaks of different genera of the latter, as exemplified by the illustration on page 239.

Perhaps the most attractive chapter in the whole volume is the one entitled "Music and Dancing in Nature," where it is shown how universal is rejoicing in one form or another among all animate nature. First, we have reference to the solitary dance of the gorgeous "cock-of-the-rock," followed by accounts of the social

FIG. 3.—Viscachas at Home.

dances of various other birds, among which those of the rails and their allies appear most remarkable. Among these, the author illustrates the performances of the ypecaha rails and the long-limbed jacanas by excellent figures, one of which we reproduce. In the jacanas, the dance appears designed to exhibit the concealed beauty of the silky greenish-golden wing-coverts. These birds, writes Mr. Hudson, "go singly or in pairs, and a dozen or fifteen individuals may be found in a marshy place feeding within sight of each other. Occasionally, in response to a note of invitation, they all in a moment leave off feeding and fly to one spot, and, forming a close cluster, and emitting short, excited, rapidly repeated notes, display their wings like beautiful flags grouped loosely together; some hold the wings up vertically and motionless, others, half open and vibrating rapidly, while still others wave them up and down with a slow

measured motion." Still more extraordinary is, however, the performance of the spur-winged lapwings, which live in pairs. One such pair may frequently be visited by a single bird from another pair, upon which the stranger takes the lead in a triangular kind of dance, of which the description is so ludicrous that we must refer the reader to the original.

The twentieth chapter is a long and interesting one on the biography of that curious social rodent, the Vizcacha, formerly the commonest of all the mammals of the Pampas. Recently, however, a war of extermination has been waged against these little creatures by the landowners—presumably on account of the harm done by their warrens, the viscachera of the natives—which has been only too successful from their point of view. Mr. Hudson's account of these animals, when in their full development, should thus be of especial value to naturalists; and his excellent illustration (reproduced) gives a good idea of the general appearance of the "viscachera" when fully inhabited.

In the chapter on the guanaco, or huanaco—one of the llamas—the author fully confirms Darwin's account of its unique and inexplicable habit of seeking a spot crowded with the bones of its predecessors when about to die. This habit, says our author, seems "less like an instinct of one of the inferior creatures than the superstitious observance of human beings, who have knowledge of death, and believe in a continued existence after dissolution."

Such are a few of what strike us as the more interesting points in Mr. Hudson's volume. When, however, he has to deal with a work which, like that before us, is teeming with interest on almost every page, the reviewer's task is one of difficulty from an *embarras de richesses*, and we can, therefore, do no more than heartily commend "The Naturalist in La Plata" to the attention of all our readers.

R. Lydekker.

SOME NEW BOOKS.

Outlines of Zoology. By J. Arthur Thomson, M.A., F.R.S.E. 8vo. Pp. 641. With 32 full-page Illustrations. Edinburgh: Young J. Pentland, 1892. Price 12s. 6d.

The number of text-books of Zoology increases rapidly year by year. Only a very short time back the difficulty was to find a book of any kind, written in English, which gave an adequate account of general Zoology and the Comparative Anatomy of Invertebrates as well as Vertebrates. Nowadays, the difficulties are just as great, not on account of the paucity of books, but by reason of their very large number, good, bad, and indifferent—particularly of the last two categories. The struggle for existence among these text-books must be very keen, especially among those which are merely cram-books for examinations of the universities of London and other places. On the whole, the more favourable varieties appear to thrive, though, unfortunately, the rest do not become extinct.

The problem which has to be solved by anyone who writes a book of this kind at present is how to pay due attention to the "types" without producing a mere cram-book. An elementary text-book, in which these types were treated with too scant courtesy, would die a rapid death. Mr. Arthur Thomson, in the manual before us, seems to have succeeded in striking the mean between a too exaggerated respect for the type system and a too pronounced contempt for it. Those animals which find a place in the schedules of universities are for the most part carefully described. We are glad to notice that 12½ pages are devoted to an account of the structure and development of that important creature, the earthworm; the leech, however, only gets 5½, perhaps rather too scanty an allowance.

Besides these descriptions of representatives of the different groups of the animal kingdom, there are good general summaries of the characters of the groups themselves. The Mollusca will serve to illustrate the method adopted. First of all the three main divisions, *i.e.*, Lamellibranchiata, Gasteropoda, and Cephalopoda, are mentioned, with a remark enclosed in square brackets to the effect that *Dentalium* is to be placed in a separate class, and that some authorities admit a few other small classes. Then follow the "General Characteristics of Mollusca," and a tabular classification. After this are "General Notes on Molluscs," containing a few words on the signification of the term "Mollusca" as used in the classificatory schemes of Linnæus and Cuvier; some additional observations upon the structure and growth of the shell; and some remarks upon the larval forms. Each class is then treated in a somewhat similar fashion, the general characters preceding the detailed description of the type, and a classification following. Although this is the general plan upon which the author proceeds, the treatment of each group is not detail for detail the same. This,

however, is no defect; on the contrary, the variations make the book more interesting. A dull monotony is avoided in this way, and also by a frequent change of type, and the interpolation of tabular comparisons of animals. The style, moreover, is attractive—a state of affairs not often found in text-books.

Mr. Thomson retains the division Worms, though he distinctly states that "the animals included . . . form a heterogeneous mob, with little in common." The expression "Worms," when used, is almost invariably written between inverted commas, which further emphasises this perfectly just objection to the retention of such a group.

Two hundred and thirty-four pages out of a total of six hundred and four are devoted to the Vertebrata, not including *Amphioxus*, *Balanoglossus*, and the Tunicates. This seems to us to be rather out of proportion, though in most text-books the Vertebrata claim an equally large share of the author's attention. One of the few books in which they are treated justly, but with no favour, is Professor Gegenbaur's well-known work.

Nearly every page has interesting notes about the habits of the animals under consideration. Thus, in describing the Spiders, Mr. Thomson does not forget that their webs are rivalled in fineness by Mr. Boys' quartz fibres; and he gives some account of the remarkable observations on the "courtship" of the creatures by Mr. and Mrs. Peckham, partly because "it affords a pleasant interlude in our systematic survey," but also as a peg whereon to hang comments on the theory of "Sexual Selection." Most, if not all, of the current generalisations of Biology and the theories which have been put forward for their explanation are treated of; and some of these are interspersed among the chapters upon the various groups, as in the instance mentioned. The general facts of Physiology, Histology, &c., occupy the first ninety pages of the book. Evolution has a short chapter of nine pages to itself.

Though the theories most in vogue are impartially stated, we notice frequent indications of the author's own views. These are by no means obtruded on the reader; indeed, the faith of the student in the orthodox theories will not be rudely shaken, even if a little doubt is occasionally infused into the complacent attitude produced by the teaching of most professors and lecturers. In treating of Sexual Selection, Mr. Thomson is inclined to lay more stress upon "the fundamental qualities of maleness and femaleness," than any selection by the female, a position taken up by Professor Geddes and himself in their "Evolution of Sex." He remarks that "it is difficult to understand how this characteristic smallness [of the males], though perhaps advantageous and likely to be favoured by natural selection, could be entailed on the male offspring only." Much harder to understand is sexual trimorphism, such as occurs in certain butterflies. We have cases where an insect has two kinds of females, one more or less like the male, the other "mimicking" another butterfly, supposed to be protected by a disagreeable taste.

The book is, in fact, to be decidedly recommended, not only to students preparing for examinations, but to persons already possessing a certain amount of knowledge of the subject, who desire to have the main facts of Zoology compressed into a single thoroughly readable volume. The only parts of the book with which we find fault are the illustrations; and this not on account of want of clearness, but from their inartistic style. They are roughly executed outline drawings arranged on thirty-two plates.

CATALOGUE OF THE PSITTACI, OR PARROTS, IN THE COLLECTION OF THE BRITISH MUSEUM. By T. Salvadori. Pp. xvii., 658. Pls. xviii. London British Museum, 1891 (1892).

THE twentieth volume of the British Museum Catalogue of Birds, a Monograph of the Parrots, by Count Salvadori, has recently been issued. The collection of Parrots in the British Museum is not so extensive or so nearly complete as that of the orders treated in the preceding volumes; and the Trustees have thus been fortunate in securing the services of an eminent specialist who is well acquainted with the genera and species in foreign collections that supplement the one on which the volume before us is particularly based. As the result of his researches, Count Salvadori presents ornithologists with a valuable work of reference, full of original matter, and admirably illustrated by a series of beautiful plates by Keulemans. At the same time, the author regrets his investigations add very little to previous knowledge of the mutual or phylogenetic relations of the various groups and families dealt with; and no satisfactory information on this subject can be expected until the anatomical work of Garrod is resumed and much extended.

There seems to be no longer any doubt that the Parrots are more closely related to the Owls than to the diurnal Birds of Prey. They form the Order Psittaci of modern classifications, and at the present time they are chiefly tropical birds, though some species extend as far as 42° N. lat. and 55° S. lat. They are specially distinguished from their allies by the well-known "zygodactyle" form of their feet (*i.e.*, two toes turned forwards, two backwards) and the extremely short, stout, strongly hooked character of the bill, which has a cere (frequently feathered) at its base, as in birds of prey. The upper jaw is moveable, being hinged on the forehead; and the tongue is thick and fleshy, sometimes fringed. The ring of bone round the cavity for the eye is often complete, and the septum between the right and left cavities of the nose extensively ossified. The feathers have aftershafts; the young are hatched helpless, without down or feathers, which successively appear. The eggs are white.

According to Count Salvadori, Parrots may be arranged in six families, three of which have the oral surface of the hook of the bill smooth, while the remaining three exhibit a file-like roughness of this surface. The first three families are those of the Nestoridæ (from New Zealand), the Loriidæ or "Lories" (from the Australian Region, except New Zealand), and the Cyclopsittacidæ (from the Austro-Malayan sub-region). These are mutually distinguished by the characters of the tongue and bill. The second three families, with file-like bill, are the Cacatuidæ or "Cockatoos" (ranging over the Australian Region and the Philippine Islands), the Psittacidæ, or "Macaws," "Parakeets," and true short-tailed "Parrots" (distributed throughout the intertropical regions), and the Stringopidæ (found only in New Zealand). Of these, the first two families have a complete sternum, while the Ground Parrot *Stringops*, as might be expected, possesses a sternum with a rudimentary keel. The Cockatoos have a crest of feathers on the head, and a complete orbital ring, and thus differ from the Macaws, Parakeets, and Parrots proper.

The detailed diagnoses of nearly 500 species are given by Count Salvadori, and 13 of the forms recognised are new.

HISTOIRE PHYSIQUE, NATURELLE ET POLITIQUE DE MADAGASCAR. By Alfred Grandidier.—Volume xvi. Histoire naturelle des Poissons. By Dr. H. E. Sauvage. 4to. Pp 543. With Altas of 63 plates. Paris: 1891.

THIS new instalment of M. Grandidier's admirable work, although bearing the date 1891 on its title page, was not received in this country until the end of last month. Most of the plates, without letterpress, had, however, been previously issued, and were reported in the *Zoological Record* for 1888.

Dr. Sauvage's work will prove of much service to the student of the fishes of the Indian Ocean, as the author, from the position he has held for several years in the Paris Museum, has had every facility to compare the types of the numerous species so inadequately described by Cuvier and Valenciennes, Liénard, and Guichenot, with the descriptions of modern ichthyologists. The figures accompanying his volume, although not comparable from an artistic point of view with those of the companion volumes on Mammals and Birds by such accomplished draftsmen as Messrs. Bocourt and Keulemans, are executed with care, and mostly from type specimens; they thus afford another source of welcome information.

For the student of geographical distribution, the present work also fails to compare in interest with its predecessors in the same series, through no fault of the author, but from the fact that the freshwater fish-fauna of Madagascar is a very poor one. In fact, apart from the very pronounced negative features of their fauna, the Chromides form the only group calling for special notice. This family, which appeared towards the close of the Cretaceous period in North America, is now confined to Central and South America, Africa and the neighbouring parts of Asia, and Madagascar. A few years ago, only two or three species were known from the latter part of the world; eight are now on record, referable to four genera, one of which approaches an African type, the three others showing decided affinity to forms now living in tropical America. The fish-fauna, therefore, poor as it is, affords further confirmation of the views held as to the relation of various other groups of animals, reptiles in particular, to South American types.

The negative features are particularly striking in the absence of Cyprinoids, so abundant in Africa, and Characinoids, a large group special to Africa and America.

Only two species are given as new in this work, one of which, described and figured as *Eleotris sikoræ*, is clearly an *Atherina*. Such an error seems inexcusable, as these two genera, belonging to widely different families, have scarcely anything in common. We are sorry to say this is not the only example of carelessness, *Hypoptychus dybowskii*, an Apodal Gadoid, being figured under the name of *Eleotris tohizonæ*, Stdr., from the mere fact that in Steindachner's original paper in the *Sitzungsberichte* of the Vienna Academy for 1880 a transposition of numbers took place on Pl. II.

THE OAK: A Popular Introduction to Forest-Botany. By H. Marshall Ward, M.A., F.R.S. 8vo. Pp. 175. London: Kegan Paul, Trench, Trübner & Co., 1892. Price 2s. 6d.

PROFESSOR MARSHALL WARD'S new work forms the third volume of the series of "Modern Science," edited by Sir John Lubbock. Starting with the acorn, the author claims to give "a short account of what is most worth attention in the anatomy and physiology of the

oak as a forest tree which has been so thoroughly investigated that we may confidently accept it as typical." If the reader is absolutely ignorant of scientific terminology, he will meet occasional stumbling-blocks, but granted a slight knowledge he will find the book an interesting and, for its size, a most thorough exposition of what is known about this familiar tree.

Chapter I. is introductory. Chapter II. deals with the acorn and the germination of the seed, and includes, under an account of the internal structure of the embryo, a clear definition of the cell as a member of a tissue, that is, as part of a whole and separated from its neighbour by a wall, which is a single structure common to both, and not the result of the juxtaposition of two separate walls. Chapters III.—VI. treat of the seedling and young plant. In Chapter III. the structure and properties of the young root are considered. There is a good explanation of the gradual development of the parts in acropetal succession, and a description of the various expressions of irritability to external stimuli, and their use to the organ in functioning as a holdfast and a collector of nutriment from the soil.

The following three chapters contain an account of the shoot-system, that is, all the structures developed from the plumule, or bud of the embryo. The arrangement of the skeleton of vascular bundles is first described and the longitudinal course of the latter illustrated. A chapter is devoted to the general outline of the stem, and excellent figures of sections in different planes have been borrowed from Hartig and Kny, though, unfortunately, the lettering by which the parts are indicated is so broken and indistinct as to be often quite illegible. If good figures are ready to hand, by all means let them be used; but surely there should be some method by which new lettering can be introduced, for the parts, especially in longitudinal sections of vascular tissue, are not always evident at first sight. The same remark applies to some similar figures in Chapter VIII., and also to a revival from Eichler's *Blüthendiagramme* in Chapter IX., where after looking in vain for x we find it to be an unknown quantity, and its place taken by a broken-down z.

The buds and leaves are next described; and there is a brief and simple account of the process of transpiration and the function of the green colouring matter, with its relations to sunlight and the assimilation of food. Here, as elsewhere, theory is avoided, the author confining himself to mere statement of the physiological facts as accounting for the various structures and tissue arrangement.

In Chapters VII., VIII., and IX., headed "The Tree," the adult root and shoot systems are considered with their mode of growth in thickness and the consequent changes leading to the formation of cork and bark. The interesting case of Symbiosis of the root and a fungus, forming the *Mycorhiza*, is noted and figured, and an explanation suggested. In Chapter IX. we are brought back again to the acorn, by an account of the inflorescence, flower, and fruit.

The next chapter is technical at times, giving in a short space a concise account of the structure and industrial peculiarities of oak-timber. The cultivation and the diseases and injuries to which the tree is subject form the matter of Chapter XI.; and Professor Ward must have found it difficult to confine his remarks on this head to a dozen pages, about half of which are occupied by some capital figures.

Finally, the relationships of the oak and its distribution in space and time are disposed of in less than five pages. The genus *Quercus* is of interest, in containing numerous closely connected varieties, a

fact which led De Candolle to regard it as a series of incipient species; for if the connecting forms died out, leaving some varieties more isolated than at present, the latter would be at once ranked as species. From the evidence of their fossil remains, together with the facts of their present distribution, "it is at least exceedingly probable that the European oaks, including our English oak, came into existence somewhere in the East, and that, after spreading from Asia towards the West, they are now slowly retreating before competing forms—*e.g.*, the beech."

A Short and Concise Account of the Eruptive Phenomena and Geology of Monte Somma and Vesuvius, in Explanation of the Great Geological Map of that Volcano constructed during the Years 1880 to 1888. By H. J. Johnston-Lavis, M.D. Small 8vo. Pp. 21. London: George Philip and Son, 1891.

Probably every geologist who has toiled over the rugged surface of a Vesuvian lava stream, or has tried to thread his way through the tortuous paths between the vineyards of its lower slopes, has felt the need of a geological map of the volcano and its immediate environs. Perplexed, moreover, by the intricate mingling of ashes, tuffs, lavas, and dykes belonging to different periods and eruptions, he has probably also longed for a work that would be more detailed than Lobley's and more modern than Phillips'. One of these desiderata has now been supplied by Dr. Johnston-Lavis's splendid large-scale geological map, and the other is promised shortly as a monograph of the mountain of which the pamphlet named above is a preliminary sketch. The map is on a scale of 1 to 10,000 ($6\frac{1}{3}$ inches to the mile): it is issued in sheets, each measuring 30 inches by 20. It is well printed and coloured, and its topography is more accurate than that of the Italian map on which it seems to be based. The colouring adopted contains some novel methods to express the slight variations in the nature of the soil which cause so marked a difference in its agricultural value. To assist reference from the monograph, the author has called several previously unnamed valleys after geologists who have worked on Vesuvius. Exact reference to the numerous dykes on the wall of the Atrio are now possible, as they have been marked by the great white numbers which tourists on the summit of the crater generally seem to mistake for advertisements. In the pamphlet accompanying the map, Dr. Johnston-Lavis gives a short sketch of the history of the volcano, and this he divides into four eras and nine phases, of which the first six were prehistoric. In the first era Vesuvius was an island, and probably ejected lavas of a more basic type than any formed in later times, and from which leucite was absent. In the second era the great mass of lavas, scorias, and ashes which now form the lofty precipitous wall overhanging the Atrio del Cavallo were produced. The third era commenced by a long period of inactivity, and ended by an explosive eruption, after which there was a series of eruptions, with periods of repose. The first historic, or the Plinian eruption of A.D. 79, occurred in this era; it destroyed Monte Somma and began to build the existing cone of Vesuvius a little to the south of the original centre of eruption. The last era began with the terrible eruption of 1631. A short account is given of the surface erosion, of which that performed by the *moyas* or mud streams of the northern slopes of Monte Somma is the most distinctive feature.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The new extension of the buildings of the University College, Bristol, is rapidly progressing, and will probably be ready for occupation by the Medical School in October next. A similar extension to accommodate the Engineering School is contemplated

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By the retirement of Dr. W. C. Williamson, F.R.S., from its chair of Botany, the Owens College, Manchester, loses the last of the original professorial staff appointed on its foundation in 1851. At first Professor of Natural History, Dr. Williamson was gradually relieved of his extensive duties by the growth and subdivision of the Biological and Geological faculties of the College, until in the end he was enabled to devote himself entirely to Botany, aided by the services of an Assistant-Lecturer. A farewell address was presented to Professor Williamson by Principal Ward on February 13, in the presence of a large gathering of colleagues and friends; and he has now removed his residence to London. The Professorship thus vacated has been filled by the appointment of Mr. F. E. Weiss, for some time Assistant-Professor of Botany in University College, London.

The British exhibit in the Botanical Gallery at the British Museum is steadily progressing. For more than two years the ferns and flowering plants of Great Britain, arranged according to Bentham's Handbook, have been exposed on sheets of stiff card, framed and protected by glass. The description is placed beneath each plant, so that collectors can compare their own specimens with the descriptions and specimens exhibited. The latter have been selected with a view to illustrate the usual form of the plant by an average-sized specimen; the rarer and often merely local forms are not included, and for these and the critical varieties, which are beyond the scope of the ordinary field botanist, the British Herbarium must be consulted. The mosses have since been added, arranged by Hobkirk's Synopsis. Descriptions of the species are included, and also, in the case of the smaller ones, a magnified coloured drawing of the whole plant and the fruit. Within the last month another addition has been made, the first instalment of a complete series of coloured drawings of the British Basidiomycetous fungi having been placed at the service of the public, in a manner uniform with the exhibits of the higher sections of the vegetable kingdom. The drawings at present shown comprise the Agarics, and number nearly 600. Mr. Worthington G. Smith was commissioned early last year by the trustees, on the recommendation of Mr. Carruthers, the keeper of the department, to make drawings of all the British Basidiomycetes for exhibition in the public gallery. Mr. Smith has lately revised Berkeley's outlines, and the series when complete will represent each of the two thousand odd species now included in this group. The drawings are life-size, but magnified representations of the smaller ones are also given. The exhibit is a most attractive one, and though only about one-fourth of the whole are as yet shown, the public are evidently already surprised to find so many different kinds of "mushrooms and toadstools," the collective name for the group with the non-scientific.

PARIS may well be claimed as the birth-place of palæontology, and we sympathise with the present eminent professor at the Jardin des Plantes, M. Albert Gaudry, when he pleads for the due recognition of this branch of science in the Paris Museum of Natural History. As the Professor remarks in the *Revue Scientifique* of February 20, the building at the Jardin des Plantes forty years ago was the finest museum of Natural History in the world; whereas to-day, except in the new galleries for Zoology, it is far behind most national institutions of this character. The palæontological collections are arranged for the most part in a temporary shed and in small rooms, with inconvenient cases. In many departments the series of specimens is unique, and it would be more than a national calamity if, by any accident, under existing circumstances, they should be damaged or destroyed. M. Gaudry institutes a comparison between the liberality of the British Government and that of the French Government in providing for the custody of its Natural History Museum, and we hope his claim for further support from the French Treasury will meet with the attention it deserves.

THE Manchester Museum, Owens College, is issuing a series of small handbooks. The "General Guide," by Mr. W. E. Hoyle, is based on the plan of the British Museum Guide Books, well illustrated, and published at the low price of twopence. It will suffice admirably for ordinary visitors, and is in nearly all respects well up to date. We would only point out that the "giant Ant-eater" (*Macrotherium*) of the "Mid-Meiocene" age was several years ago proved to be a myth, and it is thus misleading to reproduce Dawkins' outline restoration of the animal. Professor Marshall's "Outline Classification of the Animal Kingdom," price one penny, is much less entertaining, being a mere list of names of groups, intended as an index for students. The same Professor's "Descriptive Catalogue of the Embryological Models," price one shilling, is a most valuable little handbook, and deserves to find a place in the library of every zoologist who wants a concise epitome of the stages in the development of some of the leading types of animal life. We presume it contains something of the essence of Professor Marshall's forthcoming Manual of Embryology.

THE usual series of Museum Lectures at the Owens College, Manchester, is in progress this session. In addition to the short courses by the Professors of Zoology, Botany, Geology, and Mineralogy, three free holiday lectures are undertaken by Mr. W. E. Hoyle, Keeper of the Museum. On the morning of December 26th, Mr. Hoyle addressed a large audience, chiefly children, on "Unnatural History." The subjects announced for Easter Monday and Whit Monday respectively are "Strange Fishes" and "Elephants (ancient and modern)."

THE annual meeting of subscribers to the Dorset County Museum was held on January 20th last, when the chairman of the Council, Mr. J. C. Mansel-Pleydell, F.L.S., contributed a short summary of progress. One of the latest additions to the museum is a case illustrating, both with specimens and photographs, Mr. Mansel-Pleydell's discovery of the Pliocene elephant (*Elephas meridionalis*) at Dewlish, Dorset.

AN important extension of the Leicester Town Museum has just been completed, and the collections are now being re-arranged in accordance with modern methods. A convenient laboratory and workshop has been provided for the Curator and his assistants beneath a new room specially built for the Leicester Literary and Philosophical Society.

AT the meeting of the Liverpool City Council on March 2, the retirement of Mr. T. J. Moore, A.L.S., from the curatorship of the Free Public Museum was announced. Mr. Moore's term of service extends over nearly forty years, and his many friends will regret to learn that failing health prevents his continuing an active interest in

the cause of Natural Science for which he has done so much. Mr. Moore was awarded a well-earned annuity, and it is hoped that, by an arrangement with the Derby trustees, he will still be able to undertake the duties of Consulting Curator, and thus prevent the necessity of appointing a successor for the present. We are glad to add that, notwithstanding his advanced age, the Rev. H. H. Higgins is still able to take an active part in the affairs of the Museum, and has lately obtained several important acquisitions.

We also regret to announce the retirement of Major John Plant, F.G.S., who has occupied the position of Curator and Chief Librarian of the Peel Park Museum, Salford, since its foundation in 1849. Under Major Plant's direction this institution has gradually developed into one of the principal attractions of the busy centre to which it belongs. The library now contains some 60,000 volumes, in place of the 5,000 with which it started, and is provided with a handsome reading room. The rooms in which the scientific exhibits are displayed are numerous and well adapted to their purpose; while the art galleries include the great Langworthy Gallery, occupied with marble statues and fine oil paintings of the modern English and French schools. Apart from his official work, Major Plant has taken an active part in the proceedings of the Manchester Geological Society and other local institutions; and, by his retirement to Anglesey, a leading figure will be removed from Manchester scientific circles. He is succeeded at Peel Park by Mr. B. H. Mullen, M.A., late of the Science and Art Museum, Dublin.

We have received a brief report of the work of the Essex and Chelmsford Museum for 1891. A successful effort is being made to establish this institution as a centre of intellectual activity, not only for Chelmsford, but also for the whole of the county. Its work is chiefly in the direction of advanced teaching, but the practical lessons learned in the Field Lectures on Geology and Botany, by Mr. W. H. Dalton and Dr. J. E. Taylor, are sure to tend to the progress of Natural History research in the county.

Mr. Arthur Smith Woodward has been appointed Assistant-Keeper of the Geological Department of the British Museum, in succession to Mr. R. Etheridge, F.R.S.

Mr. J. Scott Keltie has succeeded the late Mr. H. W. Bates as Assistant Secretary of the Royal Geographical Society.

The volume of the Palæontographical Society's Monographs for 1891 has just been issued. It comprises continuations of the Monographs on Jurassic Gasteropoda, by Mr. W. H. Hudleston, F.R.S.; on Inferior Oolite Ammonites, by Mr. S. S. Buckman; and on the Devonian Fauna of the South of England, by Rev. G. F. Whidborne. The imposing list of memoirs "in preparation" and "in course of publication," appears as usual; but we have good reason to believe that the large majority of these are no further advanced than they were many years ago when the titles were first announced. As, however, at least half the names on the list of the Society's Council are quite unknown in the sphere of Palæontological Research, we must not expect too much.

In the last number of the *Proceedings of the Royal Society of Edinburgh* (vol. xviii., pp. 299–302), the Prince of Monaco gives an interesting brief account of his new yacht, the "Princesse Alice," in which he hopes to continue his researches in Oceanography and Marine Biology.

The local use of various Building-stones formed the subject of the Presidential Address to the Rochester Naturalists' Club, by the Rev. G. M. Livett (*Rochester Naturalist*, vol. ii., 1891, pp. 33–46). Noting first the use of Greywethers in the construction of cromlechs, such as Kit's Coty House, he proceeds to call attention to

the use, by the Romans, Saxons, and early Normans of calcareous tufa or travertine. There are records of tufa having been quarried in Kent, but the amount yielded is not likely to have been great. It is probable that the Romans, and also the Normans, imported the rock; but both Saxons and early Normans seem to have quarried a good deal of the material from Roman buildings. Tufa was used in the piers and attached shafts of the crypt of Rochester Cathedral. The successive use of other stones, such as the Caen stone, Purbeck and Sussex Marbles, Chalk, Upper Greensand, and Kentish Rag, are noted with especial reference to their employment in different parts of Rochester Cathedral. One other rock is mentioned, and that is a beautiful stalagmitic rock, of which shafts still remain in the ruined chapter-house of Rochester. It is pointed out that the chapter-house was built by Ernulf, Bishop from 1118 to 1124, and that the same stone was used at Canterbury, where Ernulf was previously Prior, and also at Peterborough, where he was Abbot. This stone was probably imported; but it is not known from whence.

THE Yorkshire Geological and Polytechnic Society has just issued its *Proceedings* for 1891, being the first part of Vol. XII., comprising 130 pages, with two plates of fossils and two meteorological tables. There are seven papers specially relating to the county, namely: (i.) On the present state of our knowledge of the Yorkshire Calamitæ, by Thomas Hick; (ii.) The mode of deposition and properties of the Carboniferous strata of Leeds and its immediate suburbs, by B. Holgate; (iii.) Evidence of Glacial action near Leeds, by James E. Bedford; (iv.) On a Permian Conglomerate Bed at Markington, by Rev. J. Stanley Tute; (v.) Exploration of the Elbolton Cave, by Rev. E. Jones; and two meteorological papers, one (vi.) by R. Reynolds, the other (vii.) by J. McLandesborough and A. E. Preston. Among other papers Mr. C. E. De Rance contributes a long and statistical account of the water-bearing strata and underground waters of Lincolnshire. There are also four general palæontological contributions, namely: (i.) The Hybodont and Cestraciont Sharks of the Cretaceous Period, by A. Smith Woodward, with two plates; (ii.) Notes on some new or but little known Eocene Polyzoa, by G. R. Vine; (iii.) British Palæozoic Ctenostomatous Polyzoa, by G. R. Vine, with two plates; and (iv.) On the affinity of *Dadoxylon* to *Cordaites*, by James Spencer. A good bibliography of geological papers relating to Yorkshire during 1889-90 is added by the Editor, Mr. James W. Davis.

THE subject of the Kentish coalfield was again brought before the Manchester Geological Society by Professor Boyd Dawkins on January 29, and specimens of the coal obtained from the Dover boring were exhibited. 762 feet of Coal-measures have now been pierced, with an aggregate thickness of more than 17 feet of coal in nine separate seams, mostly workable. Seam No. 1 (2 ft. 6 in.) occurs at a depth of 1,140 ft.; and seam No. 9 (1 ft. 8 in.) was met with at a depth of 1,875 ft. Arrangements for the sinking of a shaft are now in progress. With regard to the rumoured discovery of coal in Northamptonshire, we have the authority of Mr. Beeby Thompson for saying that true Coal-measures have not been reached by any boring at present made in the county.

THE *Report and Proceedings of the Belfast Natural History and Philosophical Society*, Session 1890-91, just issued, does not contain any original contributions to Natural Science, except a brief record of a local geological section exposing a basalt dyke. The Committee for recording the Fauna of Ulster report good progress in their work.

THE second part of the *Journal of the Institute of Jamaica*, issued in February, contains much news of local interest, but few contributions to the general advancement of Natural Science. In the reports of meetings, Mr. John Stuart comments favourably on the work of the temporary Biological Station established by the Johns

Hopkins University last summer at Port Henderson, and makes another communication on "Some Physiographical Features of Jamaica produced by the agency of Water." Among the scientific papers Mr. T. D. A. Cockerell publishes a technical "First Contribution to the Entomology of Bath, Jamaica"; and there are shorter notes on entomological subjects.

We are glad to welcome the *First Report of the Southport Society of Natural Science*, for 1890-91. The Society, as we note from Dr. Vernon's Presidential Address, was inaugurated in November, 1890, and has already done some valuable local work. The Report contains the President's Address on the Material and Educational Utility of Natural Science; Problems in the Geology of the Neighbourhood of Southport, by E. Dickson, F.G.S.; general reports of meetings, and a list of members. It concludes with an appendix, consisting of three important Reports as follow:—A List of the Mollusca of Southport and District, by G. W. Chaster, M.R.C.S.; Botany of the Southport Society of Natural Science District, by Henry Ball; and a Report upon the Foraminifera of the Southport Society of Natural Science District, by G. W. Chaster. The last report is enriched by a carefully drawn plate, and contains facts and observations of considerable interest.

The fourth annual meeting of the Australasian Association for the Advancement of Science was held at Hobart Town, Tasmania, early in January last. In his Presidential Address, Sir Robert Hamilton made an eloquent appeal to the people of Australasia to foster the growth of science and scientific methods in the Colonies. In Natural Science, Professor T. W. E. David presided over the section for Geology and Palæontology, and Professor Baldwin Spencer over that of Biology; while Captain Pasco, R.N., occupied the chair in Section E (Geography), and the Rev. Lorimer Fison presided over the Anthropologists in Section G. Professor David addressed the Geologists on Volcanic Action in Eastern Australia and Tasmania. So far as known, the facts indicate that volcanic action in these regions has occurred most frequently after periods of continued subsidence culminating in a re-elevation of the land by way of compensation. It appears that subsidence preceding volcanic activity is often due to the pressure of the accumulation of thick masses of sediment. Mr. Clunies Ross contributed papers on Coral Reefs and on *Lepidodendron* from New South Wales. The value of photography in geological work was also discussed, and there were many minor papers. In his Presidential Address, Professor Baldwin Spencer treated of the origin and development of the Fauna of Tasmania, briefly describing its present characters. Professor F. W. Hutton reiterated his views as to the derivation of the flightless birds of Australasia from flying birds related to the Tinamus of South and Central America; and there were many other papers, both on Zoology and Botany, chiefly dealing with Australasian subjects. Captain Pasco's address to the Geographical Section included a history of some of the earliest explorations in Australia, and referred, in conclusion, to the importance of undertaking further researches in the Antarctic regions. Mr. J. P. Thomson discussed recent explorations in British New Guinea; and Mr. D. Murray read extracts from the letters of Mr. Lindsay, who is exploring new regions in Western Australia. In his address, Mr. Fison made some general remarks on the method of studying aborigines, and concluded with a reference to British New Guinea as a promising field for important additions to anthropological science. Papers in this section seem to have been numerous and of wide interest. A lengthy report of the meeting will be found in *Nature* for March 3.

An Alpine Club has been inaugurated in Odessa, having for its object the scientific exploration of the mountains of the Crimea. The new Club will publish original memoirs, and will also increase the facilities for tourists visiting the little-known southern uplands of Russia.

CORRESPONDENCE.

Butcher's Broom.

The Butcher's Broom, *Ruscus aculeatus*, flowers in March, but in the years 1884, 5, 6, 7, 1890, 1 it was flowering freely in November in Sussex and Hampshire. In November, 1888 and 1889, I was in London and could not observe it. Is this an instance of the premature opening of flower-buds that should lie dormant till next spring, or has the plant two flowering seasons in the year? Only a small number of the plants, perhaps one in fifty, produce any fruit, and it is difficult to find a bush bearing as many as a dozen berries. The November flowers seldom if ever produce fruit, the November-ripening berries being formed by the March flowers. Is the scarcity of fruit in this country connected with the premature opening of most of the flowers? Whatever may be the reason of this double flowering season, it seems to be a marked instance of the non-adaptation of a plant to present climatic conditions. Clement Reid.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

Canker in Fruit Trees—(*W. H. Shrubsole, Sheerness*).—The varied and often extensive malformations to which the term "canker" is applied are not directly associated with any individual active agent. There is not the definite relation between the disease and a certain germ which obtains, for instance, in the potato-disease. The primary cause is an injury to the cambium layer, resulting in cessation of growth at the point affected. The injury is often inflicted at a very early age, and may be due to atmospheric causes, such as frost or hailstones, which have split or bruised the tender cortex, or to piercing by an insect, or some other accident. For instance, the disease often starts from the base of a branch which has died or become broken off, exposing the internal tissue.

The cambium ceases to produce wood and bark at the injured part, thus causing a break in the annual ring. If only atmospheric causes have come into play the sore may be small, and soon healed over by the growth of "callus" or healing-tissue from the margin of the wood. Frequently, however, complications ensue. The damp exposed tissue, full of nutritive sap, offers a favourable nidus for the germination of fungus spores. The resulting mycelium spreads through the surrounding cambium living upon and destroying the cells. Every year the parasite will spread further, remaining dormant through the winter as resting spores, while the efforts of the plant to heal over the gradually widening wound will lead to the formation of great lips of callus. The parasite often exercises a stimulating effect, causing, as in the case of insect-galls, hypertrophy of the surrounding tissues. The following are some of the principal works affording information on the subject:

1. Handbuch der Pflanzenkrankheiten. By P. Sorauer. Two parts. Berlin: Paul Parey, 1886. (A general text-book of plant diseases.)

2. Lehrbuch der Baumkrankheiten. By R. Hartig. Edit. 2. Berlin: Springer, 1889. (Deals with the diseases of trees.)

3. Timber and some of its Diseases. By H. Marshall Ward. London: Macmillan & Co. (Nature Series), 1889. (A small popular treatise.)

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NOTES AND COMMENTS.

Private Collections.

In March we alluded briefly to the interest of Mr. Du Cane Godman's Presidential Address, recently delivered to the Entomological Society of London. The Address has now been printed, and we make the following extract as worthy of special notice far beyond Entomological circles :—

"Working entomologists, apart from field workers, may be divided, for my present purpose, into two classes, viz., those who are connected with our museums and make the subject their profession and those who are not so employed, but who pursue Entomology at such times as they can spare from their regular business of life. It is the latter, being the more numerous body, who have contributed much more extensively to our present stock of entomological knowledge. Their work has been chiefly based upon their private collections, formed partly from old sources, and partly from new materials often collected by themselves, or brought by field naturalists from all parts of the world. These collections hardly ever have any permanent resting place in private hands; very seldom indeed does a son take up the work of his father; they either change hands, are dispersed, or become—their happiest fate—absorbed in a public museum. This system, which has gradually grown up, is, I think, likely to change, as the extent of collections is, under modern requirements, largely on the increase. This growth of collections renders the subject studied far more complicated, and the difficulty is likely to increase tenfold. The effect is already plainly visible, for private collections are becoming rapidly absorbed by museums, or by a much smaller number of private collectors, with whom they can only temporarily remain. This absorption of private collections by public museums is likely to continue as regards Entomology, as it has in other subjects. It certainly has done so with plants, as now hardly a private herbarium exists in this country—nearly all have

been absorbed by Kew. So also with birds; a large portion of the private collections of a few years ago are now being concentrated in the British and other museums.

"What, then, will be the effect of this concentration of collections in public museums on the number of systematic working entomologists other than the museum officials? Under existing circumstances, I am inclined to think the number will decrease. The necessity at the present time of examining large series of specimens of any species from as wide a range of localities as possible, so that the variation and distribution of the species can be traced, has increased the bulk of collections to an enormous extent. In former times a pair or two of a species was considered enough to represent it in a collection, but now we find that 40 or 50, or even 100 specimens are necessary to show the stability or instability of a species, its range, and all the many points connected with a satisfactory comprehension of its limits. All this vastly increases the cost of forming and preserving new collections, and is against the private collector undertaking the task, and the burden of the work of determining and classifying will be more and more thrown upon the museum officials. That the number of private workers at systematic Entomology should decrease is distinctly to be deplored, and in view of the tendency of collections to become amassed in museums, it follows that it is chiefly to the arrangements made in those museums that this evil is to be arrested.

"As we all know, when once a specimen becomes the property of the Trustees of the British Museum it must never pass out of their possession. Nothing, then, can be seen out of the museum hours, and entomologists who have their daily business to attend to can only visit the museum at rare intervals. They do this to a great extent now in order to determine specimens in their own collections; but when the latter are, from reasons already given, no longer made, the subject will cease to be studied by them. I am inclined to think this can be obviated to a great extent in a way that will greatly benefit the museum and all concerned. There are, no doubt, a considerable number of specimens, such as types, &c., which, on account of their special value, should never leave the museum under any circumstances; they are too valuable to be placed in any risk of loss. Besides these there is by far the greater part of the museum collection which falls into a different category. I refer to specimens which are either unnamed (a very large number), or, if named, are not types, and which in their existing state do not possess any special scientific value, not having had any work bestowed upon them. All such specimens could, at a very slight risk, be entrusted to competent specialists not officially connected with the museum to be named and prepared either for incorporation into the museum series or assigned to the named duplicates for future distribution to other institutions. No doubt additional work would be necessary on the part of the museum officials in preparing such series for examination; but this labour would be more than counterbalanced by the work performed upon the specimens when they are returned named to the museum, and ready for incorporation into the general collection.

"The extent of the subject of Entomology is so vast that nothing but a systematic and continuous effort to amass collections, work them out, and preserve them, can place us in a position to proceed safely with the larger questions which follow the initial step of naming species; and it will only be by the steady effort of our museum

officials, not only to work at the subject themselves, but to enlist the aid of every available outside worker, that real substantial progress can be made. The talents of the outside public can only be fully used by a system by which specimens can be placed in the charge of any competent worker to be determined by him at his own home, and at his leisure, for a certain definite time, and under well understood rules. The benefit to the museum collections under a system of this kind is obvious. The great mass of unnamed materials which we believe now exists in the Museum would be gradually worked into order; and, on the other hand, the inducement of having a good series of specimens to work at, and in a way that suits the circumstances of the outside workers, will certainly tend to increase their numbers, to the great benefit of the study of Entomology."

Mr. Godman speaks with authority as one of the most distinguished of those "unattached" workers on whose behalf he so justly and forcibly pleads. We cordially endorse his sentiments from the point of view of many other branches of Natural Science, to which his remarks are equally applicable. The rules of the British Museum have the sanction of antiquity, have worked well on the whole thus far, and are scarcely likely to be changed; and this notwithstanding the fact that our National Museum is unique in the world, so far as we are aware, in denying its custodians all powers of discretion in dealing with applications for the loan of materials. We confess, however, to a more sanguine view than that of Mr. Godman on the subject of the private collections of the future. No officialism can entirely extinguish the independent worker, whose mainspring is his enthusiasm, and whose base of operations lies in his collecting. We believe the tendency of modern research is merely to limit the scope of such collecting, and we hope and trust that as progress demands further attention to details the aims and methods of collectors will be modified accordingly. By far the largest proportion of our national collection is due to the individual efforts of independent non-professional naturalists contributing their several quota.

The Exploration of Matto Grosso, Brazil.

We have been favoured by Dr. J. W. Evans and Mr. Spencer Moore with some brief notes on their recent expedition to the remote Brazilian province of Matto Grosso, to which we referred last month. The exploration was undertaken under the auspices of a syndicate interested in the reputed mineral wealth of Matto Grosso, and its partially scientific character is believed to have been due to the influence of the late Emperor, Dom Pedro II. Messrs. Evans and Moore proceeded *viâ* Buenos Ayres and the River Parana to Cuyabá, the capital of Matto Grosso, and under the able guidance of Mr. Whitehorn, an English gentleman settled in Paraguay, they at once started for the Chapada plateau, the escarpment of which, situated some miles to the east of Cuyabá, is estimated to reach the height of over 2,000 feet above sea-level. This plateau owes its existence to the

superposition of sandstones upon slates more easily affected by denudation; the former rock, from its containing a few obscure Brachiopod fossils, discovered by Mr. White, a naturalist who spent some time in the district, is considered by Dr. Derby to be of Devonian age. The country between Cuyabá and the plateau is of the open kind known as *monte*, various species of *Ochna*, *Erythroxylon*, a *Cochlospermum* in August leafless and one mass of golden bloom, the Vochysiaceous tree *Salvertia convallarioides*, Bignonias, Bromeliaceæ, &c., being conspicuous objects in its vegetation. A stay of two days was made at the village—now much decayed—of Sta. Anna da Chapada, after which the party returned by easy stages, securing specimens of the rocks and Natural History objects by the way.

A visit was subsequently paid to Santa Cruz, followed by a journey in a north-west direction through the forest whence the province derives its name, and where is found a great part of the Ipecacuanha which reaches Europe. After considerable difficulty the forest was traversed, and the party emerged upon the fine treeless Campos de Tapirapuan, which are flanked by densely wooded hills (Sierra de Tapirapuan) running east and west. The existence of an olivine basalt in these hills was the most interesting discovery made hereabouts. Returning to Santa Cruz, the party devoted ten weeks to the examination of that district, and the naturalists made a large collection of the rocks, plants, and animals, while Mr. John Storm devoted himself to the correction of the faulty maps of this part of Brazil.

Notwithstanding the early and unexpected recall of the expedition, we are glad to learn that some results of much scientific interest may be expected from the study of the collections brought to Europe.

Ants and Mites.

The curious habit which ants have of harbouring in their nests a variety of other insects, is a well-known fact. The reason for this singular exercise of hospitality is by no means always apparent; in some cases, however, it does appear to be fairly clear, particularly in the case of certain mites (Gamasids), whose habits and customs are treated of by Mr. A. D. Michael in the recently published part of the *Proceedings* of the Zoological Society of London. The author of this paper investigated a number of ants' nests in Corsica and in the neighbourhood of Innsbrück, and in many of these nests there occurred various species of Gamasids, whose relation to their host formed the subject of the enquiry dealt with in the paper. The nests of a small yellow ant, *Tetramorium cæspitosum* var. *meridionale*, were infested with two kinds of Gamasids. One species, which Mr. Michael describes as new, under the name of *Lælaps equitans*, was not only found in the nests, but also upon the ants themselves; and

when the nests were disturbed the mites, being slow of foot, leaped on to the head of a passing ant, and were borne off to a place of safety. The ants appeared to have not the slightest objection to this familiarity on the part of their guests; on the contrary, indeed, for they carried off the mites without making the least attempt to dislodge their riders, and ants are not as a rule the most peacefully disposed of animals. In the case of another species of ant, the care taken of the Gamasids was even more remarkable. When danger threatened the colony, the ants carried off both the mites and their young, just as they carry off their own young. After a careful series of experiments, Mr. Michael comes to the conclusion that the mites repay the hospitality shown to them by removing the bodies of deceased ants, which they utilise as food.

South American Extinct Mammalia.

In a recent issue of the *Anales d. Mus. Nat., Buenos Aires*,[1] Dr. Hermann Burmeister continues his important researches on the fossil mammals of Argentina. Dr. Burmeister is one of those designated under the somewhat inelegant title of "lumpers"; and, although he may perhaps err somewhat in the opposite direction, he is certainly doing good work in calling attention to the reckless manner in which so-called genera and species have been named by some of his fellow-workers. Indeed, when, according to their own showing, no less than five genera, to say nothing of species, have been manufactured by the latter out of remains belonging to *Nesodon imbricatus* and *N. ovinus*, while Dr. Burmeister adds three more to the list, it is high time that something were done to check this endless flow of names—names in many cases no sooner published by one writer than they are relegated to the rank of synonyms by another. In addition to showing the absolute invalidity of some of these names by actual comparison of the specimens on which they were founded, our author in other cases shows that for physical reasons it would be an absolute impossibility for such a number of gigantic animals as have been named to exist within the limits of the country.

A large portion of the memoir is devoted to a description of the Ungulate genus *Nesodon*, a near ally of the huge *Toxodon*; but we have also notices of other interesting forms. Among these is the occurrence of a species of the Edentate genus *Megalonyx* (as typified by *M. jeffersoni*, of Kentucky) in the Tertiaries of South America, which is a point of considerable interest in distribution. Not less important from another aspect is Dr. Burmeister's statement that the alleged occurrence of enamel in the teeth of certain of the Ground Sloths of the Lower Tertiaries of Argentina is due to osteo-dentine having been mistaken for enamel. By this we have at a single stroke the genera *Promegatherium* and *Promylodon* severally united with *Megatherium* and

[1] Vol. iii., pp. 401–488, pls. viii.–x. (1891).

Mylodon; and the range of the two latter is consequently carried much further back in the Tertiary period.

The remaining portions of the work are mainly devoted to criticisms of genera and species founded by writers in Europe, the majority of which are regarded as invalid.

It is, perhaps, too much to expect that Dr. Burmeister's caustic criticisms will at once check the reckless multiplication of names now going on in South America; but it will at least have the effect of drawing attention in Europe to the serious nature of the evil. Palæontologists in Argentina have a magnificent field before them, but it is not too much to say that some of them have so involved the subject in a wilderness of meaningless terms, that many European workers are repelled from giving to the fossil mammals of Argentina the attention they so undoubtedly merit.

Referring to the manner in which extinct mammals are named in the New World, we may direct attention to the following paragraph from a paper by Professor Marsh in the April number of the *American Journal of Science*, p. 351 :—

"The oldest ancestor of the horse, as yet undiscovered, undoubtedly had five toes on each foot, and probably was not larger than a rabbit, perhaps much smaller. This hypothetical predecessor of the horse can now be predicated with certainty from what is known of the early hoofed mammals. It may be called *Hippops*, and its remains will be found at the base of the Tertiary, or more likely in the latest Cretaceous."

We have known cases of the description of genera and species by telegraph, and we have unfortunately had some experience of the ordinary struggle for "priority" in nomenclature; but we have never yet met with anything equal to this latest development. Who, we should like to know, is to decide which of the numerous small five-toed hoofed animals that will probably be discovered someday at the base of the Tertiaries must bear the name of *Hippops?*

An important memoir on the Oligocene mammals of the so-called Siderolites of Egerkingen, near Soleure, Switzerland, is contributed by Professor L. Rütimeyer, of Basle, to the publications of the Swiss Palæontological Society.[1] It is largely owing to the previous researches of the Professor that our knowledge of this interesting and prolific fauna is due; and the present memoir affords much additional information. The illustrations are excellent, and ought to aid materially in comparisons with specimens from other localities. Several forms previously known only from the nearly contem-

[1] "Die Eocäne Säugethier-velt von Egerkingen," *Abh. schweiz. palæont. Ges.*, vol. xviii., pp. 1–153, pls. i–viii. (1892).

poraneous Phosphorites of Central France, are now identified from the Siderolites; while of still wider interest is the identification of certain Swiss forms—especially Lemuroids—with genera from the Eocene of the United States. We note that the Professor does not accept the identification of the Lemuroid from the French Phosphorites described as *Necrolemur*, with *Microchœrus* of the English Oligocene; and it seems probable that some emendation will have to be made in regard to the mammals he refers to the genera *Hyracotherium* and *Hyopotamus*. Some interesting observations are recorded as to the homology and nomenclature of the component elements of the molar teeth; and the Egerkingen fauna is carefully correlated with other faunas of the same age. We must venture to take objection to the title page of the memoir bearing the date of 1891, when it is perfectly clear from internal evidence that it was not published until the present year was far advanced.

In the February number of the *American Naturalist* just received, Professor Cope discusses the affinities of the so-called "Marsupial Mole" (*Notoryctes typhlops*) from Australia, lately noticed in Natural Science (p. 36). He considers that its relationship to the Marsupials is not proved, and suggests that it ought rather to be placed with the Insectivora. He especially emphasises the resemblance of *Notoryctes* to the South African *Chrysochloris*, and thinks that the Notoryctidæ may belong to one of the groups required to connect the marsupial and placental mammals. He adds that the existence of a South African type of placental mammal in Australia is not surprising, since the freshwater fish *Gonorhynchus greyi* is common to both countries, and the ratite birds and pleurodire tortoises are found in both.

The subject of the so-called "tailless" trout met with in several of the Scottish lakes is discussed again by Dr. R. H. Traquair in the April number of the *Scottish Annals of Natural History*. The tail-fin in these trout, instead of being forked and fan-shaped, is rounded and degenerate. Many new facts are added, and there are several beautiful figures; but the explanation of the strange phenomenon is still as mysterious as ever.

Captain Chaves, of the Portuguese Army, who has made a special study of the fauna of the Azores, has lately found a very remarkable crustacean off the Isle of San Miguel. It is a Mysid of remarkable appearance, covered with an overhanging carapace of very large size (8 or 9 millim.) compared with the total length of the animal (12 millims., not including antennæ). Under this carapace, which is armed with spiny processes and characteristic ridges, the

limbs, abdomen, and tail of the animal are completely covered when at rest. The latter peculiarity suggested the name of *Cryptopus*, given to these aberrant Mysids by Latreille in 1829. They do not appear to have been seen again for more than fifty years. M. Chaves discovered them in the stomach of a dolphin-fish (*Coryphæna equisetis*). There are several specimens of the same species (*Cryptopus defrancii*) in the British Museum, and Dr. J. E. Gray described them in 1830 as the type of his genus *Cerataspis*; these fragments were obtained from the stomach of a dolphin off the coast of Brazil.

The discovery of an elasipodous Holothurian in the Southern Mediterranean has just been announced by Dr. E. von Marenzeller, before the Imperial Academy of Sciences of Vienna. It is a colourless species, scarcely 20 millims. in length, belonging to the family of Elpidiidæ, and apparently approaching the genus *Kolga*. Numerous examples have been taken in three localities, at depths of 798, 943, and 1,292 metres, but none are well preserved. The discovery is of great interest, for the known Elasipoda are restricted almost exclusively to the cold polar seas and the depths of the ocean: a single exception to the rule is mentioned in the "Challenger" Report, vol. iv., where *Uyodæmon maculatus* is recorded from the Philippines in water at a temperature of 21° C.

In the last part of the *Quarterly Journal of Microscopical Science*, Mr. Beddard describes the structure of a new genus of Oligochæte worm, named *Branchiura* on account of its nearly unique peculiarity of possessing gills. The worm was found in the "Victoria Regia Tank" of the Botanical Society's Gardens, in company with another very remarkable worm, lately recorded from Madras by Professor A. G. Bourne. The latter, which is a Naid, has also been quite recently described in the same journal; it possesses outgrowths in the anterior region of the body which enclose the bristles or setæ, and are probably gills in function, though they appear to suggest the parapodia of the marine Chætopods. In *Branchiura* the gills are limited to the hinder segments of the body, and are dorsal and ventral in position. There is a pair to each segment; they are contractile and recall the cirri of certain marine worms. The only other Oligochæte known, besides the two just mentioned, which agrees with the Polychæts in having gills, is *Digitibranchus niloticus*, recorded a great many years ago from the Nile by the late Professor Grube. Unfortunately the description of this creature is rendered nearly useless by the absence of any account of its structure; it is impossible to be certain that it is not after all a Polychæte. *Branchiura* belongs to the family Tubificidæ, typified in this country by the common slender red worm, *Tubifex rivulorum*, so abundant in rivers, ditches, and ponds.

We understand that Mr. W. H. Hudleston, F.R.S., has in the press a "Catalogue of the Gasteropoda of the Inferior Oolite Formation." It will be modelled on Morris's "Catalogue of British Fossils," published in 1854. The mention of Morris's Catalogue reminds us of a Committee that was formed at the author's death to perpetuate his memory by publishing a new edition of this well-known work. The labours of that Committee will be considerably reduced if they remain in obscurity much longer, for the vertebrata were some time ago taken off their hands, and soon they will be relieved of part of the invertebrata.

A "Sketch History of Marlborough in Neolithic Times" (read before the Toynbee Hall Natural History Society, on their visit in 1891) has just been issued by Mr. F. J. Bennett. Referring to the lynchets or cultivation-terraces met with on the White Horse Hill, also near Collingbourn and Lambourn, he expresses his agreement with Mr. Gomme that they were formed independently of the plough, and, indeed, before its invention. He cites instances where the lynchets must have been cut out of the hill sides, the retaining-walls having been faced with flints or sarsen stones.

Notes on the rock-fragments that occur in the Permian Breccia of Leicestershire are communicated by Professor T. G. Bonney to the *Midland Naturalist* for February and March. The specimens, which were collected by Mr. W. S. Gresley, include, as might be expected, many of the Charnwood rocks, but in addition to the fragments from these old and possibly pre-Cambrian formations, there are some from the Carboniferous Limestone and (probably) Millstone Grit, and many from the Coal-measures. The evidence afforded by these rock-fragments of unconformity between the Coal-measures and Permian strata, confirms and supplements that obtained by Mr. Horace T. Brown and published by the Geological Society in 1889. Certain striated fragments have been found in the Breccia. These, in Professor Bonney's opinion, might in some cases have been scored by other rock-fragments; but in no instance is he convinced that ice-action had any connection with the phenomena. Nevertheless, referring to the general characters of the accumulation and to certain boulders that have been found in the Permian beds at Coton Park, he admits that if we are unconvinced by the striæ, the general evidence of the breccias of this Leicestershire district seems favourable to the idea that a rather low temperature prevailed in Permian times.

The subject of Mean or Average Rainfall was brought before the Institute of Civil Engineers on March 22 by Mr. A. R. Binnie.

He came to the conclusion that the laws which govern the fluctuations are, apparently, of general application, and must be sought for in some cause, yet unknown, but yet common to the whole world. The facts he had tabulated showed that when the mean rainfall exceeded 20 inches, the geographical position of stations had little to do with the variations.

THE first number of a new botanical work, *Phycological Memoirs*, edited by Mr. George Murray, has just been issued by Messrs. Dulau & Co. The object of the publication is to keep "within the limits of one book the results of researches on Algæ in the Department of Botany of the British Museum." Further parts are promised at about half-yearly intervals, and will in time complete a volume containing about 50 plates. "In addition to the promotion of research, the authors of the memoirs have the object of making better known the treasures of the Museum which they have received facilities to investigate." The new venture promises well, judging from Part I. In the first paper, Miss Mitchell and Miss Whitting describe the type of a new Natural Order of Algæ, allied to the Fucaceæ and Laminariaceæ. Next follows the description of a fossil alga from the Oolite, by Mr. Murray. The third paper, also by Mr. Murray, deals with the structure of *Dictyosphæria;* and there are important contributions by Miss Barton and Mr. E. A. Batters.

I.

Dr. Weismann's Theory of Heredity applied to Plants.

THE essential point of Dr. Weismann's theory of reproduction is the assumption of a special reproductive substance or *germ-plasm*, distinct from the rest of the protoplasm, which is *somatic*, serving to nourish the germ-plasm and maintain the growth of the individual. There is a continuity of this reproductive substance from one generation to another, each passing on to its offspring a certain amount of germ-plasm which, nourished by the somatoplasm, is increased in the individual but undergoes no change in kind, so long as it simply perpetuates the species.

Nägeli distinguished two parts in the protoplasm; an active organising portion termed *idioplasm* being separated from the rest which is merely nutritive. But while Nägeli's idioplasm is regarded as forming a continuous network throughout the organism formed by growth from the idioplasm of the spore, Weismann's germ-plasm is confined only to a certain series in the *soma* and a few special cells of the individual, which are accordingly distinguished as reproductive from all the rest which are merely somatic.

In the following observations on Dr. Weismann's theory,[1] I propose limiting my remarks to the consideration of three points only, as follows:—

1. That there is a continuity of germ-plasm, from the germ-cells of the parent to those of the offspring (p. 173) in a certain series of somatic cells only (pp. 197, 207).

2. That the characters acquired by the individual through "use, disuse, practice, neglect, nutrition, light, moisture, and climate," (p. 387) cannot produce any effect in the transformation of species (p. 388) and cannot be transmitted at all (p. 267).

3. That the transformation of species is due (i.) to direct changes in the germ-plasm effected by external influences (pp. 327, 388, 396,

[1] *Biological Memoirs*, Authorised translation, Oxford. The pages given in the text refer to the First Edition, 1889.

411); or (ii.) by combinations of distinct germ-plasms by sexuality (pp. 293, 321, 327).[2]

1. The Continuity of Germ-plasm.

The following quotations will express Dr. Weismann's views on this point in his own words:—"A continuity of the germ-*cells* does not now take place, except in very rare instances; but this fact does not prevent us from adopting a theory of the continuity of germ-*plasm*" (p. 173). "That continuity is maintained, by assuming that a small part of the germ-plasm persists unchanged during the division of the segmentation-nucleus, and remains mixed with the idioplasm of a certain series of cells, and that the formation of true germ-cells is brought about at a certain point in the series by the appearance of cells in which the germ-plasm becomes predominant" (p. 197). This "point" is, on the one hand, the nucleus or egg-cell of the embryo-sac, and, on the other, the sperm-cell or fertilising nucleus of the pollen-grain (p. 402); for "the nuclear substance must be the sole bearer of hereditary tendencies" (p. 179).

With regard to the mode of development of hereditary characters, whether in the vegetative or reproductive organs of plants, he quotes Strasburger, who says, "the nuclei determine the specific direction in which an organism develops" (p. 185). "The nucleoplasm will enter upon definite and varied changes which must cause the differences appearing in the cells which are produced" (p. 185). "The complexity of the molecular structure [of nucleoplasm] decreases as the potentiality for further development also decreases" (p. 191); and therefore, "it has lost the power of originating any new kind of cell" (p. 195). Therefore, "A sudden transformation of the nucleoplasm of a somatic cell into that of a germ-cell would be almost as incredible as the transformation of a mammal into an amœba" (p. 200). "There is no nucleoplasm like that of the germ-cell in any of the somatic cells" (p. 195), excepting presumably the germ-plasm contained in "the certain series of cells" (p. 197) by which its continuity is maintained.

The above deductions were drawn from Dr. Weismann's study of the Hydroids, on which he thus speaks:—"As a result of my investigations on Hydroids, I concluded that the germ-plasm is present in a very finely divided, and therefore invisible state in certain somatic cells, from the very beginning of embryonic development, and that it is transmitted through innumerable cell-generations to those remote individuals of the colony in which sexual products are

[2] Dr. Weismann has also said elsewhere:—"A species is only caused to change through the influence of changing external conditions of life, the change being in a fixed direction. . . . Transmutation by purely internal causes is not to be entertained. . . . The action of external inciting causes in the widest sense of the word is alone able to produce modifications." *Nature*, vol. 22, p. 141.

formed. This conclusion is based upon the fact that germ-cells only occur in certain localised areas" (p. 207).

Dr. Weismann now endeavours to apply his hypothesis of germ-plasm being limited to a certain series of somatic cells to plants. Since, however, Begonias can be propagated by their leaves, he admits:—"I think that this fact only proves that in *Begonia* and similar plants, all the cells of the leaves, or, perhaps, only certain cells, contain a small amount of germ-plasm, and that consequently these plants are specially adapted for propagation by leaves" (p. 211). He then proceeds to base an argument upon this, as follows:—"How is it then that all plants cannot be reproduced in this way? No one has ever grown a tree from the leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus." The author here infers that because no one has raised tulips from their leaves, therefore it cannot be done! The fact, of course, really is that it has never been worth while to do it. If, however, a premium were offered for tulips or oak trees raised from leaf-cuttings, plenty would soon be forthcoming. It is known to all horticulturists and botanists that there is virtually no limit to the propagation by leaves or any other part of a plant.[3] The sole conditions are the presence of "embryonic" cells, and a sufficient amount of nutritive material at the section to enable the part to develop roots.

Reading in Sachs' lectures on the Physiology of Plants,[4] that in the true mosses almost any cell of the roots, leaves, and shoot-axes, and even of the immature sporogonium, may grow out under favourable conditions, become rooted, form new shoots, and give rise to an independent living plant, Dr. Weismann makes the further admission: "We have here a case which requires the assumption that all or nearly all cells must contain germ-plasm" (p. 212). He does not seem aware that the propagation by roots, stems, leaves is habitually practised in *all* kinds of plants whenever desirable, that the power of hereditary reproduction by the vegetative system is no special peculiarity of Begonias and Mosses, but a universal feature of the vegetable kingdom. Even fruits such as cucumbers, prickly pears, and strawberries have been known to send out roots and buds.

This fact is, of course, perfectly well-known to all botanists and horticulturists, and needs no further illustrations. Dr. Weismann's mistake lies, therefore, in supposing the phenomenon to be exceptional instead of general.

Moreover, besides the production of flowers, fruits, and seeds from the vegetative system, it must be remembered that roots, usually confined to subterranean regions, can be produced anywhere in the

[3] For example, though it is usual to propagate Chrysanthemums by stem-cuttings, a florist exhibited at one of the exhibitions of the Royal Horticultural Society a large quantity raised from leaves. The Dutch propagate Hyacinths solely rom sections of leaf-scales, &c.

[4] P. 723, English Edition.

aërial parts of plants. Similarly, the tubers of the potato, a specific character, as the name *Solanum tuberosum* implies, can be made to appear on the upper part of the plant, by not allowing them to grow underground. Lilies will sometimes develop bulbils on their stems abnormally, just as *Lilium bulbiferum* normally produces them. All such, and many other cases that might be mentioned, appear to indicate that if germ-plasm be requisite for the development of hereditary characters, it must be present practically everywhere, and in readiness to produce them when called upon to respond to external influences which give rise to them.

Hence we arrive at the necessity of extending Dr. Weismann's hypothesis and, adopting his own words, to say: "That *all* somatic nuclei of plants may contain a small amount of unchanged germ-plasm" (p. 211).

2. Acquired Characters are not Hereditary.

Dr. Weismann defines them as being "those characters which have been acquired by the influence of special external conditions during the life-time of the parent; such cannot be transmitted at all" (p. 267). "They cannot produce any effect in the transformation of the species, simply because they can never reach the germ-cells from which the succeeding generation arises" (p. 388). Acquired characters he considers to be such as are attributable to "use and disuse, practice or neglect, nutrition, light, moisture, and climate" (pp. 387, 8). On the other hand, he asserts that "changes in the germ-plasm are the first results of the new conditions" (p. 414).

Thus, in the case of wild carrots cultivated by Hoffman, in which changes produced by cultivation in the structure of the root were hereditary, Dr. Weismann observes:—"It is sufficiently obvious that in this case we are also concerned with a change which did not become visible until after some generations had elapsed, and which was therefore a change in the germ-plasm" (p. 414).

It would seem not unreasonable to demur to his supposed necessity of the effect being visible at first, for it is quite conceivable, and indeed probable, that time may be required in most cases for the influences of the environment to accumulate in order to produce any marked effects. Moreover, although no changes may be externally visible for two or more generations, yet the effects may be recorded in some alterations in the tissues only observable by the microscope, and are potentially accumulating until at last they become very manifest to the naked eye. Indeed, no one, probably, has as yet actually examined the roots microscopically with the express purpose of testing this point, for the attention of botanists has not been brought to it before.

In the case, however, of certain experiments conducted by the late Professor J. Buckman with parsnips and carrots, the change *was*

actually produced and visible in the first season. He thus describes his results:—"In the spring of 1848 I planted two of my experimental plots[5] with seeds of *Pastinaca sativa* and *Daucus carota*. The seeds were gathered in the previous summer from wild plants. In the autumn, whilst most of the specimens presented the dark green hairy leaves peculiar to the wild plant, there were a few examples in which the foliage had assumed the light green and smooth aspect devoid of hairs which characterises the cultivated plant, and among the latter there were a few with larger leaves and broader divisions, or leaf-lobes, than the rest, the leaves, too, all growing symmetrically around one central bud. The roots of the plants taken up were observed to be, for the most part, more fleshy than those of the wild examples."

In the spring of 1849, the reserved roots were dug up, and the best put aside for transplanting. Late in the summer of 1849, the seed was collected, and sown in 1850.

The result of the second sowing was that the leaves in all indicated an advance to the cultivated form. Some specimens were much finer than others, of good size, and with a tolerably smooth outline.[6]

"In the case of carrots of the first crop all the specimens manifested a disposition to send up several heads, and most of them ran to seed. The roots were much forked, and but little inclined to increase the tissue. The second crop was certainly improved, the crowns of many displayed but one bud. The roots became more succulent and enlarged."[7]

This experiment seems to justify my remarks, for it is evident that *all* the plants, both of parsnips and carrots, were not equally affected by the new environment, and while in some the changes were "visible" at once, in others, especially the carrots, it required a generation to evince them satisfactorily. The probability is—judging from the pronounced way in which *some* were modified—that all were more or less influenced, and had Professor Buckman examined the tissues of the roots he might possibly have detected the commencement of hypertrophy in the first season in every one.

As another instance of experimental evidence, I will refer to that of M. Lesage upon maritime plants. It is usual to attribute the fleshy character of the leaves of several maritime plants to their growth by the seaside, where salt can reach them; and that salt is

[5] In the garden of the Agricultural College, Cirencester, at which he was the Professor of Natural History.

[6] Professor Buckman called one "The Student," a hollow-crowned form, and forwarded seeds to Messrs. Sutton & Sons to grow; they approved of it, and issued it as "Improved," in 1851. On writing to them in 1890, they informed me that they still consider it the "*best variety*" in their stock. Hence it has been now established in the trade for 40 years. In 1891 they issued a new illustration of "The Student," which shows marked differences from that drawn in 1861.

[7] *Journ. Agricult. Soc., England*, 1864.

the *cause* of the fleshiness, is believed, from a large number of coincidences, coupled with the fact that the same plants are not always fleshy when growing inland. M. Lesage examined the tissues of a large number of plants growing both near and away from the sea; and he came to the following conclusion:—"Il résulte clairement du rapprochement de ces faits que le voisinage du bord de la mer en même temps qu'il détermine l'épaississement de la feuille, y provoque le développement exagéré du tissu palissadique."[8]

Then, following up the investigation experimentally, he succeeded in producing an identically similar fleshiness by watering with salt water, thereby verifying the deduction that it was due to the presence of salt from the sea. He adds:—"J'ai réussi sourtout pour le *Lepidium sativum* cultivé en 1888. J'ai obtenu pour le même plante, des résultats plus nets encore dans la culture de 1889, entreprise en semant les graines récoltées avec soin dans les pots de l'année précédente et traitées exactement de le même façon."[9]

This experiment, like Professor Buckman's, shows that the alteration of the tissues was carried on in the second generation from the point gained in the first. In both cases no germ-plasm (in the germ-cells) existed at the time during which the alterations arose, as they were confined to the vegetative system; and in the case of the parsnips and carrots, being biennials, no germ-cells are produced till the second year has arrived.

As a third example, Professor Bailey observes:—"Squashes often show remarkable differences when grown upon different soils, and these differences can sometimes be perpetuated for a time by seeds. The writer has produced from the same parent squashes so dissimilar, through the simple agency of a change of soil, in one season, that they might readily be taken for distinct varieties. Peas are known to vary in the same manner. The ends of a row of peas sown of the same kind, last year gave the writer marked variations, due to differences in soil. . . . Pea-growers characterise soils as 'good' and 'viney.' Upon the latter sort the plants run to vine at the expense of the fruit, and their offspring for two or three generations have the same tendency."[10]

Once more. It has often been observed that spinescent features are characteristic of arid regions. M. Lothelier[11] found that the Barberry produced only spinescent leaves in a dry air, and none at all in a moist one. I find by experiment that *Ononis arvensis* loses its spines in a rich, moist soil, whether grown from cuttings or raised from seed.

As a further illustration in which climatic influences appear to

[8] *Rev. Gén. Bot.*, vol. ii., p. 64. [9] *Loc. cit.*, p. 114.

[10] Extracts from an article in the "American Garden," by Professor Bailey; quoted in the *Gardeners' Chronicle*, May 31, 1890, p. 677.

[11] *Rev. Gén. Bot.*, 1890, p. 276.

be the direct cause, is the fact that the species of cultivated plants often, if not generally, "sport" simultaneously in various districts, and even in different countries. Thus, I am assured that double Petunias were never raised until they simultaneously appeared in one and the same season in France, Germany, and England. Similarly, Chrysanthemums are known to furnish sports of a like kind in different places. Now, although double flowers, as of Petunias, are mostly propagated from cuttings, yet, speaking generally, they can be perpetuated by seed and intensified by self-fertilisation. It was thus that Mr. Veitch raised his "Balsamifloral" section of East Indian Rhododendrons, and others raised double Begonias.

Again, there is a class of phenomena which are designated as "habits," as of insectivorous, parasitic, saprophytic, and climbing plants, &c. These are scattered over the vegetable kingdom, often without any affinities, respectively; and when one imagines the *first individual* of any species which acquired any one of these habits, becoming, *e.g.*, parasitic, such a habit must have been a character acquired during the lifetime of the individual. Yet, in all cases these habits, and the anatomical structures correlated to them, are now hereditary.

As other instances in which peculiar structures are now hereditary may be mentioned aquatic plants, and those possessing subterranean stems. Whether they be dicotyledons or monocotyledons, there is a fundamental agreement in the anatomy of the roots and stems of aquatic plants, and, indeed, in many cases, in the leaves as well. Such has hitherto been attributed to the aquatic habit. The inference or deduction was, of course, based upon innumerable coincidences; the water being supposed to be the direct cause of the degenerate structures, which are hereditary and characteristic of such plants in the wild state.

M. Costantin has, however, verified this deduction by making terrestrial and aërial stems to grow underground and in water. One could not say, *à priori*, what alteration would take place, if any at all; but the new structures at once begin to assume the subterranean or aquatic types, as the case may be; and, conversely, aquatic plants made to grow on land assume the terrestrial type of structure; analogous results following changes from a subterranean to an aërial position, and *vice versâ*. This verification, therefore, proves not only that the influence of the environment is at once felt by the organ, but that it was indubitably *the cause* of the now specific and hereditary traits peculiar to normally aquatic, subterranean, and aërial stems or roots.

The difference in structure between the upper and under side of a horizontal leaf is known to be interchangeable, according to the incidence of light; while erect leaves, equally exposed, are similarly constructed on both sides. Here, as in all organs, we discover by

experiment how easily the anatomy of plants can be affected by their environment; and as long as the latter is constant, so are the characters of the plant constant and hereditary.

Dr. Weismann, however, would attribute such susceptibilities to an assumed "predisposition," instead of regarding it as a universal property of protoplasm to respond to external influences. One cannot disprove the existence of such a predisposition, but it seems unnecessary to assume it, when probabilities are accumulative on the other side, which render it entirely unnecessary.

As an instance of a natural change of structure and habit, I have often found *Ranunculus heterophyllus* growing on mud after the water has dried up. As in the case of the normally submerged plant, finely dissected foliage had still grown, but altered in character. The segments had become rigid, broader, and with stomata. The leaves were all erect, forming a sort of green "pile," and perfectly adapted to aërial conditions. Such may, perhaps, be compared to the Mud-fish, which can breathe in both water and air. In another case the plants became prostrate, rooted at the joints, formed no more dissected leaves, and were in all respects similar to a form of *R. hederaceus*.

Similar alterations occur in terrestrial plants raised by seed sown in pots, collected from the aquatic form. The cotyledons at once appear of a different character. Then, the first leaves are dissected, but "aërial," the subsequent ones being of the "floating" type; as if the plant had reached the surface of some imaginary water! This illustrates, therefore, very well what is a universal feature, viz., the combination of hereditary characters in conjunction with direct adaptations to a new environment. If the above plants be transferred to water, the "aërial" dissected leaves die, and "aquatic" dissected leaves are immediately developed.

As Dr. Weismann includes "climate" as one of the forces acting on the individual which may produce "acquired characters," I may add the effects produced by a change from an Alpine to a low-level habitat, as well as from a high to a low latitude. It is well-known that there are not only plainly visible differences, such as a richer foliage and brighter coloured flowers, in high latitudes and Alpine regions, but the anatomical details of structure of the leaves are very different.

As in the case of experimental evidence of the effects of water, so in the case of these conditions, M. Bonnier[12] has proved by cultivation that when plants are changed from one locality to another, being grown from seed, the vegetable system soon begins to respond to the new climatic conditions, and assumes the histological characters of the plants normally growing in the same environment.

[12] *Bull. Soc. Bot. France*, 1886, p. 467. See also *Ann. des Sci. Nat. Bot.*, 1879, and *Rev. Gén. Bot.*, vol. ii., 1891, p. 513

The evidence of the direct influence of the environment as causing changes of structure is thus accumulative. Indeed, what is surprising is not so much that changes are produced, as the constancy of species in Nature; for any pronounced alteration of habitat seems almost always to set up alterations of structure. Such is the experience of horticulturists, for when, for example, bulbous plants, as tulips, &c., are first introduced from the East into English gardens, they soon begin to change in all parts of their structure, so that in a few years they are hardly recognisable.[13]

3. Transformation due to direct Changes in the Germ-plasm.

In maintaining the proposition that all hereditary structures have arisen through changes first wrought in the germ-cells alone, and must have been produced through the influences of external agencies working directly upon the germ-plasm, Dr. Weismann seems to have raised at least two very formidable and unnecessary difficulties so far as plants are concerned. Firstly, the growth and development of all vegetative structures takes place, and is mostly completed, *before* any germ-cells are produced, the flowering process (which alone contains them) invariably following the close of the period of vegetative growth. Secondly, while the duration of the vegetative period is relatively long, that of the existence of the germ-cells is excessively short. Thus, no flowers last for more than a few hours only. On the other hand, an annual requires at least several weeks for its development; perennial herbs and trees demand months and years; an aloe may be fifty years old before it blossoms, while ferns and horsetails never have any germ-cells at all during the whole of the agamic generation.

If, however, we suppose with Strasburger that germ-plasm be everywhere present and capable of being influenced by the environment, then these difficulties are at once removed, and the probability is much increased that variations have come about through the responsiveness of germ-plasm (assumed to accompany the protoplasm) coupled with the power of fixing the effects and of transmitting them to the germ-cells on the appearance of the latter at the period of flowering.

Dr. Weismann's objection that "acquired characters cannot be transmitted at all" (p. 267), because "they can never reach the germ-cells" (p. 388), is thus met. For if germ-plasm be always "unchanged," and capable of receiving and registering impressions from without, wherever it may be, and not solely within the germ-cells, as he seems to maintain, we thus have at least a sufficient material.

Here, again, we have admissions from him that this is con-

[13] Such, *e.g.*, is the experience of Mr. Elwes. *See also* "Sur la production et la fixation des Variétés dans les plantes d'ornement," by B. Verlot, 1865.

ceivable, for he says:—"It must be admitted that there are cases, such as the climatic varieties of certain butterflies, which raise some difficulties against this explanation. I myself experimentally investigated one such case, and even now I cannot explain the facts otherwise than by supposing the passive acquisition of characters produced by the direct influence of climate" (p. 99).

The next and last point is the *machinery* required for transmitting the effects of the environment, presumably produced at the periphery of the organism, to the germ-cells. When, however, we remember that protoplasmic continuity is probably maintained through all nucleated cells in conjunction, that the threads unite the nuclei into a community, and that the cell-walls themselves are, so to say, saturated with living protoplasm, too; and if germ-plasm accompany this universally distributed protoplasm, we seem to have the very machinery required for the purpose. Thus can we hypothetically account for the fact that flowers with germ-cells can be produced from any part of the vegetative system, and that any specific characters, such as roots, bulbs, and tubers, may be produced abnormally on the aërial parts of plants; and, lastly, that the power of reproducing any acquired characters may be transmitted to the germ-cells, and consequently such may be then hereditary.

If the theory, as represented in Dr. Weismann's work, is thus shown to be insufficient to account for hereditary traits of plants, as seen in the vegetative system, there remain the reproductive organs which constitute the flower.

As these, and these alone, contain germ-cells, it would seem that they are the only organs which one would suppose, *à priori*, capable of being influenced by the direct action of the environment. The application of the theory to *The Origin of Floral Structures through Insect and other Agencies*, is, therefore, apposite.[14]

Dr. Weismann observes with regard to the localisation of germ-cells:—"The whole individual is composed of the millions of somatic cells by which the reproductive cell is surrounded" (p. 75), and again, "the number of somatic cells becomes so large that they surround the reproductive cells with many layers" (p. 77).

This description does not apply to plants. The egg-cell of the embryo-sac, and the sperm-cell of the pollen-grain are close to the surface. Thus light can penetrate to the ovule and turn it green, and one layer of cells only separates the pollen from the atmosphere.

In my work just quoted I endeavoured to show that the infinite variety of adaptations to insects discoverable in flowers may have resulted through the direct action of the insects themselves, coupled with the responsive power of protoplasm. I cannot verify the deduction experimentally, for it is impossible to make a flower,

[14] This is the contention of my work with the above title. *Internat. Sci. Series*, vol. lxiv.

except indirectly;[15] but I "proved" it by appealing to the argument of the accumulation of probabilities, based on innumerable coincidences and correlations.

Now, however, Dr. Weismann has provided me with an unlooked-for support, supposing such to be necessary, because he insists upon the action of the environment being directly on the germ-plasm of the germ-cells. He thus speaks:—"There only remains one principle by which we can explain the transformation of species—the direct alteration of the germ-plasm" (p. 388), or "the direct change in the germ-plasm affected by external influences, using these words in their widest signification" (p. 411), and "we must include everything which is not germ-plasm" (p. 396).

In flowers visited by insects, the ovules and pollen-grains are both prepared and ready at the very time the insect arrives. Its irritating action is directly upon the anthers and stigma, which is in immediate communication with the ovule, embryo-sac, and egg-nucleus; so that it is impossible to conceive of a closer contact than that which thus obtains between the insect and the germ- and sperm-cells.

If, therefore, it be necessary, in order that the peculiarities of flowers should be hereditary, that the influence of the irritations be directly upon the germ-cells, then the conditions for securing such are exactly those which every flower that is in adaptation to insect fertilisation supplies.

With regard to the origin of new specific characters by sexuality, Dr. Weismann observes that "permanent hereditary variability may follow from the varied combinations which are due to the mixture of two individually distinct germ-plasms at each act of fertilisation" (p. 327). And again: "Variability depends upon sexual reproduction" (p. 293).[16]

When, however, we perceive the constancy of specific characters of plants growing wild, Nature does not seem to supply any encouragement to the acceptance of this idea. Not only are the species of plants named by Linnæus just as they were in his day, but Dr. Weismann calls the reader's attention to the fact that species of plants in Egypt have remained unchanged for thousands of years; while he refers also to arctic plants, now widely dispersed, which are probably very much the same as they were at the Glacial Epoch.

On the other hand, had he alluded to varieties and hybrids, there would have been more to be said in his favour, as the successive generations of such, under cultivation at least, vary immensely; but their variations mainly refer to size and colour, and only to a limited degree in form, *e.g.*, "Pelorian" varieties.

[15] See *op. cit.*, chap. xiv., on "Acquired regularity and Peloria," p. 128.

[16] A theoretical description is given on p. 321.

The great variability under cultivation Dr. Weismann refers to "Panmixia," or the cessation of Natural Selection, with the consequence of indiscriminate crossing. He thus describes it:—"As soon as natural selection ceases to operate upon any character, structural or functional, it begins to disappear" (*Note*, p. 140). If so, and if Natural Selection is withdrawn under cultivation, thus ceasing to keep up any (and that is equivalent to saying *all* the) characters of plants to the required standard, then *all* the organs of a plant ought to degenerate and atrophy. But hypertrophy and not atrophy is the most obvious characteristic of all cultivated plants.

Again, he says:—"As soon as an organ becomes useless, the continued selection of individuals in which it is best developed must cease, and *panmixia* takes place" (p. 291).

It is not easy to find cases of degeneracy in cultivated plants, but the two following may suffice. In *Bellevallia comosa* the terminal flowers of the raceme are already abortive in the wild state. Under cultivation they *all* deteriorate and become rudimentary, or vanish altogether, but when this stage is arrived at, the flower stalks, now absolutely useless, become subject to hypertrophy, so that a large feathery process of violet-coloured pedicels is obtained. Similarly in the Guelder-rose, as soon as the central and normally perfect flowers have lost their sexual organs under cultivation, the corollas, now useless to attract insects for fertilising the pistils, become increased in size, until a great globe of barren flowers is the result. According to Dr. Weismann, both the flower-stalks of *Bellevallia* and the corollas of *Viburnum opulus* should have atrophied and disappeared. Similarly we ought not to have any "double" flowers at all, and garden roses should be a thing unknown.[17]

Lastly, by panmixia there "follows a mixture of all possible degrees of perfection, which must in the course of time result in the deterioration of the average development of the organ" (p. 292).

In making this somewhat categorical statement, Dr. Weismann does not appear to have allowed for the prepotency of the healthy parent. Thus, a plant which begins to show deterioration by one or more of the stamens becoming petaloid, if it be crossed by a healthy "single" flower, will produce offspring with single flowers only.

[17] Mr. Romanes, on reading my MS., has appended the following interesting observation:—"I agree that Weismann is wrong in saying that cessation of selection must always, and in the first instance, produce *atrophy*. What it must always produce is *degeneration*. But the degeneration may, first of all, have reference to the breaking down of *organisation* (mutual co-ordination of parts, &c.) which had been previously formed and maintained by selection. Now, the breaking down of organisation need not entail atrophy, and is even compatible with the hypertrophy of some of the previously co-ordinated parts. The whole apparatus being thrown out of gear, nutriment before supplied to the vanishing structure may be supplied in excess to the others, and all kinds of mal-correlations of growth ensue. In a state of nature, however, the *reversal* of selection will prune away all such useless growths."

Similarly a weak or tender constitution may be strengthened by crossing with a more robust one, &c., so that the effect is not to lower the standard of the whole of the offspring (as if the weaker one were prepotent), but the superior strength of the healthy parent raises them up to its own standard, and obliterates the downward tendency.

A few concluding words on the nature of proof. Dr. Weismann says:—"My theory rests, on the one hand, upon certain theoretical considerations. . . . On the other hand, it rests upon the want of any actual proof of the transmission of acquired characters. My theory might be disproved in two ways—either by actually proving that acquired characters are transmitted, or by showing that certain classes of phenomena admit of absolutely no explanation unless such characters can be transmitted" (p. 388).

The reader will judge whether I have not sufficiently fulfilled both these requirements of proof. He speaks of the belief in the transmission of acquired characters being "vaguely felt." It is not so with botanists.

It is based, first, upon a deduction of identically the same kind as of Geology and Astronomy, viz., the vast accumulation of coincidences and correlations, which preclude the converse probability. Indeed, Dr. Weismann recommends the use of this line of argument himself, for he observes that the doctrine of evolution is based upon it, so that "it may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation" (p. 255).

The deductions of geologists cannot be verified by experiment, because we cannot make Nature retrace her steps and show us how the Oolitic rocks were laid down, or how the Devonian fishes came into existence.

Similarly, astronomers cannot secure a portion of the sun for chemical analysis, but we believe we know something of its composition by spectrum analysis, which furnishes a great number of coincidences, but nothing more.

With plants, however, not only is the belief based on innumerable coincidences and correlations, infinitely more numerous than the spectrum can furnish in the case of the sun, but experimental evidence, whenever it has been tried, as with aquatic, subterranean, maritime, spinescent, and hypertrophic structures, &c., as I have shown, invariably corroborates, and thus verifies the deduction.

Lastly, we must never lose sight of the fact that Dr. Weismann does not pretend to offer anything more for our acceptance than an "assumption" (p. 197), or a "theoretical consideration" (p. 388), or an "imagination" (p. 174). He admits that no microscope has ever revealed the nature, or we may add, the existence of germ-plasm, or proved it to be as complicated as he assumes (pp. 168, 191, 271).

Nor can it be distinguished from idioplasm (pp. 180, 200). Nor, we may add, can either one or the other be practically differentiated, but only by the imagination, from what is known as protoplasm, which, however complicated its structure may prove to be, appears to be perfectly capable of acquiring new characters and fixing them by heredity.

GEORGE HENSLOW.

II.

The Poisonous Secretion of Batrachians.

THE author of this review would regret if, by drawing attention to the toxic properties of the Batrachian integument, he were to revive the former dread of a most interesting and useful group of animals—a feeling which, although still so prevalent, is gradually abating among the educated class. For it must be borne in mind that, although every species, even including the frogs, possesses a greater or lesser amount of poisonous properties for mere protective purposes, no Batrachian has the gift of spontaneously squirting out its poison. No more serious accident than a slight irritation of the mucous membranes can be caused by gently handling even the most formidably endowed members of that group of animals.

As remarked by Brunton (4), however, animal poisons are at present acquiring a new interest, from the relationship now found to exist between them and the poisons formed by disease germs, and we therefore think some of the readers of this journal will be pleased to find a *résumé* of the present state of our knowledge of Batrachian poisons as a sequel to the short communications which have recently appeared in the correspondence columns of the *Lancet* (4, 15). The tolerably complete bibliographical index appended to this article will enable those specially interested in the matter to refer to the sources from which it has been compiled.

The skin of the dorsal surface of all Batrachians is studded with glands and follicles. In various species, the Toads and Land Salamanders for instance, some of the larger glands appear as very prominent warts pierced with wide pores, especially a large mass situated on each side of the back of the head behind the eyes (see Fig. 1, next page), which has received the misnomer of parotid, a name now generally changed to parotoid gland. These glands secrete, more or less abundantly according to the species, a viscid or milky, more or less and variously odorous fluid, which is exuded when the animal is disturbed, by means of the contraction of small muscles disposed in the skin around the glands (Leydig, 18). If the larger glands be pressed or subjected to an electric current, the secretion is squirted out with great strength, and sometimes to a considerable distance. Kobert (16) recommends, as the best means of obtaining an abundant supply of toad-poison, the subcutaneous injection of a small dose of chloride

of barium; after a few minutes the animal is so covered with its secretion as to appear as if coated with whitewash. The object of the secretion is to guard Batrachians from the attacks of various enemies. A dog will not often repeat the experiment of seizing a toad; the signs of pain which it evinces after the poisonous fluid has come into contact with its mouth is familiar to most of those who have kept young dogs in the country. That the toad's poison does not protect it against snakes is well known, our common snake, for instance, not feeling any more repulsion for it than for a frog, although it will not eat such Batrachians as *Bombinator* and *Salamandra*. I myself have witnessed the discomfort of a hungry specimen after having seized a Land Salamander, which it very soon let go. But even the Salamander is not proof against one kind of snake at any rate, the common viper. It is also well known to all who have handled

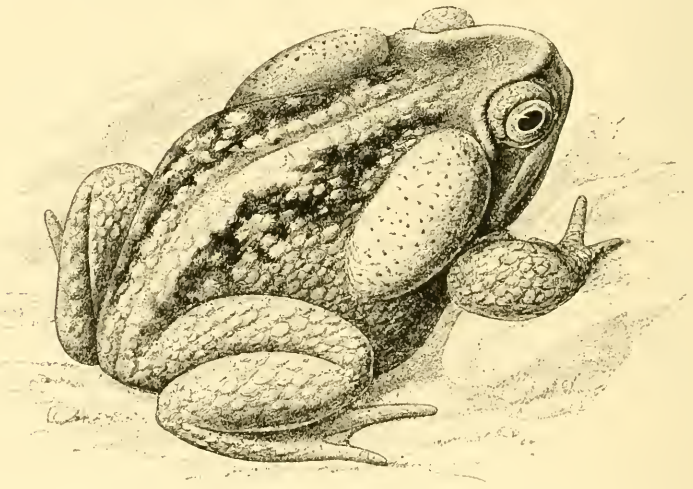

FIG. 1.—*Bufo marinus*, a South American Toad with enormously developed parotoid glands.

freshly-caught Newts, and certain toads, especially *Bombinator*, that the secretion of these Batrachians acts as a sternutatory, and causes irritation of the mucous membrane of the nose and conjunctiva; the effects produced on us by *Bombinator* being comparable to the early stages of a cold in the head. German violinists, it is said, when suffering from moist hands, are accustomed to check the perspiration by handling live toads. Many collectors of Batrachians have learned, to their discomfiture, how the introduction of examples of certain species in the bag containing the spoil of their excursion may cause the death of the other prisoners; for although the poison has no effect on the skin of individuals of the species which produces it, different species, however closely allied, may poison each other by mere contact. We shall see further on that, when ingurgitated or inoculated, the poison acts even on the individual by which it has been produced.

Miss Ormerod (19) thus relates a personal experiment with the exudation of the Crested Newt:—"For the sake of exactly ascertaining the sensations (which in the lower animals could only be judged of by their apparent effects), a part of the back and tail of a live *Triton* were gently pressed between the teeth sufficiently to alarm the animal and cause it to give out its acrid cutaneous exudation. The first effect was a bitter astringent feel in the mouth, with irritation of the upper part of the throat, numbing of the teeth more immediately holding the reptile, and in about a minute from the first touch of the newt a strong flow of clear saliva. This was accompanied by much foam and violent spasmodic action, approaching convulsions, but entirely confined to the mouth itself. The experiment was immediately followed by headache lasting for some hours, general discomfort of the system, and half an hour after by slight shivering fits."

Contrary to what Rainey (24) and Davy (7) were still inclined to believe as late as 1863, the secretion of Batrachians possesses more than an irritating quality; it is a very active poison when introduced into the circulation, as has been proved by repeated experiments from the time of Laurenti (17) down to the present day. It is principally with the researches of Gratiolet and Cloez (13, 14), Albini (1), and Vulpian (26, 27) that the study of the subject entered its truly scientific phase; and experiments have since been pursued by Zalesky (28), Fatio (9), Fornara (11, 12), Calmels (5), Dutartre (8), Phisalix (20, 21, 22), and others. All these experiments have shown the poison of toads, salamanders, and newts to be capable of killing mammals, birds, reptiles, and even fishes, provided, of course, the dose be proportionate to the size of the animal inoculated. It is unnecessary to give here particulars of the experiments, for which the reader is referred to the publications of the authors quoted above; it will be sufficient to say that small birds and lizards succumb, as a rule, in a few minutes, and guinea pigs, rabbits, and dogs in less than an hour.

The notion first entertained by Vulpian (26) that Batrachians are proof against their own poison, has been disproved by Claude Bernard, and subsequently by Vulpian himself (27), Capparelli (6), and Phisalix (20). Toads and salamanders inoculated with their own secretion perish; but this effect is only produced by using a stronger dose than is required for killing individuals of another species. In his elaborate contribution on the poison of the Crested Newt, Capparelli (6) states that he succeeded in killing specimens of that Newt by introducing the secretion in the buccal or abdominal cavities; he also shows that the poison produces very different effects, according to its being introduced into different parts of the organism.

The poison of Batrachians, unlike that of Snakes, is not a septic, but acts upon the heart and the central nervous system. That of the common toad has been compared, as regards its effects, to *Digitalis* and *Erythrophlæum*. For a long time authorities disagreed as

to the nature of the secretion of toads and salamanders. Gratiolet and Cloez at first (13) regarded it as an acid, but in a later paper (14) pronounced it to be an alkaloid, which view was also held by Zalesky (28). Quite recently, Phisalix (21) claims to have solved the question by reconciling both views: toads and salamanders are simultaneously possessed of two different kinds of glands, different both anatomically and physiologically. These are mucous or slime glands, spread over the greater part of the body, the exudation of which is controlled by the animal; and specific glands (the parotoids and the larger dorsal glands), the product of which can only be squirted out by foreign agency. The secretion of the former glands is an alkaloid, and acts as a narcotic; that of the latter is an acid, and acts as a convulsive. The anatomical differences of the two kinds of glands have been worked out by Paul Schultz (25), who, however, regards the product of the slime-glands as innocuous.

The frogs, so sensitive to the poison of other Batrachians, are, nevertheless, not free from toxic properties. This fact, first noticed

FIG. 2.—*Dendrobates tinctorius* (*Phyllobates chocoensis*, Posada-Arango).

by Vulpian (27), has been placed beyond doubt by the experiments of Paul Bert (3). A goldfinch inoculated with the secretion of the edible frog died in one minute, a frog of the same species in one hour and a quarter.

We still know very little with regard to the poisonous secretions of exotic Batrachians. Lacerda Filho (10) has experimented upon the common toad of Brazil, and Posada-Arango (23) informs us that the Indians of Colombia employ the secretion of a small frog, *Dendrobates tinctorius* (renamed *Phyllobates chocoensis* by that author), for poisoning their arrows. The exudation is obtained by exposing the frog to a fire, and after being scraped off the back is sufficient for poisoning fifty arrows. Posada-Arango's experiments on small mammals and birds have shown the poison, which he calls *Batracine*, to act on the central nervous system, like that of the Salamander, the *Samandarin* of Zalesky, or *Salamandrine* of French authors, not on the heart like toad-poison, for which he uses the name of *Bufine*. The name

Phrynine has since been proposed for the latter (Fornara, 12). Claude Bernard (2) had previously alluded to the fact that the Indians of South America use Batrachian poison, with the same effect as curare, for poisoning spears and arrows which are specially used against monkeys. This *Dendrobates tinctorius* (Fig. 2) derives its name from the property it is said to possess of "dyeing" parrots. The method employed for that purpose consists in removing part of the feathers of green parrots, and injecting the bare skin with the blood of the Batrachian; the feathers are regenerated green or yellow on these spots, and artificial varieties are said to be thus obtained.

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III.

The Evolution of Flat-Fishes.

IN starting to deal with any biological question it is well to plant our feet firmly on the solid ground of fact. In the present instance we can ascertain, without much risk of ambiguity, what are the structural peculiarities of the Flat-fishes. All we have to do is to compare them carefully with various symmetrical fishes. The most important and most obvious peculiarities are (1) the position of the eyes, both on the upper side of the body; (2) the absence of pigment from the lower side; (3) the anterior extension of the dorsal fin separating the blind side of the head from the eyed side. To these may be added a fourth, which is less important, being more marked in some genera or species than in others, and in some apparently absent, namely, the greater development of the jaws and teeth on the lower side. Mere dissection of adult specimens shows that the anomalous position of the eyes is due to a distortion of the facial region of the skull. The cranial region of the skull is but slightly altered, but the interorbital parts of the two frontal bones are bent away from their original position in the dorsal median line down to the side of the head, and they are also compressed into a thin plate. But the eyes have pretty nearly the same relations to the interorbital septum as in an ordinary fish. There is an eye on each side of the interorbital septum, as usual.

It is, in fact, the curious condition of the dorsal fin in the flat-fish, even more than the mere distortion of the eyes, which makes it so different from an ordinary fish. If the fin terminated some distance behind the eyes, or if it was prolonged in the direction which it ought to follow, that is along the line which divides the two frontal bones from one another, it would be plain at a glance which was the left side of the head and which the right. It would then be obvious that the left eye was still on the left side of the head, and the right eye on the right. But the dorsal fin does neither of these things. The ectethmoid bone belonging to the blind side is much enlarged, and sends back a process outside the eye belonging to that side to meet another process from the cranial region of the skull. Thus the eye which has migrated, the upper eye when the fish is held in a vertical plane, is enclosed in a complete bony orbit, while the lower eye is merely bounded on its outer side by the jaw-muscles.

It is on this bony bridge, entirely foreign to the anatomy of an ordinary fish, that the dorsal fin supports itself in its advance towards the snout. Properly speaking, the left side of the face in a plaice, for instance, extends from the ventral edge, or chin, to the line between the eyes; but the dorsal fin in its anterior extension divides this side of the face into two parts.

The colour of the skin in a flat-fish is the combined effect of three distinct kinds of tissue. On the inner side of the skin is a dense continuous opaque layer, with a white reflecting surface; this may be called the *argenteum*. Near the outer surface of the skin are two sets of microscopic elements—the *iridocytes*, which are fixed in form, and, like the argenteum, opaque and reflecting, and the *chromatophores*, which contain pigment, and are able to expand and contract their radiating processes. Both argenteum and iridocytes are present on the lower side as on the upper; it is only the chromatophores which are absent on the lower side.

The origin of the structural peculiarities in each individual Pleuronectid by a process of metamorphosis is also within the region of verifiable fact. In the majority of genera the lower eye moves apparently round the edge of the head, remaining always at the surface, and always functional. When the transformation takes place in this way the dorsal fin terminates behind the region of the eyes up to the moment when the lower eye has reached the upper side. Then it advances and, as it were, cuts off the retreat of the eye which has (apparently) migrated. In certain cases observed by Steenstrup and Alexander Agassiz the dorsal fin extends between the eyes, while there is still one on each side of the head.

The lower eye then seems to pass through the head to reach the upper side. Steenstrup, through insufficient knowledge of the anatomy of the adult flat-fish, concluded from this observed passage of an eye through the head that the lower eye passed under the frontal bone of its own side to reach its final position. But Agassiz, with truer insight, explained that the eye merely passed through the base of the dorsal fin, which in this particular instance (the genus *Plagusia*) extended forwards before the change in the position of the eyes, instead of after that change.

The disappearance of the chromatophores from the lower side of the flat-fish also takes place during the individual metamorphosis. The larva has chromatophores on both sides, as it has an eye on each side; but when the fish has taken to lying on one side on the bottom, the chromatophores on the lower side disappear.

These primary and essential facts of the peculiarities of the adult flat-fish and their development in the individual are at the present day well-known, and generally accepted. When we pass on to the deeper question of the explanation of these facts, we can likewise formulate certain broad propositions which no modern zoologist will dispute. But the statement that flat-fishes were evolved from

symmetrical ancestors, and that their individual metamorphosis is a recapitulation of their secular evolution, is now a mere truism. We have to consider the causes of the evolution and the causes of the metamorphosis.

There are doubtless some whose minds are satisfied with the purely orthodox Darwinian explanation. The theory is so simple and so universal, and it is not only so much easier to believe than to doubt, but it saves so much trouble to have one key to all cases of evolution, instead of having to find a special key for each by laborious research and experiment. What can be more obvious than that the peculiarities of the flat-fish are beautiful adaptations to its peculiar mode of life? It is certain that variations of the most diverse kinds occur; there are the variations of these very peculiarities in the flat-fishes to prove it. What other explanation is required than the survival of the most adapted individuals in each succeeding generation while the less adapted were continually slain in the never-ending battle of life?

Well, perhaps the weakest point in this argument lies in the fundamental assumption that all the peculiarities of the flat-fish are adaptations. Weismann says "alles ist angepasst." There must be no quibbling here as to the meaning of adaptation or *Anpassung*. The Darwinian theory depends for its force on the assumption that every character of an organism is or has been of some definite advantage to it in its endeavours to secure its own existence or to propagate its species. What then is the advantage to the flat-fish of the absence of chromatophores from the side on which it rests? No naturalist has ever ventured to suggest that it uses the conspicuous whiteness of its lower side in courtship or in obtaining food, and it is certain that when alarmed every flat-fish hides this lower surface completely, burying itself in the soft ground on which it lives. The Darwinian may reply that no positive advantage need in this case be assumed, that the chromatophores have a special function in concealing the fish by changing its tint, and that being useless on the lower side they have disappeared from absence of selection and economy of organisation. Why then have not the iridocytes and the argenteum also disappeared, since these have much to do with the coloration of the upper side?

This question of the absence of colour, that is, of chromatophores, from the lower side of flat-fishes has given rise to the most curious misconceptions. The observations of Alexander Agassiz, made only fourteen or fifteen years ago, on the metamorphosis of these fishes, constituted an epoch in the history of our knowledge of the subject, for they reconciled the apparently contradictory conclusions of Steenstrup and Malin concerning the migration of the lower eye. But in the same paper in which Agassiz described the transformation followed by him for the first time in living specimens, he has hopelessly confused the two entirely distinct questions of the changes

of colour in the upper side of flat-fishes and the absence of chromatophores from the lower side. He might conceivably have been able to prove that there was a connection between these two phenomena, but he has not attempted to do this. He refers to Pouchet's paper on the *Changements de Coloration sous l'Influence des Nerfs*[1] as indicating such a connection. He states that Pouchet studied chromatophores especially in relation with the "atrophism of the colour on the blind sides of Flounders, pointing most plainly to the partial atrophy of the great sympathetic nerve, effected during the passage of the eye from the right to the left, or *vice versâ*, as the cause."

It will, perhaps, seem incredible, though the truth of the statement can easily be verified, that Pouchet, in the paper to which I have referred, says not a word concerning the cause of the absence of colour from the lower side of the Pleuronectid. It is strange, indeed, that the distinguished son of the great author of the *Poissons fossiles*, whose native language was French, should have fallen into such an error in quoting a French memoir. Agassiz appears to have forgotten for the moment that the optic nerves in a fish cross one another at their origins from the brain, so that the eye of the right side is connected with the left half of the nervous system and *vice versâ*. Pouchet particularly describes two experiments upon turbots made expressly to ascertain whether one eye had more influence than the other on the chromatophores of the upper or left side. It is well known that Pouchet found the power in a fish of accommodating the general tint, *i.e.*, the lightness or darkness of its skin, to that of the ground on which it was placed, to depend upon the eyes. When both eyes were removed this power of accommodation was permanently destroyed; the chromatophores were unable afterwards to contract or expand. He found that the removal of one eye in *Gobius niger* did not affect this power of accommodation—the chromatic function—in the least. He removed the right or upper eye of one turbot, the left or lower eye of another, and found that in both cases the chromatic function was uninjured; the upper side continued to grow light or dark according as it was placed on light sand or dark mud.

Pouchet, however, further demonstrated by experiment that the connection between the eyes and the dermal chromatophores was effected exclusively by the great sympathetic nerve cord or chain of ganglia. If the sympathetic plexus in the inferior vertebral canal of a turbot was divided by cautery, the chromatophores of the skin behind the section were paralysed. Agassiz may have supposed that although both eyes were connected with each sympathetic, yet the migration of the *lower* eye in some way affected the sympathetic of the lower side, and that each sympathetic was connected with the chromatophores only of its own side. But Pouchet says nothing about all this, and Agassiz makes no mention of any evidence that the alteration

[1] *Archives de Physiologie et d'Anatomie*, 1876.

in the position of the eyes in the flat-fishes affects in any way the sympathetic nerve of the lower side.

Agassiz remarks in a foot-note that Pouchet has succeeded in producing a white side in trouts by destroying the eye *of that side*. As authority for this, he refers to an article in the *Revue Scientifique* for 1877. I have not seen this particular article of Pouchet's, but I have before me the latter's own abstract of his experiments on the trout as described in two papers published by the *Société de Biologie* in 1876 and 1878. He states in this abstract that by taking a trout from a river where the bottom was in some places light sand and in others dark weed, and destroying one eye, he was able to provoke a unilateral change of colour. The side *opposite to that from which the eye was removed* became permanently dark, the chromatophores being paralysed and expanded. He says that the crossing of the optic nerves being complete, one can conclude from this experiment that the great sympathetic of each side depends directly on the corresponding half of the brain. But this result was never obtained in flat-fishes. The difference between Pouchet's investigations and Agassiz's description of them could not possibly be greater. Pouchet does not even refer to the question of the origin or the atrophy of the chromatophores; Agassiz supports, as though it were Pouchet's, a theory that the stimulation of the chromatophores by reflex action through the eyes might not merely produce pigment, but pigment of colours similar to the colours of the light acting upon the eyes—that is, to the colours of the surroundings of the fish—and thus explain the origin of what he calls mimicry—in more modern language, protective resemblance. He puts aside the idea that the absence of chromatophores or of pigment from the lower side of flat-fishes is in any degree due to their withdrawal from the direct action of light. He states that he placed young flounders in a glass dish at a height over a table so that light could reach them from below, and that this arrangement neither prevented the transfer of the eye nor produced any effect in retaining the pigment spots of the blind side longer than in specimens struck by the light only normally from above.

I have made a series of careful experiments of this kind with young flounders taken at the beginning or in the middle of their metamorphosis, improving on the method followed by Agassiz, by placing a mirror below the aquarium at an angle of 45°, and cutting off the light from above by means of an opaque cover. In the *Zoologischer Anzeiger* of 1891 I published the result of the first of these experiments, namely, that in the great majority of the specimens treated in this way, after several months, although no effect was produced upon the eyes, more or less of the skin of the lower side was pigmented.

This experiment of mine has attracted the attention of Professor Alfred Giard, who criticises it in a recent note published in the

Proceedings of the *Société de Biologie*. Giard has considered the evolution of the Pleuronectidæ before, although he has not made a special study of their anatomy or development. Years ago he suggested a new theory in explanation of their origin, to which he refers in this note. His familiarity with the problem, as well as his studies in the doctrine of evolution generally, caused me to expect to find his remarks both sound and suggestive; but I must confess to disappointment. The immediate occasion of this note was the capture of what is commonly known as a double turbot. The right eye in this specimen was on the edge of the head, so that the dorsal fin, instead of extending as it usually does to the anterior part of the head, was separated therefrom by a concavity. The right side was pigmented all over like the left, but the colour was a little less intense. The specimen was 19 cm. long.

In almost every ichthyological work of any importance the occurrence of double flat-fishes having the characters just mentioned is described. Yet Giard proceeds to remark that the relative frequency in *Rhombus maximus* of this monstrosity, which has not been stated to occur in other Pleuronectids, is due without doubt to the fact that, as Day and McIntosh have pointed out, the young turbot swims in a vertical position for a longer time than the other flat-fishes. It is perfectly well known that double specimens occur in many species of flat-fish. They are as common in the flounder as in any species, and a specimen of this species is described and figured by Traquair in his memoir published by the Linnean Society in 1865. I have in my possession adult specimens both of the flounder and the plaice which exhibit this monstrosity. Day himself, to whom Giard refers, states in his "British Fishes" that double specimens have been observed in the flounder, turbot, plaice, soles, &c. Ritzema Bos records a case in the flounder in the *Biologisches Centralblatt* of July, 1886. There is, therefore, no evidence whatever that the monstrosity in question is more frequent in the turbot than in other species.

After describing his abnormal specimen, Giard remarks: "L'animal *devait* nager en se tenant verticalement et se poser rarement sur le côte droit." *Il devait nager*, the fish must have swum in a vertical position, and must have rested rarely on its right side. The question is, did it swim vertically and lie on its side rarely? Day states that these abnormal double specimens are seen to swim vertically, and to be more frequently found near the surface of the water than those which progress in the usual manner. He gives no actual observations in support of this statement. I have for some time past been endeavouring to obtain evidence on this particular point. Apart from observations on living adult double specimens, I have good ground for believing that they do not swim vertically, for I have observed living young flat-fishes many times during the period of their metamorphosis, and I know that they never swim vertically after the moment when the eyes have ceased to be perfectly

symmetrical. When the lower eye has begun to move towards the edge of the head, the body is held in a slanting position, and throughout the metamorphosis the line joining the two eyes is always kept horizontal, so that when the lower eye is on the edge of the head the body is at an angle of about 45° or more with the vertical. This is particularly well seen in the young turbot, which possesses a swimming bladder, and never seeks the bottom at all until the metamorphosis is almost complete. Giard quotes McIntosh as witness that a young turbot, 9 millimetres long, with its eye on the edge of the head, swam in the vertical position. This is simply an error of observation.

We have, however, no satisfactory evidence at present that the monstrous specimens, whose metamorphosis is permanently arrested, swim about in whatever position any more frequently or rest on the bottom less constantly than their normal brethren. I have now under observation a living double specimen of the plaice; its eye is on the edge of the head, the dorsal fin terminates behind the eye, and the posterior three-fourths of the lower side are coloured like the upper, the anterior fourth being white. This specimen, instead of showing a tendency to continue swimming in the water, cannot even be induced to leave the bottom long enough to enable me to see whether it holds itself perfectly horizontal or not. I have never seen it leave the bottom of its own accord; it lies always buried in the sand up to its eyes, and, when disturbed, makes violent struggles to bury itself again. I have had this specimen for more than six months.

In spite of all this, Giard not only assumes that double specimens swim vertically, but actually maintains that the existence and assumed habits of these double monstrosities is better evidence of the dependence of pigmentation upon light than the result of my experiment. "Dans ce cas comme dans beaucoup d'autres, la nature réalise pour celui qui sait la comprendre, une expérience bien plus parfaite que celles que nous pouvons imaginer dans nos laboratoires." For Giard goes on to remark that it was only at the price of a thousand difficulties that I succeeded in showing, in an incomplete fashion, that pigment appeared on the lower side of the flounder when light was directed upon it, and that the experiment is not absolutely conclusive. He detects a fallacy in my evidence, and this part of his short paper is still more remarkable than the rest. He points out that, of all the flat-fishes, it is in the flounder that reversed specimens are most common, that is, specimens in which the white side is the right, and the eyes are on the left, instead of *vice versâ*, as in normal specimens. He argues that the specimens upon which I experimented may have numbered reversed individuals among their ancestors, and the pigmentation adduced in evidence by me may have been due to inheritance from one of these ancestors which was pigmented on the left side. The probability is the greater in that double individuals are not rare

among flounders taken from the sea.[2] The pigmentation in my experiment may even, he suggests, be sufficiently explained as a souvenir of the original bilaterally symmetrical condition. He magnanimously mentions, however, that the individuals kept by me under the ordinary conditions remained white on the blind side, which is a fact that I stated, and he also adds that reversed specimens must be rare at Plymouth, since I have not even anticipated the objection, which is an erroneous inference. At Plymouth reversed flounders are exceedingly common, almost as abundant as normal specimens. It is true the objection did not occur to me, for it is obvious that if inheritance in the flounder acted as Giard supposes, there would be none but double flounders, since right-sided and left-sided individuals are always breeding with one another. It is remarkable that Giard does not perceive that his objection is more applicable to his own explanation of the ambicoloured turbot.

In conclusion, Giard reiterates his belief in his own theory, that the asymmetry of the Pleuronectidæ is due to the inequality of the eyes in their pelagic larvæ. This theory was announced in 1877, *à propos* of Agassiz's researches. The kernel of the theory appears to be contained in this sentence:—"From the moment when one eye is stronger than the other there must be a rotation towards the more feeble side, and a displacement of the (stronger) eye which can see through the body on account of its transparency and slight thickness: little by little selection reduces the amount of material between the eye of the blind side and exterior objects," and so on. I confess I do not understand Nature sufficiently to be able to appreciate this theory. Transparent pelagic animals have sense organs, especially eyes, according to Giard, of unequal size, but he does not say why. As a matter of fact, the eyes in young flat-fishes are not unequal in size—at least I have looked for any such inequality in vain. It is true that Owen states that the optic nerve of one side is thicker than the other in the halibut; but I have recently dissected the head of a halibut, and found no difference in thickness between the two optic nerves. It seems to me that even if one eye were larger than the other, this is no reason why it should move out of its place; and I imagine the mere transparency of the body could not produce inequality in the size of the eyes. If I understand Giard aright, the flat-fish lies on its side because its eyes are asymmetrical, whereas it is obvious to other naturalists that the eyes are asymmetrical because it lies on its side.

It is not my purpose on the present occasion to set forth at length the views I agree with or hold on the subject I have dealt with. I have intentionally devoted this opportunity to destructive criticism. I have considered some examples of the loose and

[2] Giard evidently supposes that the abnormality of the eyes does not occur in double flounders, since on the preceding page he states that it is confined to the turbot.

irresponsible fashion in which the deeper problems of Biology are too often discussed. There is too much striving after brilliancy and originality, and far too much importance attached to these qualities. In Biology, at present, it would sometimes seem that the Italian epigram, *se non è vero, è ben trovato*, is the verdict which authors desire for their papers. When the arguments of eminent biologists exhibit such an absence of logic and accuracy, such an abundance of self-contradictory propositions and baseless assumptions as some that have been published concerning the evolution of flat-fishes, it is small wonder that the claim of Biology to rank as an exact science should be questioned.

The physiology of the organism, like the physiology of organs, must be investigated by experiment. The final object of the exact sciences is to trace the connection between cause and effect, to ascertain the antecedent conditions which are not only necessary but sufficient to produce observed phenomena. The rigid and exact methods of experimental science have been learned and applied by the physiologist who studies the functions of organs in the vertebrata. But the evolutionist has still to learn the logical meaning of the word "cause." He seems to think that accuracy and caution are only necessary in observation and description, and that when he leaves these to consider the causes of things observed, he enters a region of romance, where he may soar on the wings of fancy, and where discoveries can be made by a lively imagination without the labour of research.

J. T. Cunningham.

IV.

Ticks.

IN many parts of the world people are becoming considerably excited just now on the subject of Ticks. It appears to be specially the case in some of the West Indian Islands, but is not by any means confined to that locality. This is not altogether an unusual occurrence, for the little animals are apt to increase in numbers and to bring themselves vigorously into public notice, and they are always more free than welcome. When I say "Ticks," I am using the word in its more scientific sense, as signifying the Ixodidæ, a family of Acarina; not in the popular sense of almost any objectionable small creature which bites and sucks. Thus, for instance, the "Sheep-tick," which is the dread of our farmers, is not only not an Acarid, but it is not even an Arachnid; it is, if one may be excused for using an apparent contradiction, a wingless dipterous insect, somewhat closely related to the curious *Nycteribia* of the bat, and more closely still to the winged *Ornithomyia* of birds.

The true *Ixodes*, however, can make himself quite sufficiently unpleasant without having also to bear the evil reputation of others. No one whose experience is confined to England, and who has only on rare occasions found a minute black speck firmly fixed in his arm when he returns from beating ferns for insects, or has found that his dog has come out of bushes and long grass possessed of somewhat more life than he carried in, has any idea of what a nuisance these creatures can be in tropical or semi-tropical countries. In the West Indies, parts of South America, the southern part of North America, parts of India, &c., they breed in the long grass and bush in countless numbers and of numerous species. Any large vertebrate visiting the infested locality in the Tick season without being most carefully protected, is apt to emerge covered with a quantity of Ixodidæ, which in the case of man is very unpleasant, and in that of animals, which cannot so readily rid themselves of the pest, is even dangerous, both from irritation and, in the case of cattle continually visiting the infested districts, from loss of blood.

The bite of an *Ixodes* in England is practically painless to most people, but it must be remembered that a small wound which would not inflame here may do so in a hotter climate, and the matter is generally greatly aggravated by the means taken to get rid of it. If the Acarid were allowed to suck its fill, it would then withdraw the

rostrum with the terrible-looking armature of recurved hooks by which it is fixed to the skin of its victim, and drop off; but when irritation is felt scratching or rubbing naturally begins, and thus the *Ixodes* is torn away by the simple process of breaking the horny rostrum short off, and leaving the barbed portion in the wound for nature to remove as best she may. Thus, a short time since I received two lots of Ixodidæ almost on the same day. One series was obtained from North Africa, and had been gathered from bats; these had sucked but little, and there was not a single whole rostrum among them. The others, from the Leeward Islands, were fully fed, and had been carefully gathered from cattle, the rostra being unbroken. People, however, naturally do not desire that the Acarid should be allowed to finish its repast in peace, and the question arises, cannot it be induced to withdraw its rostrum before sucking to repletion? M. Mégnin, of Vincennes, who has studied the question, says that it can, and that a drop of turpentine or benzole will cause it to

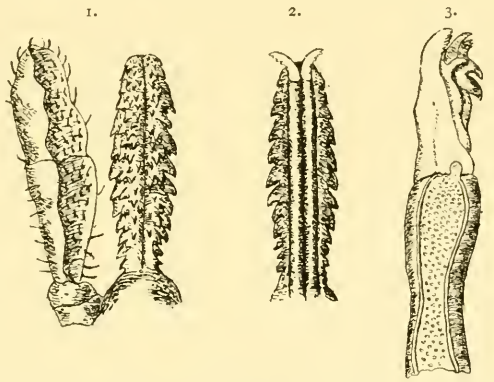

FIG. 1.—Rostrum (maxillary lip) of *Ixodes reduvius*, female (= *I. ricinus*) seen from below mandibles retracted. After Canestrini. One only of the cultriform palpi is drawn.

FIG. 2.—The same part (without the palpus) of *Ixodes lacertæ* seen from above; mandibles protruded. After Pagenstecher.

FIG. 3.—One mandible of *Ixodes reduvius*, female seen from the side. After Pagenstecher.

be so uncomfortable that it promptly removes itself to less unpleasant quarters. I am told that even a drop of oil will have the desired effect in most cases. If there be many Ticks, the question of whether they are allowed to proceed with their meal, or are cruelly removed while still hungry, must be an interesting one to the host, for their capability for sucking is certainly remarkable. When fasting, the male and female are about the same size. The male, however, although not incapable of feeding, like the male gnat, is satisfied with a moderate amount; not so the female, which continues to suck until, in some species, from a size which may almost be described as microscopic, she becomes the greater part of an inch long. Professor Leidy lately had the idea of weighing a considerable number of adult female North American Ticks of one species fasting, and then

weighing the same number of the same species and sex fully fed; and it is startling to learn that the fed specimens weighed more than a hundred times as much as the unfed.

The power of enormous feeding at one time is accompanied, as is often the case, by extraordinary powers of fasting at other times. It has been lately observed by Dr. Strachan, in Jamaica, that young *Ixodes* bred in confinement can live for months without food, and he is inclined to attribute it to their being born with a food-supply; but the knowledge of their ability to live for long periods without food is not by any means new, nor is this capacity confined to the young. Probably it is carried to the greatest extent in the Argasinæ, a sub-family of the Ixodidæ and clearly belonging to them, although formerly, in an unaccountable manner, included in the Gamasidæ by some authors. Some years ago, some dead Acarids were sent to me from Adelaide, Australia, and these proved to be not distinguishable from the well-known *Argas persicus*. As this was an interesting fact, I wrote to enquire if my correspondent could forward some alive, and two or three were accordingly sent; but they were put loosely in a tin box, without food or packing of any kind, nothing in the box except the mites, and in this state they came by post. I was away when they arrived, but on my return, some two months afterwards, I found them all alive. Moreover, numerous young ones had been born, either during the journey or after it, and these also were living. The best instance, however, is that related by MM. Laboulbène and Mégnin. Some time between 1860 and 1870, the Shah of Persia engaged the services of a French physician, Dr. Tholozan, who was a friend of Laboulbène, and was asked by him to examine into the stories concerning the Persian *Argas*, and to send home specimens. In June, 1877, the creatures were sent alive, but without food, packed in cotton-wool, in a box, and wrapped up in paper. Laboulbène sent the box to Mégnin, who mislaid it; just four years afterwards, the box was found and opened. The males had died; the females had had numerous young, which had died; but the females themselves were living, and ready to feed when they got the chance.

This species affords an example of what I said before, namely, that the bite may possibly produce much worse results in a hot country than in England; it was originally described by Fischer, of Waldheim, in 1809, under the name of *Argas persicus*, and he speaks of it as much feared and very dangerous, even destroying life. About ten years later, Maurice Kotzebue, who uses as a popular name "Punaise de Miana," but identifies it with Fischer's *Argas*, gives a terrible account of the creature, saying that its bite produced high fevers, convulsions, delirium, and finally death in twenty-four hours.

Tholozan was not certain whether there was truth in these stories; he found that at Chahroud-Bastam the creatures had a very

bad reputation, and were known by the name of "Guérib-guez," or "strangers beware"; while at Miana there were great differences of opinion, some people—including most doctors—thinking that they were harmless, and that the fevers came from the climate, while others—including most natives—maintained that the fevers arose from the bites of the Acarids. Tholozan sent home specimens from both places. Mégnin tried them on rabbits, and also tried one on his own arm; this bit freely after its four years' fast, and became deep purple instead of dull yellow. For an hour after the creature had left his arm, Mégnin felt as if it were still sucking; for a fortnight there was considerable itching; and six weeks elapsed before the arm was absolutely well. There was not any fever or general illness, but may it not be possible that in a hot and unhealthy climate the wound would have inflamed more, and possibly rendered the patient more susceptible to the unfavourable local conditions, so that there might be some slight foundation for the doubtless greatly exaggerated stories of the earlier writers?

The power of living without food for a long time doubtless explains such cases as the so-called "Canterbury Tick," really *Argas reflexus*, found in the timbers of the roof of Canterbury Cathedral in a place then not accessible to birds, but where it had probably been left by some pigeon years before.

The food of the Ixodidæ is, however, not supposed to be wholly animal. It has usually been considered that they are primarily vegetable feeders who like to indulge in a more generous diet when they have an opportunity, as is believed to be the case with gnats. Of the millions of mosquitoes which in the short Norwegian summer often thicken the air of the Tromsdal—a wild valley within the Arctic Circle, practically uninhabited either by man or probably by beast—how few are likely ever to taste blood? Similarly, it is believed, so far as our present knowledge goes, that the young *Ixodes*, and probably the old ones also in most instances, feed primarily on vegetable juices, only attaching themselves to animals as occasion offers. It seems certain that the eggs are not laid on the animal; the female when her voracious appetite is at last satisfied, *i.e.*, when she cannot contain any more, drops off, and the nutriment serves to supply the store of nourishment to a great mass of eggs which she produces, and deposits on the grass and herbage. The place and mode of deposition is somewhat curious, inasmuch as she places the eggs one by one in front of her in a pile, or round ball, which gradually becomes almost as large as the mother-*Ixodes*. It is probably this position of the eggs which gave rise to the idea held by some of the earlier writers, that the *Ixodes* laid their eggs through the mouth-opening; the fact being that the genital opening is very near the mouth, and the position during oviposition such that the mouth is not readily seen.

The structure of the mouth-organs, which enable the possessor

to fix itself so firmly and also to drop off at will, is interesting. The anatomy of *Ixodes* is not by any means a new subject; from Pagenstecher's almost monographic description, which formed vol. 2 of his "Anatomie der Milben" in 1861, to Professor Batelli's "Comunicazione preventiva" in the *Monitore Zoologico Italiano* for 1891, a good deal has been written about it. I have used the word "rostrum" hitherto for the anchoring organ because that is the name usually given to it; but the appellation is scarcely a happy one, because in other mites it is applied to the whole portion of the body in which the mouth-organs are placed, whereas the anchoring apparatus in *Ixodes* is only one of those organs. It is the homologue of what is often called the labium in other Acari, but which is not in any sense the same as the labium of insects; it is a maxillary lip, answering to the maxillæ of insects or the pedipalps of other Arachnida. It is a paired organ, or perhaps I should say paired organs; but the basal parts of the two sides are broad and flattened, and are fused at their inner edges, so as to form a lower lip closing the mouth and bearing the palpi; the distal parts are rod-like though free, flattened on their inner edges and lying close against each other, but capable of some divarication. On the outer and lower edges these rods are armed with a large number of strong recurved hooks or spines. On the upper surface of this maxillary-lip are two longitudinal channels in which the mandibles play backward and forward. The latter are provided at their distal extremities with elaborate cutting organs, and can be protruded a little beyond the lip. They cut the hole into which the rod-like ends of the lip are pushed; the rods being then slightly divaricated, hold with extreme tenacity; but they also enlarge the hole a little, so that when the creature brings them together again they can be withdrawn, and it can return to its grass-lands and lay its eggs in peace. Another point of interest discovered not long since by Winkler is, that *Ixodes* possesses a heart, or dorsal vessel; and as, with the single exception of *Gamasus*, it is the only heart-like organ known in the Acarina, while its one-chambered form is probably about the simplest arrangement known in the Arthropoda, it is worthy of special remark.

Anyone keeping a large *Ixodes* alive will scarcely fail to be struck by its frequent and wide changes of colour; it is the Chameleon among Mites. An instance of this is related in Sir J. Hooker's "Himalayan Journal," which one would almost hesitate to quote from a work of less authority. Ticks are very common on Lizards, and Hooker found a black and yellow Lizard which had three Ticks on it; one was on a black part, and was black; a second was on a yellow part, and was yellow; the third was just on the line dividing the two colours, and one half of its body was black, while the other appeared yellow.

ALBERT D. MICHAEL.

V.

Physical Geology in the Basin of the Colorado.

THE basin of the Colorado River offers a wonderful series of object-lessons to the physical geologist. The two operations of erosion and crust-movement, in particular, are there exhibited on the grandest scale and in the simplest manner. The region to be noticed comprises the southern and eastern portions of Utah, with parts of the neighbouring States and Territories, Wyoming, Colorado, New Mexico, and Arizona. Utah itself is divided by an ill-defined water-shed, the northern and western parts being included in the Great Basin, which drains only into inland salt lakes. The Colorado River is formed by the union of two great branches, the Green River, which rises in Wyoming and runs south, and the Grand River, which heads in the Rocky Mountains of Colorado, and has a general westerly course. The united waters flow through northern Arizona, in the famous Grand Cañon, and finally reach the Gulf of California.

Physically, the region is a remarkable one. It has, indeed, mountain-chains, some like the Wasatch, the westerly front range of the Rocky system, rising 11,000 or 12,000 feet above sea-level; but essentially it is a country of high plateaux having more or less of the character of deserts. The highest plateaux, bordering the Grand Cañon, sometimes reach an elevation of nearly 9,000 feet.

The phenomena of erosion are intimately connected with the climate, which is an extremely dry one, owing to the great range of the Sierra Nevada, cutting off the moisture from the Pacific. Not only is the annual rainfall very small, but it is concentrated in a few violent storms, leaving the country absolutely parched during the greater part of the year. Except on the highest plateaux and mountains, vegetation is generally restricted to a few hardy forms. The only permanent streams are those fed by the snows of the Rocky Mountains. It would be a great mistake, however, to suppose that aridity of climate implies slowness in denudation. There is every evidence that the general surface of the country is undergoing degradation and removal at a rapid rate. Over large tracts the strata from the Permian to the Eocene inclusive, with an average thickness estimated by Captain Dutton at 10,000 feet, have been completely removed, and the beginning of the process cannot date back farther

than middle Tertiary times. A geologist is at first staggered by the proofs of enormous denudation evident everywhere in a country apparently devoid of water; but the mystery is solved when he has the fate to be overtaken by one of the erratic storms which are a characteristic feature of the region. The whole surface of the plateau is covered with running water; the accumulated detritus of the rocks is hurried along by boiling torrents where an hour ago were only dry gullies; and the larger streams abundantly justify the name which several of them bear—Rio Puerco, the dirty river. The great diurnal variations of temperature have already disintegrated the surface of the rocks; there is no vegetation to receive the direct impact of the rain, and no soil to impede the transport of the material; the rain has thus an enormous power for degradation, a power which is, indeed, a direct consequence of the dryness of the climate.

It is when a tract of homogeneous, horizontally-bedded rocks is subjected to action of the kind described that the typical "Bad Lands" are seen. The country is studded with thousands of bare hills, following one another in close succession as far as the eye can reach, and divided only by a network of dry channels. When harder strata alternate with softer, the simplicity of contour of the hills is modified by projecting ledges, and we have what Major Powell has called "Alcove Lands." The alternation of ledge and talus, or, on a larger scale, of cliff and slope, is strongly marked, and the emphasis with which the stratification thus expresses itself in the surface-configuration is again a consequence of the dry climate, which makes the impact of the rain so formidable a factor in erosion. In a country like England the soil holds the water, which is enabled to effect disintegration in the rocks beneath, whether hard or soft, and the differences of existence are minimised. In an arid region the soft rocks are destroyed rapidly, leaving the more resisting beds in strong relief, and the only limit to this is fixed by the undermining of the ledges, which removes their support.

The traveller on the Rio Grande Western Railway over the two hundred miles from Castle Gate to Grand Junction traverses a magnificent panorama of bad lands and alcove lands, the stratification being here, as is often the case, accentuated by brilliant contrasts of colours in the naked rocks. During most of the journey he has in sight on his left a range of steep cliffs 2,000 feet high, known as the Book Cliffs. These are an example of a feature common in the region —the escarpment cliff, or "cliff of erosion," dividing a higher from a lower plateau. Such abrupt escarpments characterise gently inclined formations of alternating hard and soft characters, the cliff being formed by a hard rock resting usually upon a soft one. There may be two or three parallel escarpment cliffs built of successive formations, as in the plateaux traversed by the "Terrace Cañons" of the Green River. The surface of each platform is approximately a bedding plane, inclining gently towards the cliff which overlooks it.

The surface drainage is in the direction of this slope, and then along the foot of the cliff to one of the permanent rivers. The directions of these permanent rivers bear no such relation to the cliffs, and give rise to an important observation.

Powell has pointed out that when the Green River in its southward course encounters the great range of the Uinta Mountains, lying directly across its path, instead of being diverted, it cuts right through the range and follows its own course. Again, in crossing the Yampa Plateau, the same river cuts directly through the highest ground in the district in a deep gorge appropriately named Split Mountain Cañon. This entire independence of the mountain and plateau systems characterises all the main water-courses of the region. Several rivers rising on the eastern slopes of the Wasatch run directly westward through the heart of the range, and fall into Great Salt Lake. Of such phenomena there is only one explanation; the rivers existed before the mountains and plateaux; in other words, we have a striking example of an "antecedent" drainage system. As an elevation was gradually formed in the path of a river, the latter exercised its most active erosive power upon the part of its bed thus raised, and was able to keep its channel cut down to the proper level, and maintain its right of way. In order that the river should not be diverted, erosion must have been able to keep pace with elevation throughout the whole time, and we have here a clear proof of the very gradual operation of the elevatory forces.

Such movements of elevation as those just indicated have affected in varying degree the whole region, though sometimes the amount of the elevation can be shown to have differed by thousands of feet in adjacent tracts. The rivers, meanwhile, have maintained approximately their original position relative to sea-level. Hence arises one of the most remarkable features of this part of America: the rivers flow in deep narrow gorges or cañons instead of meandering in broad valleys like the streams of a country comparatively stationary. The cañon-type of valley, which is by no means confined to arid regions or to rocks of any particular character, seems to belong especially to regions undergoing uplift, where the rivers, entirely occupied in cutting downward to maintain their grade, have had no energy to spare for eroding laterally.

Bearing this principle in mind, the well-preserved cañon-form of the valleys in the Colorado basin shows that the process of elevation initiated in middle Tertiary times is still in progress. A very striking evidence of this is seen on the abrupt western flank of the Wasatch Mountains. The generalised structure of the range is that of a sharp uplift bounded to the west by a fault of at least 6,000 feet throw. The movement along this fault has doubtless been effected gradually, and a portion of it is clearly of recent date, for where the gorges in the mountains debouch on the great plain of the Salt Lake the fresh fault-scarp is often seen truncating the terminal moraines

of the Glacial Epoch and the still later alluvial cones of the torrential streams.

The uplift of the plateau-districts, as already stated, has been differential. The high plateaux bordering the Grand Cañon stand at different levels. They have horizontal or very gently inclined surfaces, and are separated by cliffs often 2,000 feet in height. Examination has shown that one geological horizon, the summit of the Carboniferous, forms the surface of all these plateaux; consequently the cliffs are actual "cliffs of displacement." In most cases they are fault-scarps, only slightly modified. Sometimes a monoclinal fold is found instead of a fault, the two not being essentially different, but passing into one another. The structure of the country is, then, that of a number of blocks uplifted independently and to different elevations, and bounded by faults or monoclinal flexures. The mountain chains of the region have in general the same kind of structure with more complication and more modified by subsequent erosion. This type of mountain-structure is known to American students of orography as that of the Great Basin ranges. It is characterised by broad monoclinal folds, normal faults, and a form due in the first place to the mountain-building forces. It is strongly contrasted with the Appalachian type with its closely compressed acute folds and reversed faults and a contour due entirely to the agents of erosion. The one type speaks of vertical elevation, the other of lateral compression.

The features of interest to the physical geologist exhibited in the region are too many to be noticed in a short article. In some districts igneous rocks have played an important part in the structure of the country. Besides the typical laccolites of the Henry Mountains, so lucidly described by Gilbert, there are great bodies of intrusive rock connected with much more extensive uplifts. Extinct volcanoes and lava-flows of various ages are seen in large numbers on some of the high plateaux. The lavas are harder than the sandstones and other strata of the district, and so the older flows often form the capping of broad, flat-topped "mesas" standing high above the surface of the surrounding country which had no such protection. Later outbursts of lava on the margin of the elevated tract may extend the protected area, and from successive periods of eruption and erosion arise large mountain-masses consisting externally of lava but internally of stratified rocks long ago removed from the surrounding plateau. Such is the character of the Uinkaret Mountains and of Mount San Francisco, the latter rising nearly 6,000 feet above the plateau on which it stands.

Most of the features noticed above, and many others of equal interest, are typically displayed in the Grand Cañon of the Colorado, an unrivalled example of erosion in an arid climate and in a rising region. Near its upper end it has an average width of ten or twelve miles, with a depth of more than 6,000 feet. Between the

river and the Coconini Plateau above, a succession of life-zones is comprised equivalent to the range from the cactus-plains of Mexico to the pine-forests of northern Canada, the effects elsewhere due to difference of latitude being here given by difference of altitude. In the winter, when the snow lies deep on the plateau, humming-birds flit about among the shrubs in the depths below.

The precipitous form generally associated with the idea of a cañon and finely exemplified here in the many tributary gorges, gives place in the Grand Cañon itself to a more complex structure. There is a wide outer cañon floored by a broad platform, and through this is cut the deep inner cañon at the bottom of which the river runs. The walls of the main trunk are broken by many minor chasms, enormous if considered by themselves, giving rise to deep recesses and long projecting ridges. These branch-cañons again have their tributaries, and when two of the latter have cut back so far as to meet, the salient point is left standing as a great castellated "butte."

Of the spectacle as a whole, no description, however vivid, could give any adequate suggestion. A part of the deep impression it produces is due, as Dutton has well remarked, to the startling suddenness with which it bursts upon the traveller. He may have been traversing for days a gently rolling plain, monotonous almost to weariness, and apparently illimitable: he ascends one more gentle undulation, and, as if raised by a magician's wand, a new world is before him. The endless detail of cliff and talus, butte and alcove, is at first bewildering. The imagination is perplexed for comparisons, but perhaps the dominant impression is that of Titanic architecture. Monuments, pinnacles, buttresses, castles, temples, appear in wonderful diversity, and crowd the distant perspective away to its vanishing point. But soon order begins to appear in the confusion. Guided by the characteristic colour of each formation, buff or crimson, lilac or chocolate, the eye can follow the level bedding into deep amphitheatres and round projecting headlands for fifty or sixty miles. In the broad platform which seemed to be the bottom of the cañon is seen the steep inner gorge, only in places disclosing the river in its channel of dark gneiss. For a juster view we must take our stand on one of the abrupt promontories, such as Spanish Point where Coronado, the earliest explorer of the country, obtained his first view. Here we look westward along the cañon, and the true structure of the whole is clearly revealed.

Far below, at the bottom of the dark inner gorge, rushes the Colorado River, which throughout the two hundred miles of the cañon is a close succession of headlong rapids. As seen from the plateau, its waters, brown from their burden of mud, rise in huge standing waves, a fearsome sight to one who thinks of navigating them. In 1869, Major Powell, now Director of the U.S. Geological Survey, started on a voyage of exploration from Green River Station with a small party in life-boats. Passing down the long

chain of cañons of the Green and Colorado Rivers, he emerged, after fourteen weeks of adventurous travel, at the mouth of the Rio Virgen, below the Grand Cañon. When this voyage through an unknown country was projected, the engineers opposed it as an impossibility. On a river falling more than 5,000 feet in a thousand miles it was held that great water-falls must be met with. Powell was convinced to the contrary: there could be no falls on so muddy a river. In other words, he saw that a stream so intensely active in erosion, as this detritus-laden torrent must be, would always keep its channel cut down to a fairly uniform grade. This foresight is worth quoting as an example of the value of geological reasoning in an untravelled region.

Although there are no falls, there are terrible rapids. The most formidable of these occur where the bed of the rapid river is choked with the huge boulders brought down by its far more rapid tributaries.

The Grand Cañon is the last of the long chain of deep gorges through which these waters flow. Below it, the Colorado emerges into an open country at a lower level, and so pursues its way with more of the usual characteristics of a large river.

It is difficult in any description to avoid emphasising whatever is superficially striking, but the impression of strangeness produced by great magnitudes or unusual simplicity of structure is one to be combated by those who cherish the principles on which modern geology is founded. If the science is built on the laws of Nature, these laws must be the same in the Old World as in the New. Deserts, high plateaux, monoclines, fault-scarps, cañons—these things are not peculiar, but only peculiarly striking manifestations of principles with which geologists have to reckon; and British geologists especially will not fail to find in such features valuable clues to the problems offered by the history of their own island.

ALFRED HARKER.

VI.

Sigillaria and Stigmaria.

I.

THE short notice of M. Grand' Eury's admirable memoir on the Geology and Palæontology of the Coal Basin of the Gard,[1] to a copy of which I am so fortunate as to have access, raises some questions which the older workers on *Sigillaria* and *Stigmaria* had supposed to be settled, and on which it may be useful to make a few definite statements.

1. It is quite certain that *Stigmariæ* are not "rhizomes which floated in water or spread themselves out on the surface of mud." Whether rhizomes or not, they grew in the soil or in the upper layers of peaty deposits since changed into coal. The late Richard Brown and the writer have shown that they grew in the underclays or fossil soils, and that their rootlets radiated in these soils in all directions.[2] In one of my papers I have figured a Stigmarian root penetrating through an erect *Sigillaria*, and Logan in his Report of 1845 had already figured a similar example. The penetration of decaying stems by the rootlets of *Stigmaria* is a fact well known to all who have studied slices of Carboniferous plants,[3] while *Stigmariæ* are often found creeping inside the bark of erect and prostrate trunks. Besides this, as I have shown in "Acadian Geology," in the section of 5,000 feet of Coal-measures at the South Joggins (including seventy-six distinct coal groups, and a larger number of soils with *Stigmaria* or erect trees) *Sigillaria* and *Stigmaria* occur together, and the latter nearly always either in argillaceous soils or sands hardened into "Gannister," which are often filled with roots and rootlets, or on the surfaces of coal-beds. On the other hand, the numerous bituminous limestones, and calcareous and other shales holding remains of fishes, crustaceans, and bivalve shells, do not contain *Stigmaria in situ*—the only exception being two beds of bituminous limestone, the upper parts of which have been converted into underclays. This section, and that of North Sydney—two of the most complete and instructive in the world—have afforded conclusive proof of this mode of growth of *Sigillaria* and *Stigmaria*.

[1] NATURAL SCIENCE, March, 1892.

[2] *Quart. Journ. Geol. Soc.*, vol. ii., p. 394 (1846); *Ibid.*, vol. iv., p. 47 (1847); *Ibid.*, vol. v., p. 355 (1849); *Ibid.*, vol. v., pp. 23, 30.

[3] Williamson has noticed this in his excellent memoirs in the *Phil. Trans.*

2. Stigmarian roots are not confined to *Sigillaria*. Brown has figured and described the Stigmarian roots of a *Lepidodendron*.[4] His specimen, it is true, was not very characteristic in so far as the stem is concerned, resembling rather the fossils known as *Lyginodendron*, but I subsequently found at Horton Bluff the Stigmarian roots of *Lepidodendron corrugatum*, a lower Carboniferous species very near to *L. veltheimianum*. This is described and figured in my report on the Lower Carboniferous Flora of Canada.[5]

3. The bulb-like appearance figured by Grand' Eury at the base of *Sigillaria* is, in my experience, an exceptional phenomenon. Of more than a hundred erect *Sigillariæ* which I have examined or extracted, it occurred in only two or three, the others having trunks spreading at the base in the manner of ordinary trees.[6] It may be a specific or varietal character, or a result of the expansion of the lower part of the stem above the line of insertion in the soil and attachment of the roots, just as an exogenous stem becomes expanded above a ligature; or perhaps in some cases it may be an effect of lateral squeezing applied to the more compressible matter, partly organic and partly argillaceous, often filling the lower part of the stem. Erect Calamites are in this way sometimes made to assume a beaded appearance by compression between the nodes.[7]

4. The use made by Grand' Eury of the terms *Syringodendron* and *Stigmariopsis* requires some explanation. The appearance compared to *Syringodendron* at the base of *Sigillariæ* has really no connection with the genus *Syringodendron* as described by Sternberg and Brongniart. This genus includes a peculiar type of Sigillaroid tree, but the base of any ribbed *Sigillaria* assumes the appearance of having double rows of round marks or punctures, owing to the obliteration of the leaf-bases and separation of the vascular marks caused by expansion of the bark. Examples of this will be found in the Quarterly Journal of the Geological Society, vol. x., p. 32, figs. 9, 10, 11; and in "Acadian Geology," p. 180, fig. 30.

The term *Stigmariopsis*, on the other hand, might be fairly applied to those *Stigmariæ* which, like the roots of my *Sigillaria* (*Leioderma*) *sydnensis*, have a wrinkled surface and somewhat irregular areoles.[8] But an appearance similar to this generally occurs on main roots of *Sigillaria*, near to the stem, and arises from the same kind of expansion already referred to as occurring at the base of the trunk. This is, indeed, the explanation deducible from Grand' Eury's description and figures.

5. I am not aware of any instance of a stem of *Sigillaria* springing as a bud from a branch of *Stigmaria* or its termination. There are abundance of examples (quite numerous in my collection) of the

[4] *Quart. Journ. Geol. Soc.*, vol. iv., p. 46 (1847).

[5] Geol. Survey of Canada, 1873. [6] "Acadian Geology," p. 192, figs. 1, 4 (1855).

[7] Figured in "Acadian Geology," p. 198. [8] "Acadian Geology," pp. 435, 475.

conical tap roots springing from the lower sides, but nothing rising from the top, except where a *Sigillaria* stem crowns the meeting of its four or eight main roots.

6. The objection to calling the *Stigmariæ* roots and their processes rootlets, appears to me a finical application of modern botanical usages to times for which they do not hold. We might equally object to the application of the term roots to those which spring from the earthed-up stems of Calamites, radiating as they do from nodes which, in the air, would produce branchlets. Grand' Eury's figures show abundant instances of this. We might also object to the exogenous stems described by Williamson, which belong to cryptogamous plants, and, unlike anything modern, are made up exclusively of scalariform tissue. If the articulation and regular arrangement of those gigantic root-hairs, the rootlets or leaves of *Stigmaria*, are to be regarded as depriving them of the name which clearly describes their function, we may call them underground branches, though, by so doing, we set at nought both their function and their mode of growth.

7. The above remarks show, so far as they go, how completely the doctrine of *Stigmaria*-underclays and the terrestrial growth of coal, as originally explained by Sir William Logan, are borne out by the facts; and I may refer, in further illustration of this, to the remarkable history of the erect *Sigillariæ*, containing remains of land animals at the South Joggins, as given in my memoir on erect trees containing animal remains.[9]

In making the above statements, I would not be understood as derogating in the least from the merits of my esteemed confrère, M. Grand' Eury. His work on the Carboniferous Flora of the Department of the Loire, and the later work above referred to, are triumphs of that careful and elaborate exploration of fossil plants *in situ* which, while most laborious and difficult, gives us the only certain means of restoring, in their integrity, the plants of the old coal forests. No labourer in this arduous field deserves greater credit than Grand' Eury; but, to obtain their full benefit, the results of his labours require to be compared with those of others in different localities, and to be interpreted, not on the limited rules of modern botanical nomenclature, but in accordance with wide views of the versatile methods of nature, more especially in the older periods of the earth's history.

J. William Dawson.

[9] *Phil. Trans.*, 1882, pp. 621–659. I may also refer to a paper by Wild in the last number of the *Trans. Geol. Soc., Manchester*, vol. xxiv , pt. 13 (1892).

II.

The publication of a new work (Géologie et Paléontologie du Bassin Houiller du Gard, 1890) by my old friend M. C. Grand' Eury has again raised the question of the nature of the *Stigmaria ficoides*. My own views on the subject, based wholly upon our British specimens, were embodied in full detail in my Monograph published in 1887 by the Palæontographical Society, and nothing has occurred since its appearance to require me to alter a single statement of any importance which that memoir contains. In it I confined myself, as I have said, wholly to the study of British examples of the plant, paying no attention to the figures and descriptions of Continental specimens which I had no opportunity of studying personally. Hence, respecting such specimens I gave no opinion.

M. Grand' Eury has not, either in his new figures or his descriptions, entered into much detail, but he has reiterated in all its decisiveness the French hypothesis that the *true Stigmariæ* are not roots, but rhizomes, "qui, ayant été incapables de se soutenir, ont flotté dans l'eau ou rampe sur la vase qu'ils ont aussi pénetrée" (p. 236). "Une fois seulement je les ai surpris divergeant d'un centre sous tige." We have here a statement altogether inapplicable to anything that we discover in Great Britain. His hypothesis appears to be identical with the vague and speculative guesses that were prevalent among us in the early years of the present century, and to which the discoveries of Sir William Logan put an end.

Whenever we discover a perfect example of our British *Stigmaria*, like my magnificent one from Clayton now in the museum of the Owens College at Manchester, we invariably find it to consist of a circle of dichotomously-branching roots radiating from a centre whence ascends a stem either of a *Sigillaria* or a *Lepidodendron;* and the recent discovery of several similar, though smaller, examples at Osnabrück in Germany, chronicled by M. Potonie,[1] are exactly of the same character. M. Grand' Eury divides the objects which he discovers in France into two groups. To one of these groups he gives the name of *Stigmaria*, and this he considers to be something very different from our roots of *Sigillaria* and *Lepidodendron*, to which we apply the same name. He describes his Stigmariæ as long rhizomes, not varying much in diameter, or radiating from a common centre. Once only he appears to have met with a specimen which did so radiate. The appendages to these "rhizomes," unless I misunderstand him, which appear to me to differ in no respect from the *rootlets* of our British examples, he and M. Renault regard as leaves. On the other hand, he finds

[1] "Jahrbuch der königl. preuss. geologischen Landesanstalt," 1889.

examples which, so far as I understand his views (which I must confess I am unable to do very distinctly), seem to be identical with our British type, but to which he applies his name of *Stigmariopsis*.

Now, to all this I entirely object. Our British forms are those which have been recognised as the true *Stigmaria ficoides* ever since the days of Sternberg and Artis; and I contend that if our French friends have discovered in their coalfields a new type, it is not for them to divert our old name to their new forms, but to invent for them a new name, *Stigmariopsis* or any other, and leave us in the possession of our own time-honoured title.

Notwithstanding all that has been written by MM. Renault and Grand' Eury on this subject, and which I have studied with careful attention, I am still unable to understand clearly what their views are, beyond their conclusion that some of these objects are rhizomes furnished with leaves, while others are true roots. When I try to comprehend which are the actual examples of the one and the other, I fail to do so. Anyhow, I claim for *all* our British forms, *first*, that they represent the true *Stigmaria ficoides* of the many authors who have written on the subject, and, *second*, that these forms are *roots* furnished with rootlets, and not rhizomes bearing buds and leaves. Imprimis, they are *roots* employed to fix and sustain a noble aerial stem in the ground, and also to extract from that ground such nitrogenous and mineral substances as the giant aerial Lycopodiaceous structures require for their nutrition.

My belief that the appendages which clothe these roots are rootlets and not leaves, is shown by their organisation. Like the roots of recent Lycopods, each one has a single remarkable *monarch* vascular bundle, which M. Van Tieghem first showed to be so characteristic of the roots of the Lycopodiaceæ. Then they possessed that remarkable root-like power and habit of distributing themselves, penetrating every minute aperture into which they could, by the faintest possibility, obtain an entrance, in search of nutriment, a faculty which is wholly foreign to leaves of every kind and degree; besides this, the fact of their subterranean position altogether unfits them for performing any foliar functions. The aerial stems and branches were magnificently supplied with real foliar organs, to which the performance of these subaerial functions was committed. Organs so different in structure and disposition could not possibly be merely modified homologues of an identical organ. Then we have the cardinal distinction, so fundamentally characteristic of almost the entire vegetable kingdom, that these roots are geotropic organs, while the leaves belong to a heliotropic system.

A further proof that the appendages to *Stigmaria* cannot be leaves is shown by the *origin* of their vascular bundles. Those of the true aerial leaves spring solely from the primary, or non-exogenous xylem, which terminates at the base of the aerial stem, and never enters the roots. The vascular cylinder of the latter organs, on the

other hand, consists of the secondary or exogenous xylem, and it is from this structure that the monarch bundles of the rootlets are derived. Two such absolutely different zones of the wood could not supply homologous organs.

I repeat, therefore, my conviction that our true British examples of *Stigmaria* are, and can be none other than, true roots; and, so far as my limited acquaintance with the German specimens enables me to form an opinion, they appear to belong to our British type. That some different objects may be discovered in the French collieries, I am alike unable to affirm or to deny; but I am compelled to entertain doubts on the subject from more reasons than one. I am unable to reconcile the descriptions of M. Renault with those of Grand' Eury. I cannot obtain any clear and definite account of the nature of the bulbs supposed to be developed at the under surface of the so-called rhizomes. In my Monograph (*loc. cit.*, fig. 18) I have described roots of our *Stigmariæ* from a specimen which cannot have been much more than one-fifth of an inch in diameter, to the large Clayton example in the Owens College Museum, which extends nearly thirty feet from side to side; yet in none of these do we discover the faintest trace of any bud or bulb, nor yet of any indication of a point to which such a growth could have been attached. The idea of the possibility of some gigantic protonemal condition having been the first product of the germinating Lepidodendroid spore has for some time past suggested itself to my mind, such a protonemal state being represented somehow by the "Rhizomes" and "Bulbs" of which the French authors speak; but, until we obtain more exact details and figures of these French structures than we at present possess, I must confess to a large measure of doubt being left on my mind in reference to the entire subject of these supposed discoveries and the conclusions based upon them.

Further, when Grand' Eury is describing what appear to me to be the French representatives of our British *Stigmariæ*, he defines their roots as "court." One of these roots, which I measured at Clayton, was 37 ft. 4 ins. long, and while at its thick base the root was more than 8 ft. in circumference, it tapered away at its apex to a mere point.

My valued friend tells me that there is really less difference between his views and mine than I believe to exist, and the acquisition of more exact details and clear definitions than we have yet got may prove that this is the case. He is too indefatigable and distinguished a geologist, as well as too conscientious a man, to make it possible to doubt that he is in the possession of some facts which have no parallels in Great Britain. How far what at present appears to be his interpretation of those facts will prove correct, yet remains to be ascertained. Personally, I am yet as incapable of understanding them as of rating them at their true value.

W. C. WILLIAMSON.

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Fig. 3.—The Southern Walls of Chimborazo: under Point Veintemilla. B. Upper Cliffs; D. Lower Cliffs; E. Vertical Section of the Glacier on the Summit; F. Possibly the point reached by Humboldt and Boussingault; X. A Lichen found here.

SOME NEW BOOKS.

Travels amongst the Great Andes of the Equator. By Edward Whymper. With "Supplementary Appendix" by H. W. Bates, T. G. Bonney, G. A. Boulenger, &c. 2 vols. 8vo. London: J. Murray, 1892. (Vol. ii., 1891.)

It is now nearly twelve years since Mr. Whymper returned from his expedition among the Andes of Ecuador, and probably the first impression upon receiving the two handsome volumes, in which he records his experiences and discoveries, will generally be one of irritation at having had to wait so long; and this feeling is somewhat excusable, for Mr. Whymper unquestionably ranks as the most popular English writer on mountaineering. Other climbers may have led the way up more difficult peaks, have added more to our knowledge of the geology of the higher ranges, or have possessed a sounder grasp of the broad outlines of Alpine topography; but Mr. Whymper's connection with one of the most famous of Swiss mountains, his clear and easy English, his racy humour, and his exquisite wood engravings, have roused an interest in his achievements among a much larger circle of readers than usually follows the progress of mountain exploration. As soon as we get into the book, however, we find that the time has not been wasted, for every page gives evidence of the conscientious care and elaboration which is typical of Mr. Whymper and all his works. He did not go to South America simply to "do" Chimborazo. Mr. Whymper treats his mountains with more respect, and prefers to carry them by a regular campaign rather than a sudden assault. Moreover, besides ordinary exploration, he wished to ascertain the results of a long residence at high elevations, and to test the accuracy of the aneroid as compared with the mercurial barometer. For these purposes he would have preferred a higher region, but political troubles then rendered both the Himalaya and the Southern Andes unsuitable for experiment, and so he had to put up with Ecuador. The preparations were most elaborate, and occupied as long a time as the journey itself. Perhaps the only point one feels inclined to question was the wisdom of the selection of J. A. Carrel as chief guide. The mountaineering seems, as a rule, to have been quite easy, and to have afforded few opportunities for the display of Carrel's prowess as a rock climber; there were difficult rocks such as the aiguilles on Chimborazo (Fig. 1)[1] but they never came in the way, and for most of the time the guides were only employed as porters, or in other still less congenial work, which could have been equally well done by a man less generously endowed with self-will than the famous J. A. Carrel.

Mr. Whymper and his guides landed in Guayaquil on the 9th

[1] These figures have been reproduced by the kind permission of Mr. Whymper, but they are inadequate representations of the originals.

December, 1879, and he remained in Ecuador till the end of the following July. He first made for Chimborazo, the highest peak in this part of the Andes, and one which had never previously been scaled. This was successfully ascended on the 4th January; the summit was reached from a camp, the highest of three, placed at an elevation of 17,285 ft. At the second camp, both Mr. Whymper and the Carrels were prostrated by mountain sickness in a very acute form; this rendered it necessary to proceed slowly so as to become gradually acclimatised to the low atmospheric pressure. Mr. Whymper proposed to camp on the summit, but the Carrels struck work and declined to reascend, for Louis Carrel, having failed to provide himself with gaiters, had had his feet frost-bitten; as it was their own fault, the guides concealed this fact as long possible. Mr. Whymper was also taken ill, so that climbing had to be temporarily abandoned, and the party retired to Ambato to be doctored. On recovery, they went north to Quito, ascending Corazon (15,871 ft.), and Cotopaxi (19,613 ft.) on the way. They camped for a night close below

FIG. 1.—Aiguilles on Chimborazo.

the summit of the latter, and Mr. Whymper draws a most graphic picture of the interior of the crater at night. Just as a treat to the guides, as a change from the "severely scientific" ascent of Cotopaxi, Mr. Whymper turned aside and climbed the sharp peak of Sincholagua: it was the first ascent and it will probably be the last for a good long time. After passing through Quito, Mr. Whymper made the first ascents of Antisana and Cayambe, the third and fourth peaks in height in the Northern Andes. Disappointed in the eastward view from Cayambe, he crossed the watershed, and after a miserable week of perpetual rain in a district that was as wet as a saturated sponge, he climbed Saraurcu; but the mists again robbed him of the cherished view into the basin of the Upper Amazons. The quest for antiquities then led Mr. Whymper into the extreme north of Ecuador, where he found old pottery, and stone implements, and the way up Cotocachi (16,301 ft.). On the return journey, he reascended

Chimborazo, witnessing on the way a violent explosive eruption of Cotopaxi, which in less than a minute shot a cloud of ash to a height of 20,000 ft.; the column was then deflected at right angles by a strong upper current, and spreading out as a cloud made the summit of Chimborazo dark at midday (Fig. 2).

Those who turn to Mr. Whymper's book in the hope of finding stirring stories of difficult climbing will be disappointed; Mr. Whymper's narrow escape by a fall through an *ice*-covered crevasse is almost the only thing worth calling an adventure. Bad weather gave them serious trouble and at times rendered the camp "as comfortable as a wet ditch on a winter's night," while an inn was discovered at Ambato with more fleas per square yard than Mr. Whymper had previously met with, so the record of the Alpine hay chalets is broken at last. But the volumes make up in careful observation and scientific results for any lack of sensational incident. Mr. Whymper has shown that instead of the orthodox two ranges of Cordilleras, the western one rising steadily from the sea, the main chain of the Andes consists of a rather irregularly scattered series of peaks and basins, while an important

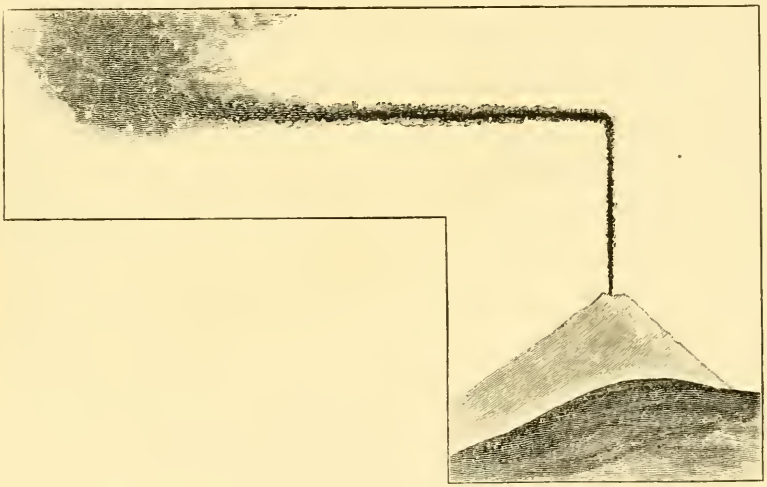

Fig. 2.—Commencement of the Eruption of Cotopaxi.

series, named by Mr. Whymper the "Coast Range," occurs between the Andes and the Pacific; the mountains of the district are all of volcanic origin, except Saraurcu, the easternmost peak reached, which is composed of crystalline schists, referred by Professor Bonney to the Upper Archæan; the rest are of andesites. Only Cotopaxi and Sangai are permanently in eruption, and in the others the craters are probably buried in snow and ice. Mr. Whymper has also shown that the higher mountains are glacier-covered, and found traces in places of the former greater extension of the glaciers. Clear evidences of a stone age have also been discovered.

One of Mr. Whymper's main objects was, as we have said, to determine the exact influence of the low pressures of high altitudes upon men, and whether it would be possible gradually to become acclimatised to them sufficiently for it to be possible to scale the highest of the Himalaya. Mr. Whymper shows fairly conclusively

that low pressure has a distinctly weakening effect, even after a six months' residence, but that the acute form of mountain sickness is only temporary; hence there is good reason to hope that by rising by slow and easy stages the very highest summits on the earth may be reached. One of the most valuable contributions made to geographical science by the expedition was the comparison of the aneroid with the mercurial barometer; two of the latter were carried, and a whole battery of aneroids. Distrust of the aneroid has often been expressed, and Mr. Whymper shows with what good reason. The main record of this work has been given in a separate pamphlet, but such constant reference to it is given that the main conclusions may be referred to. The aneroid appears to be perfectly reliable for *sudden* differences of pressure, but in a long and gradual ascent the aneroid drops considerably below the right point, and continues to drop after it has been brought to rest. This error amounted on the summit of Chimborazo to 1·152 in., and recorded the summit as 1,500 feet too high. By a prolonged series of experiments, after his return, Mr. Whymper maintains that he has found out the cause of this error and how it can be allowed for, so that implicit faith can in future be placed on aneroid measurements. We very, *very* much wish we could believe it.

Mr. Whymper's zoological collections were very extensive, and part of them are described by various specialists in the second volume. Mr. Whymper is heartily to be congratulated on having devoted his attention to the lower vertebrates and invertebrates, instead of spending all his time in trying to catch a puma two inches above record length, or a lama with two tails. The special interest of the collection is in regard to its bearing on the theory that the fauna of the higher Andes is a relic of a temperate fauna, and worked its way across the tropics during the glacial period. The collections made by Mr. Whymper show that the high fauna is simply composed of representatives of the genera of the adjoining tropical lowlands; the genera characteristic of the temperate regions both north and south do not occur in the area, and a pretty theory of a faunistic migration falls to the ground.

But probably the most interesting results of Mr. Whymper's studies are what he has taught us to unlearn. Ecuador was once a wonderful place, and contained wonderful animals whose achievements were vouched for by eminent authorities; but Mr. Whymper has mercilessly unmasked several of the most startling of these marvels. Thus, the grand old Condor which, in the words of Professor Orton, "can dart in an instant from the dome of Chimborazo to the sultry coast of the Pacific," and which Humboldt saw soaring over the loftiest summits of the Andes, turns out to be a sorry fraud. Mr. Whymper's observations show that at present the Condor is restricted in Ecuador to a range of from 9,000 to 16,000 ft. Humboldt's Condors may, perhaps, have been frightened up by one of the revolutions to which they have now grown accustomed; and, until Professor Orton tells us how his bird was identified at the various stations it passed at the fine pace of over 400,000 miles an hour, perhaps he will excuse our accepting his testimony as worth anything, although he be a Professor of Natural History in New York, and was sent out by the Smithsonian Institution.

Serious doubt is thrown by Mr. Whymper upon the whole of Humboldt's story of his exploits on Chimborazo, with his ascent to the height of over 19,000 ft. and his marvellous descent of 3,600 ft. along

a difficult arrete in an hour, a rate which the astounded Whymper declares to be "simply divine;" but, as Humboldt always spoke of the summit as single instead of double, and as he said that there were no glaciers upon the mountain, whereas the whole summit is clad with them, and it is impossible to reach anything like the height Humboldt claimed to have done without crossing a good lot of glacier, it seems difficult to know what weight should be attached to his account. But before we dismiss it as an entire fiction we may remember that in much later times Professor Whitney and a party of American geologists succeeded in surveying Mount Shasta, and reporting a complete absence of glaciers, whereas all subsequent visitors have found several.

Another story that has long done duty in our text-books, on Humboldt's authority, is that of the subterranean fish erupted from Cotopaxi alive and dead, and in such vast numbers that, if his account had been accepted literally, geologists would have included fried fish among the normal volcanic ejectamenta. But after Mr. Whymper's *exposé*, *Cyclopium cyclopum* may join the resurrected rotifers as the latest addition to the rank of zoological fables.

The general results of Mr. Whymper's expedition are certainly of very high value; he has cleared the ground of many serious blunders, and given us a good basis of solid fact and accurate observation which must prove of permanent service in further explorations of the geography, geology, and zoology of the Ecuadorian Andes. Mr. Whymper has obviously been to enormous trouble, both in the field and at home; but we hope he feels that the labour has not been excessive, for his scientific results are of great value, and he has issued them in a work so well illustrated, bound, and printed, that it will at once take its place as one of the choicest classics in the literature of mountaineering.

J. W. G.

THE LABRADOR COAST: A Journal of Two Summer Cruises to that Region. With Notes on its Early Discovery, on the Eskimo, on its Physical Geography, Geology, and Natural History. By A. S. Packard. 8vo. Pp. 513. With Maps and Illustrations. New York: N. D. C. Hodges; London: Kegan Paul, Trench, Trübner & Co., 1891. Price 18s.

SINCE in these days of specialism it is but seldom that an author is qualified to write of a comparatively unknown region alike from the geographical, geological, and zoological points of view, Dr. Packard's work ought to appeal to a wider circle of readers than the majority of books of scientific travel. And so well has the author accomplished his task, that the work remaining for future investigators of the Labrador Coast will be mainly confined to filling in the details of Dr. Packard's masterly sketch. The interior of the country, however, still offers a wide field of investigation to such hardy explorers as care to brave an inclement climate, a rugged country, and the attacks of the swarms of black-flies and mosquitoes which made life at times so unbearable to Dr. Packard's party.

It speaks well for the accuracy of the author's observations that a large part of the work relates to cruises made on the coast as far back as the years 1860 and 1864, and that his descriptions of the geological and zoological results of these two cruises are reprinted, with but little addition and alteration, from the Memoirs of the Boston Natural History Society for 1867. Other portions of

the work, including the account of the author's first cruise to Southern Labrador in 1860, and the first chapter of the voyage to the northern part of the country in 1864, have also seen the light in various serials; but it appears that the other four chapters descriptive of the latter voyage, as well as one on Recent Explorations, are now published for the first time.

The earlier portion of the work is devoted to a description of the physical geography of the country, and to an investigation into the history of its discovery, from which we learn that Labrador was known to the Norsemen "nearly five centuries before John Cabot made his first landfall of the American Continent." The chapters descriptive of the author's two journeys to Labrador contain much entertaining reading, and give a good idea of the general characters and productions of the country. Many interesting observations are recorded on the sea-birds of the coast, but we wish that the author was not quite so fond of calling one and the same bird by different names, since this is sometimes rather puzzling to his readers, as, for instance, on page 103. Then, again, although we are well aware that the puffin is frequently known as the "sea-parrot," we must object to its appearance under the name of "perroquet," without any qualifying prefix (p. 66). The vast diminution in the number of the gannets referred to on page 98 shows how urgent is the need of protection for sea-birds, even in such a remote region as Labrador; this necessity being even more apparent in the case of the eider-duck, which was threatened with extermination until the forays of the "eggers" were stopped.

While the zoology and botany are fully treated, the ethnology and geology of the country receive full attention from the author, and the experiences during his own voyages are well supplemented by a short chapter on recent explorations. Ample lists of the fauna and flora of the coast are given in the concluding chapters of the work before us, which will probably long remain the standard authority on all matters connected with the "Land of Labour."

PHASES OF ANIMAL LIFE, PAST AND PRESENT. By R. Lydekker. 12mo. Pp. 248. London: Longmans, Green & Co., 1892. Price 6s.

THIS little volume, which is the first of the "Knowledge" series, is a reprint, with some additions, of articles originally appearing in our contemporary *Knowledge*. Needless to say, the whole of the subjects are treated in a popular manner, as the work is intended solely for those having an interest in natural history, but not professed students thereof. There are many somewhat similar works devoted only to the animals of the present epoch, but the especial claim to attention of the one before us is that it endeavours to illustrate, in a popular manner, some of the leading features of many extinct groups of vertebrates. It is, indeed, almost exclusively with vertebrates that the work deals, but it must by no means be inferred that fossil forms alone are referred to. There is a chapter on Tortoises and Turtles, and there are others on Marsupials and Monotremes, all of which deal mainly with living types. Some of the chapters treat solely of the structural features of various groups, living or extinct, while others are designed to show how different groups of animals are modified for similar modes of existence; and others, again, show the modifications which particular organs, like teeth, have undergone in different orders of animals. The volume is illustrated by nearly

eighty figures, of which the great majority have previously appeared in other publications. A striking feature in the work is the small extent to which technical terms are employed, even when structures of considerable complexity are treated; and this absence, together with the general interest of the subjects, ought to ensure for it an extensive circulation among that section of the public for whom it is primarily intended.

Table and Market Poultry versus Fancy Fowls. By W. B. Tegetmeier. 8vo. Pp. 111. London: Horace Cox, *Field* Office, 1892.

In the little work which bears the above title, written though it is with a pre-eminently practical object, Mr. Tegetmeier has made a most interesting contribution to Zoology. During the time that has elapsed since he published his well-known work on Poultry, the fanciers have not stood still; but the result of their labours in selective breeding has been, as Mr. Tegetmeier conclusively proves, a continuous degradation of the breeds they have taken in hand, regarded as profitable stock.

This is very strongly insisted upon in the introductory chapter of the work, and attention is drawn to the similarly bad effects of competitive shows on the practical qualities of other animals, such as dogs and pigs. To show what the farm-yard fowl was before corrupted by admixture with various artificial breeds, we are presented with a reproduction of Bewick's figure, representing "a short-legged, strong-winged, full-chested bird of great value as a table-fowl—one that is in strong contrast to the coarse-boned, narrow-chested, feather-legged mongrels" that have been the result of crossing with the Cochin and Brahma.

The next nine chapters deal with the several breeds separately or in groups, the game fowl, in which the change has been especially conspicuous, being taken first. This unfortunate variety has been, in the pursuit of what is known as "reachiness," elongated out of all knowledge, so that it reminds one of a wading bird, hens having sometimes the keel of the breast-bone raised, when in the standing position, 12 inches off the ground. The practice of cock-fighting, however morally reprehensible, certainly tended to the production, by survival of the fittest, of compact, vigorous, and meaty birds, superior in every way to their present representatives; and, to show the contrast, the frontispiece gives portraits of a young game-cock of the modern type, and one of the old fighting strain, represented as trimmed and "heeled" for the pit, its more full-bodied character being thus rendered apparent. It may be of interest to note here that a recent writer on the old fighting game, Mr. Fletcher Moss, states that in the examination of twenty-nine fighting specimens he found that the general plumage was either the black-red (the colour of *Gallus bankiva*) or some approximation to it. As the "cockers" cared nothing for colour, this must mean either that the absence of selection for this point allowed of reversion, or that the natural coloration was correlated with a stronger constitution and a higher courage, though it must be noted that in the celebrated Lord Derby strain the legs were white, unlike those of the wild bird.

Dorkings are next treated—a breed whose original usefulness as table-fowl has been sadly impaired by the encouragement of coarse-boned specimens, the defect having originated through a cross with the Malay, resorted to in order to gain increase of size.

There is an interesting statement by Mr. John Baily to the effect that white fowls have a tendency to yellowness in the fat. It may be added that fowls of this hue are apt to be tanned or discoloured by the sun.

It is gratifying to notice a protest against the fifth toe, an additional hallux, so characteristic of this breed. Easily perpetuated, it is "an unquestionable deformity," and connected with disease of the foot.

With regard to Cochins, a breed which Mr. Tegetmeier is inclined to think might have had an independent origin, they are stated to have given the first strong impulse to the now prevalent pursuit of poultry-keeping. A characteristic illustration allows us to judge of the alteration wrought in the breed in the few decades which have elapsed since its first exhibition in 1850; and progress is easily seen to have been mainly in the direction of the production of a mass of useless feather, especially on the shanks, where it is most objectionable. On the other hand, the main economic fault of the breed, its deficiency of flesh on the breast, correlated with the extremely weak powers of flight, has not been corrected. It is very properly pointed out that the extra feathering is merely so much food wasted.

Brahmas, which for practical purposes are stated to be identical with Cochins, are likewise portrayed as they were and in the "improved" form of to-day. Mr. Tegetmeier regards these as a manufactured breed. Mr. Lewis Wright has mentioned several interesting points wherein they tend to resemble *Gallus furcatus*, though not assigning them a direct descent from that species; the single median wattle having been found in at least one instance, and the non-serrated comb, and greenish gloss on the plumage, having also occurred in this variety. But such resemblances as these may be doubtless set down to analogical variation; just as, in the domestic duck, about whose descent from the mallard there is no doubt, the white on the tertiaries, characteristic of the Spotted Bill (*Anas poecilorhyncha*) may be occasionally observed.

It is curious that with Brahmas, as with several other varieties, it is necessary to maintain two distinct breeding studs for show birds, since equally perfect specimens of both sexes cannot be bred by mating a cock and hen which satisfy exhibition requirements. This, of course, does not increase the usefulness of the varieties in question.

Other Asiatic breeds of more recent introduction are next dealt with. The Langstan, at first hardly differentiated from the Black Cochin, has been bred larger-winged and closer-feathered than that bird, besides having less feathering on the legs; yet, as a table fowl, it appears not to be of high quality.

Plymouth Rocks are a good example of a new breed, produced by crossing older varieties, a practice which the present system tends to encourage. A new variety, it appears, can be bred tolerably true to form and colour in three or four generations, and is then likely to become the rage if sufficiently advertised. This has been the case with the recently-invented Wyandotte breed, whose distinct markings, unfortunately, seem to be leading its adherents away from the cultivation of its useful qualities. This, like most of the leading breeds, has its club of admirers.

Under the head of Malays, Indian game, and Aseels, are included breeds of Eastern origin, characterised by very scanty combs and

wattles, close plumage, and often a very savage disposition. The Malay is too long in the leg to be of much practical value, but the nearly allied breed, miscalled Indian game, is an excellent table-fowl. The true fighting-cocks of India belong to the Aseel breed, the chief drawback to which is its extreme pugnacity. The young cocks, we are told, "will fight while little chickens, and when they can no longer stand will lie upon the ground and peck at one another." The small combs of these very pugilistic birds do not afford much support to Eimer's view, that frequent stimulation by seizure in fighting has led to the increased size of the comb in domestic poultry as compared with the wild birds.

Even the French table-breeds, which come next in order, have not escaped deterioration; the modern Houdan, with its head like a feather mop, presenting a woeful contrast to the moderately crested bird at first imported. These first importations, too, were often without the additional toe; but this useless deformity has now been established as a feature of the breed. In this chapter, too, attention is drawn to the curious fact that "large combs, although they show that the birds possessing them are good layers, also indicate that they are not readily fattened"; though in the case of breeds where a large comb is a show-point, it would not be safe to select the largest-combed hens as layers. This is the case with the various breeds of Mediterranean origin, to which a chapter is devoted. The best known is the Spanish, whose chief point, for many years, has been a perfectly white face and ear-lobe. This latter appendage has been so developed by cultivation as to become 3 or 4 inches in length, and in pursuit of this and similar excellences the breed has been rendered extremely delicate and infertile; although a writer in the *Stockkeeper* maintains that Mr. Tegetmeier has overstated the case against the modern specimens of this formerly profitable breed, saying that the prize hen alluded to as barren had really laid one egg, while, as to hardihood, a high-class cock, kept in an open shed during the winter, had only lost part of his comb and the ends of his toes. But even such prolificacy and hardihood as this can hardly be considered sufficient for practical purposes. These Mediterranean varieties do not sit, and this absence of the instinct for incubation, so advantageous for egg-production, is also characteristic of the Hamburgs and Polish, treated, with other varieties, in Chapter X. The former of these, though very beautiful, are, like the latter, whose heavy crests have always been a drawback to their usefulness, not to be recommended for practical purposes.

In the excellent chapters on the practical management of fowls, we are glad to see that the notion of crooked breasts arising from the use of small perches is contradicted. There is a too prevalent idea among bird-keepers that round perches are objectionable, even for passerine birds, whose perfectly constructed grasping feet are so eminently suited for holding a round support. With regard to hatching, it is stated that eggs with any irregularity of shell should invariably be rejected, as they will not hatch. In France, chickens are largely hatched by turkey-hens, and it might have been mentioned that the cock-turkey will also undertake this task.

With regard to ducks, the Aylesbury is the breed recommended. That neglected species, the Muscovy (*Cairina moschata*), is not mentioned, though its hybrids with the common breeds must possess some value, since a very practical breeder advertises eggs from the Aylesbury, mated with this bird, for sale, and it is not in repute as a

fancy bird. We hear nothing, too, of a breed of ducks which has recently come into notice under the mysterious name of Indian Runners, which are said to be gifted with extraordinary laying powers. As to geese, attention is drawn to the fact that they maintain their productiveness for an unusually long period, up to even twenty years; and with turkeys it is noteworthy that much additional vigour has been gained by an admixture of American blood, the American breed having been largely crossed with the northern race, *Meleagris gallo-pavo*, so that, supposing the domestic birds to have arisen from the southern form, *mexicana*, we now have two subspecies, at least, united in some of our tame turkeys.

In the chapter on diseases, an interesting account, with an illustration of the bacillus, is given of a disease which proved very fatal on a poultry farm in Kent in 1888. The folly and impracticability of these ventures is fully pointed out in the succeeding chapter, but with this we have not at present to do.

Far too little attention is usually paid by naturalists to the progress of the domestication and breeding of various animals, and deplorable as they are from the practical and æsthetic points of view, there is no gainsaying that the too often monstrous productions known as fancy breeds possess a genuine scientific interest. A debt of gratitude is undoubtedly owing to Mr. Tegetmeier for having so vividly detailed in this work the results of the last 40 years' breeding on the plastic organisation of *Gallus bankiva*.

FRANK FINN.

THE WORLD-WIDE ATLAS OF MODERN GEOGRAPHY, POLITICAL AND PHYSICAL, containing 112 plates. With an Introduction by J. Scott Keltie. Edinburgh and London: W. and A. K. Johnston, 1892. Price 7s. 6d.

THE "World-Wide Atlas," considering the exceedingly low price at which it is published, is a remarkable production, containing in a very small space as much information as can be obtained from many larger and more expensive atlases.

The introduction by Mr. Scott Keltie gives a concise epitome of the present state of our knowledge of the geography of the different regions of the globe, accompanied by notices of the history of the gradual acquisition of this knowledge and the names of the chief explorers.

The maps, which measure $10\frac{1}{2}$ by $7\frac{1}{2}$ inches, are, for clearness and excellency of engraving, admirable examples of the chartographer's art, and appear thoroughly up to date. Not only are the various continents mapped geographically, but a replica of each of the continental maps is given, with the physical features clearly indicated by distinctive colouring so that the general contour can be comprehended at a glance. While the larger countries have each a separate map of their whole area, other maps on a larger scale illustrate in greater detail their provinces, and the Atlas can thus vie with many of its larger rivals.

While not too detailed for the young scholar, the "World-Wide Atlas" will be found sufficiently comprehensive to serve the purpose of many who require the aid of good maps for their special studies.

SOME NEW MEMOIRS.

On the Photographic Analysis of Plant Movements. By Dewèvre and E. Bordage. *Revue générale de Botanique*, February 15, 1892.

Photography has already been applied with so much success to the study of animal locomotion, that MM. Dewèvre and Bordage have lately been adopting this method for determining the movements of plants. To the top of the part to be studied, a minute thread of blackened glass, ending in a bright head, is attached by a drop of glue. The movement of the bright head is recorded on a sensitised plate contained in a photographic camera. By means of two cameras placed one above, and the other by the side of, the plant, a record of the movement in a horizontal and a vertical plane is obtained at the same time. The plant may be placed in a blackened box, and the tubes of the cameras inserted in holes in the sides, while the glass head is illuminated by light entering at a small hole in one side; but as continued darkness is often disadvantageous and sometimes not allowable from the nature of the experiment, the authors find it sufficient if the mouths of the cameras are directed towards internally-blackened tubes, placed beyond the part the movement of which is to be registered.

Experimenting with the Convolvulus, they have been able to certify that this climber exhibits true heliotropism, a fact denied by several authors, though Darwin thought it existed, but in a very slight degree.

They have also studied the action of light of different colours on the heliotropic movement, *e.g.*, in the case of *Lepidium sativum*, which is not a climber, they found yellow light to have no action. The curvature of the stem in response to illumination began in the green, increased towards the blue and violet, and reached a maximum at the limit of the violet and ultra-violet. It then slowly decreased in the ultra-violet region, but was still feebly manifested even in that part of the spectrum where silver-salts cease to be blackened and fluorescent substances to shine, and only stopped entirely a little further on, at a distance equal to twice the length of the luminous band obtained with a quartz prism.

An attempt was also made to analyse the normal movements of circumnutation described by a root pushing through the soil. As the presence of an indicator, however small, attached to the sensitive tip of a root causes a Darwinian curvature in the opposite direction, another method had to be employed. The whole root was covered with a thin layer of lamp-black, except the tip, at the level of the region of growth. This part remaining white, affected the sensitive plate, provided sufficient light was received. By this arrangement it was determined that the root, thus left quite at liberty, described a very regular circumnutatory movement, with less oscillation than the tip of the stem.

The sleeping and waking movements of leaves of several leguminous plants were also studied. The leaf observed was usually covered with lamp-black, and then a little flour put on the apex, which was thus able to register the movements. To obtain separate records of the repeated upward and downward movements, it was also necessary to place the plant on a slowly revolving plane. By this means the authors find that the apex of the moving leaf describes a series of very regular oscillations. It arrives, by a series of downward and upward movements, at the state of repose on which it enters by a rapid succession of slight tremulous movements. There are always, between the state of repose and the normal, two distinct periods, which the authors term phases of *oscillation* and *tremulation* respectively.

EXPERIMENTAL RESEARCHES ON THE POSSIBLE ACTION OF GAS AT HIGH TEMPERATURES, ACCOMPANIED BY VERY GREAT PRESSURE AND VERY RAPID MOVEMENT, IN VARIOUS GEOLOGICAL PHENOMENA. By A. Daubrée. *Bull. Soc. géol. France* [3], vol. xix., p. 313 (1891).

M. DAUBRÉE, the well-known author of *Géologie Expérimentale*, has turned his attention to the geological action of gas at high temperatures and pressures.

Charges of dynamite and gun-cotton were exploded in a small chamber, the orifice of which was closed by cylinders of rocks of various kinds, such as granite, limestone, basalt, trachyte, and the Pultusk meteorite. The pressures realised in the experiments varied from 1,100 to 2,300 atmospheres, and the temperatures were estimated to range from 2,500° C. to 3,200° C. It was found that rocks of various kinds could be readily perforated, and that the channels by which the gas escaped were striated. Vertical pipes or "necks," such as those in which the diamonds of South Africa occur, are believed to have been formed in this way. The striations on the sides of these pipes, and the absence of contact metamorphism in the surrounding rocks, are easily explained on this hypothesis.

The nature of the powder produced by the pulverisation of the rocks was examined by placing a piece of cardboard coated with vaseline some distance above the orifice. The larger fragments perforated the cardboard, but the minute particles were retained by the vaseline. They were found to consist of angular fragments of the constituents of the rocks, quartz, felspar and mica in the case of granite, and small opaque, or nearly opaque, globules bearing a striking resemblance, at any rate so far as form and size are concerned, to the so-called cosmic spherules of the deep-sea deposits.[1] The author does not deny that the passage of meteorites through the air must be accompanied by the formation of such globules as those described by Messrs. Murray and Renard, but he points out that terrestrial agencies may also be concerned in their production.

One of the most striking results of the experiments is the proof that, in the extremely minute fraction of a second, during which active change is going on, rocks may be moulded to the form of the surface against which they are pressed. This moulding is so perfect that the striæ left by the tool used in turning out the cavity in the mass of steel are reproduced on the rock. Microscopic examination shows that this plasticity is due to the fracture and re-cementation of

[1] See NATURAL SCIENCE, No. 1, p. 21.

the constituents. If any doubt still remained as to the possibility of the most rigid rocks becoming plastic under earth-stresses, it would be removed by these remarkable experiments.

In the concluding part of the paper, the author discusses the bearing of his experiments on the ejection of masses of rock, molten or otherwise, from the interior of the earth through the channels of communication opened by explosive action. He points out that volcanoes are distributed so that in one and the same district there is a general uniformity in the heights of the principal vents. The elevation characteristic of any particular group is, according to him, a measure of the intensity of the gaseous pressure capable of being developed in the subterranean regions, from which the volcanoes of that group derive their energy.

Contributions to the Knowledge of the Liparites of Iceland. By Helge Bäckström. *Geol. Fören. i Stockholm Förhandl.*, vol. xiii., pp. 637–682 (1891).

This is a petrographical description of the acid igneous rocks of Iceland. It is of interest to British geologists because it confirms and extends the conclusion, arrived at by previous observers, that there is a close similarity between the rocks of Iceland and those of the North-east of Ireland and West of Scotland.

Acid igneous rocks are now known from many parts of Iceland. They occur as lava streams of very recent date, and also as dykes, stocks, and sills, intrusive in the basalts. The principal types are classed as liparite, obsidian, pumice, and granophyre. The most striking chemical fact is the very general predominance of soda over potash. Mineralogically, the liparites are composed of felspar, pyroxene, iron-ore, zircon, and glass. Quartz, tridymite, apatite, and olivine have also been observed; and, in a few cases, hornblende, biotite, hypersthene, and titanite. The porphyritic constituents are principally striated felspar and pyroxene. The silica percentage ranges from 63 to 79. In the frequent occurrence of striated felspar and pyroxene among the porphyritic constituents, and in the relative proportions of the alkalies, there is a resemblance between the Icelandic liparites and the pitchstones and felsites of Eigg and Arran.

The granophyres also furnish a most interesting point of resemblance between the British and Icelandic areas. These occur as intrusive rocks in the basalts of both areas. They were discovered *in situ* by Thoroddsen on the north coast of the Snäffels peninsula in West Iceland, and by Helland in three localities in South-east Iceland. In the former locality, plagioclase is the oldest constituent of the rock. Each grain is surrounded by a narrow zone of orthoclase, which in its turn is surrounded by micropegmatite. Ferromagnesian constituents are very rare. They are represented by biotite in this rock; but in a similar rock known only in ejected blocks, and formerly supposed to be a definite mineral (krablite), a green augite occurs.

It seems impossible to avoid the conclusion that the volcanic rocks of Iceland and the West of Scotland have been derived from the same magma-basin, and that the sequence has been the same in both cases.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We have received the prospectus of "A Summer School of Art and Science," detailing the arrangements for the sixth annual meeting of the "Edinburgh Vacation Courses." The lectures and excursions, which are to be conducted by Professor Geddes, Mr. Arthur Thomson, Professor Mavor, and others, extend from August 1 to August 31. Among those who will lecture upon Biological subjects are, besides the above-mentioned gentlemen, Professor Lloyd Morgan, Professor Haddon, Dr. Haycraft, and possibly Professor Haeckel and M. de Varigny. These courses of lectures correspond to those held at Oxford and Cambridge by University Extension Societies; and as the meeting of the British Association will take place at Edinburgh during the same month, it is probable that a good many persons will be induced to extend their visit so as to attend the entire series. A modest fee of three guineas covers the lecturers' fees, and arrangements are made to board intending students at a moderate cost. Applications should be made to J. Arthur Thomson, Esq., University Hall, Edinburgh.

Dr. D. H. Scott shortly resigns his position as Assistant-Professor of Botany at the Royal College of Science, having undertaken the charge of the Jodrell Laboratory at Kew. Some good work in anatomy or morphology will doubtless result from this release from the trammels of teaching.

The name of M. Vasseur is placed first on the list of candidates nominated by the Academy of Sciences for the vacant Professorship of Geology at the Paris Museum of Natural History.

We hear that Professor H. S. Williams, of Cornell University, N.Y., has accepted a Professorship at Yale, where he acted for several years as assistant to Professor Marsh.

Dr. Heincke, a well-known authority on fishery matters, has been appointed director of the newly-established Imperial German Zoological Station at Heligoland.

Dr. Hugh Robert Mill has been appointed Librarian to the Royal Geographical Society, in succession to Mr. Scott Keltie, who is now Assistant-Secretary. Dr. Mill has made important contributions to Physical Geography.

THE Royal Scottish Geographical Society has established a London branch at No. 20 Hanover Square, W. At the inaugural meeting last month, the Marquis of Lothian presided, and Professor James Bryce delivered an address on "The Migrations of the Races of Men considered historically." The three primary disturbing causes were described as food, war, and labour, which had in their turn led to the transference, diffusion, and permanence of types. There had been a constant tendency towards reduction of the number of varieties of men. Assimilation had been going on in every part of the world, so that to-day along the shores of the Mediterranean there were but nine different languages spoken, whereas in the time of Herodotus there were probably over thirty-seven distinct tongues. At its London meetings the Scottish Society proposes to deal chiefly with commercial geography.

THE Manchester Geographical Society has arranged a course of lectures at the Owens College on "The Habitable World: its discovery, partition, and commerce." The lecturer is Mr. H. Yule Oldham, and the opening meeting was held on April 4, under the presidency of Professor Boyd Dawkins.

THE combination of the Irish Natural History Societies inaugurating the *Irish Naturalist* has been joined by the newly instituted Cork Naturalists' Field Club, of which Professor M. M. Hartog is President. The first number of the *Naturalist* appeared in April, and the journal promises to be a useful record of progress in Ireland. There are contributions by A. G. More, on recent additions to the list of Irish birds; by Professor T. Johnson, on the sea-weeds of the west coast; by R. J. Ussher, on the crossbill in Ireland; by Professor Grenville Cole, on the Geology of County Dublin; and by Rev. W. F. Johnson, on the Coleoptera of Armagh. A few short notes and valuable reports of recent meetings of the Irish Societies conclude the number.

AT the Annual Meeting of the Yorkshire Naturalists' Union, held at Scarborough, the retiring President, Professor A. H. Green, F.R.S., read an address "On Some Moot Points in Geological Speculation." Mr. Charles P. Hobkirk, F.L.S., was elected President, and Committees of Research were appointed to consider the matter of local Boulders, Marine Zoology, Fossil Flora, Coast Erosion, Micro-Zoology and Micro-Botany, Disappearance of Native Plants, Geological Photographs, and Protection of Wild Birds' Eggs. Sectional officers were also appointed: in Vertebrate Zoology, Wm. Eagle Clarke, F.L.S.; Conchology, John W. Taylor, F.L.S.; Entomology, Geo. T. Porritt, F.L.S.; Botany, William West, F.L.S.; and Geology, J. W. Davis, F.G.S.

THE twenty-third Annual Meeting of the Norfolk and Norwich Naturalists' Society was held at the Norwich Museum on March 29. At the conclusion of his Presidential Address, Dr. Wheeler treated of the gradual extinction of many of the species of Lepidoptera known to have once inhabited the fenland. The species actually extinct he considered to be four in number—the Great Copper butterfly (*Polyommatus dispar*), the Gipsy moth (*Liparis dispar*), and two others little known save to entomologists, *Noctua subrosea* and *Phibalapteryx polygrammata*. After a few notes on the history and habits of these insects, he gave some account of his own experience of the Whittlesea Ermine (*Orgyia caenosa*), the Fen Leopard (*Macrogaster arundinis*), and other fen insects, which still linger in Wicken fen, but whose extinction may be looked upon as not far distant. Considering the causes of the

destruction of these insects, he thought that the direct action of man might in most cases be wholly disregarded. Indirectly, by draining the fens, man was no doubt responsible for the extinction of many of their peculiar denizens, but even this did not account for all, since some insects disappeared or became very rare without any striking change in the locality they inhabited. He considered that such cases were generally due to climatic causes, the insects being possibly on the extreme limit of their geographical area. In some cases the gradual drying of the fen might, by affecting the food-plant, prove fatal in the end to the larvæ feeding on it. Mr. Horace B. Woodward, of the Geological Survey, was elected President of the Society for the ensuing year.

We have received "The Caradoc Record of Bare Facts: a Systematic List of the more noteworthy Observations made by the Members of the Caradoc Field Club and Others, during the year 1891." The principal object of the publication is to collect and systematically arrange a series of definitely ascertained facts in the Natural History of the Caradoc District, for future reference. The services of several specialists have been enlisted, and the record is placed under their superintendence. Mr. W. Phillips is the referee for Botany, Mr. W. E. Beckwith for Ornithology, Mr. E. S. Cobbold for Entomology, and Rev. J. D. La Touche for Geology. Brief hints are given to observers, and the results for 1891 occupy eleven pages of tables. The authority for each identification is stated, and the names of Messrs. Harting, Howard Saunders, Etheridge, Lapworth, and Callaway appear among others. Such records of occurrences are of great interest and value, and we wish the Club success in its new undertaking. At the same time, we cannot refrain from urging again our contention of last month, that records of this kind ought not to be the ultimate aim of the Field Clubs. While the indexing is being done, there are ample opportunities for obtaining items of immediate importance in the progress of the philosophy of Biology and Geology; and we hope the Caradoc Club will be able to pay more attention to this aspect of its work in future Records.

In reference to our remarks last month on the Palæontographical Society's announcements, a correspondent writes as follows:—"Your comments on the Society's list of memoirs promised are opportune, inasmuch as the Annual Meeting is approaching. The list of memoirs in preparation is better than it used to be, but there are still ridiculous entries. The Society first 'promised' in 1862, and one of these early promises, the 'Graptolites,' by Wyville Thomson, was repeated until 1881. The promise in 1865 of 'Cretaceous Mollusca,' by Wiltshire and S. P. Woodward, was no doubt cancelled by the death of the latter in 1865, but the work was still kept in the list until 1891. The 'Purbeck and Rhætic Mollusca,' by Etheridge, were first promised in 1866, and still remain, though it must be as apparent to the executive as to the outsider that they might as well leave the list. The same may be said of 'Fossil Cycadeæ,' by Carruthers (1877), or even of 'Silurian Fishes,' by Harley (1883). The first part of the 'Crag Foraminifera' appeared in 1866, and the completion has been regularly promised ever since; while the memoirs on the Cretaceous and Liassic Foraminifera were 'in preparation' for at least 20 years until they finally dropped out of the list."

The *Jahrbuch der Hamburgischen Wissenschaftlichen Anstalten* for 1891 (vol. ix.) contains several contributions to the Zoology of Eastern Africa, largely based on the collections of Dr. Fr. Stuhlmann. Earthworms are described by W. Michaelsen; Termites, Odonata, and Neuroptera by A. Gerstäcker; and Mammals by T. Noack. The spiders of Madagascar and Nossibé are treated by H. Lenz; and there are some interesting notes on the tropical nut-bearing plants of Eastern Africa by R. Sadebeck.

There is also a noteworthy well-illustrated memoir on Throwing Weapons, by C. W. Lüders.

THE Italian Botanical Society invites the botanists of every nationality to an International Botanical Congress, to be held in Genoa from September 4th to 11th of the present year. Judging from the circular, this will form part of a great International Congress of "the Societies of Geography and of Natural Science," by which Genoa is solemnising "the fourth centenary of the greatest geographical discovery of all time—the work of her renowned citizen, the immortal Christopher Columbus." It will also be the occasion of the inauguration of the new Botanical Institute, which Mr. Thomas Hanbury has built and presented to the University of the town. Italian botanists cordially invite their colleagues of every nation to give the Congress an essentially cosmopolitan character, "for above all things it aims at strengthening, by the powerful influence of Science, the bonds of fraternity between nations." All enquiries and communications concerning the Botanical Congress should be addressed to Professor O. Penzig, Royal University, Genoa.

AFTER many failures, the horticulturists of Kew have succeeded in growing a young "double cocoa-nut palm" (*Lodoicea seychellarum*), which was placed for exhibition in the Victoria House last month. The plant is a native of the Seychelles, and very rarely seen in cultivation. The germination of the double cocoa-nut occupies nearly two years, and its attainment to maturity is very slow. Its peculiar mode of germination is a source of great difficulty in cultivation. The radicle grows down from the large heavy seed in the form of a stout tap-root, carrying with it the stem-bud or plumule enclosed in the sheath of the cotyledon. By the ultimate splitting of the latter the plumule is set free and able to ascend. If this long and slowly-growing process be injured success cannot be expected. The trunk of the adult tree may reach 100 feet in height, though scarcely a foot in diameter. The male and female flowers are borne on separate individuals. The immense fruits average 40 lbs. in weight; they contain, within a thick fibrous husk, one, two, or sometimes three large nuts, with hard and thick black shells, each divided about half-way down into two lobes. Before the discovery of the Seychelles islands, in 1743, considerable mystery attached to these nuts, which were often found floating in the Indian Ocean (hence the name "Coco de Mer"), and highly prized by the natives of the Archipelago. Rumphius, in his Herbarium Amboinense (1750), speaks of the nut as "hujus miri miraculi naturæ, quod princeps est omnium marinarum rerum, quae rarae habentur." It is not, he says, a terrestrial fruit which has happened to fall into the sea and thus become petrified, as Garcias ab Orta would persuade us, but a fruit actually growing in the sea, the tree being hidden from the human eye. He mentions some curious fables in connection with it, and says there are many more not worth the telling.

THE new part of the *Transactions of the Manchester Geological Society* contains Professor Boyd Dawkins' interesting paper on the "Further Discovery of Coal at Dover," to which we alluded last month. The Professor begins by expressing astonishment at the incredulity with which the discovery has hitherto been received in many scientific circles; but considering that he had never exhibited to any learned society the specimens on which his assertions were based, until the recent meeting in Manchester, we think there was good reason to suspend judgment.

FOR the first time since 1836 the Zoological Society's Gardens in London have no specimens of Giraffe. Owing to the closing of the Soudan none have been

imported into Europe for some time, and Dr. Sclater appeals to those who are entering Africa on the East Coast to help him to obtain some new specimens of this interesting mammal.

THIS year's meeting of the Museums' Association will be held at the Manchester Museum, under the Presidency of Professor Boyd Dawkins, F.R.S. The Address will be delivered on Tuesday, July 5; and the two following mornings will be devoted to the reading and discussion of papers.

WE regret to have to announce the retirement of the veteran Swedish zoologist, Professor Sven Lovén, Först Intendent of the Riks Museum, Stockholm. After a period of 51 years' service, the Professor is unfortunately compelled, by failing health, to sever his official connection with the Museum; but zoologists will be glad to learn that he is still able to continue scientific researches. At the present time, we understand, Professor Lovén has in the Press a memoir on the Cystidea, while he is also occupied with an investigation of the young stages of *Salenia*. The new keeper of the Zoological Collections at Stockholm will probably be Professor Theel, another distinguished authority on the Echinodermata.

OBITUARY.

SERENO WATSON.

BORN DECEMBER 1, 1826—DIED MARCH 9, 1892.

IN the death of Sereno Watson, Curator of the Herbarium of Harvard University, American Systematic Botany has sustained its greatest loss since that of Asa Gray. Mr. Watson was born at East Windsor Hill, Connecticut, December 1, 1826, and graduated from Yale College in 1847. For twenty years after graduation, he was engaged chiefly in teaching and in literary work. During this time he also took up the study of medicine at the University of New York. Although he had early manifested an interest in plants, and as a teacher had made some study of them, it was not until 1867 that he became actively engaged in botanical pursuits. In July of this year he joined as a volunteer the United States Geological Expedition under Clarence King, to explore the region of the fortieth parallel of latitude in Western America. For the first year Mr. Watson's work was largely topographical, but at the close of the year he took charge of the Botanical division. The material collected in the new territory was worked up in the Herbaria of Professors D. C. Eaton and Asa Gray; and in 1871, with Professor Eaton, Mr. Watson published the 5th volume of King's *Report of the Geological Exploration of the Fortieth Parallel*. It was at this time that Professor Gray invited Mr. Watson to become his assistant; and since then his life has been devoted to a critical study of the North American Flora, and to the care and development of the Gray Herbarium and Botanical Library of Harvard University. In appreciation of the high merit of his botanical work, the honorary degree of Doctor of Philosophy was conferred upon him by the University of Iowa, in which he had formerly been an instructor.

When it is considered that Dr. Watson's work in Systematic Botany began only about twenty-five years before his death, the number of his publications is wonderful. He has described over one thousand species of phaenogams, mostly from the Western United States and Mexico; and nearly all have stood the test of time and further exploration. Most of these species were published in a series of eighteen *Contributions to American Botany* in the *Proceedings of the American Academy of Arts and Science*, in which he has also elaborated many orders and genera, important among which are the

Liliaceæ, Chenopodiaceæ, *Lupinus*, *Potentilla*, and *Eriogonum*. His more extensive single works are *The Botany of California*, published in connection with Professor W. H. Brewer; and the *Bibliographical Index to North American Botany*, a most valuable work, covering the Polypetalous orders.

After the death of Thomas P. James, and during the feeble health of Lesquereux, Dr. Watson carried out successfully the editing of their work on North American Mosses; and two years ago, with Professor John M. Coulter, he revised Gray's *Manual of the Botany of the United States*, which had not been brought to date since 1867. By far the most important work Dr. Watson had undertaken was the completion of the *Synoptical Flora of North America*, begun by Dr. Gray. While he has not been spared to go far with this flora, yet many of his contributions will be of the greatest assistance in future work which may be done on the parts yet unpublished.

In 1885 Dr. Watson was in Guatemala, where he contracted the malarial fever of that climate, and since that time his constitution had not gained its former strength.

About the middle of December last he was attacked by the prevailing influenza, which later affected the heart, and on March 9 he died at his home in Cambridge, at the age of sixty-five years.

Dr. Watson was a man of high moral character as well as of scholarly attainments. He was of a retiring disposition, preferring the quiet of his study to social pleasures. His ability for continued application was remarkable, yet he was always glad to stop his more important work to give a word of advice or assistance to those whose privilege it was to work under his direction.

MERRITT LYNDON FERNALD.

Cambridge, Massachusetts.

WILLIAM BOWMAN.

BORN JULY 20, 1816—DIED MARCH 29, 1892.

ONE of the most distinguished pioneers in the modern development of Histology has just passed away in the person of Sir William Bowman, Bart. Born at Nantwich in 1816, and having received his early education at Birmingham, Bowman entered King's College Hospital, London, in 1837, and two years afterwards became Demonstrator of Anatomy in the Medical School. In 1840 he commenced surgical practice, and during the next decade all his leisure was devoted to the investigation of the minute structure of the mucous membranes of the alimentary tract, muscular fibres, the kidneys, and other tissues at that time imperfectly known. The more important results of these researches were published by the Royal Society, of which Bowman became a Fellow in 1841, and

from which he received one of the Royal Medals in the following year. Other results were incorporated in Bowman and Todd's "Physiological Anatomy and Physiology of Man," and in Todd's "Cyclopædia of Anatomy and Physiology." For an estimate of the value of the work, and its fundamental importance, we may refer to an article by Dr. Burdon Sanderson in *Nature* of April 14. In 1846 Bowman was elected assistant surgeon to the Royal London Ophthalmic Hospital, and within the next decade he attained to the position of surgeon in King's College Hospital. By this time he had decided to devote himself exclusively to the diseases of the eye, and henceforth his early brilliant researches were unfortunately discontinued. Bowman's work in ophthalmic surgery was characterised by the same thoroughness that had been conspicuous throughout his histological investigations; and, in recognition of his professional eminence he was created a baronet in 1884. Apart, however, from his purely scientific and surgical work, Sir William Bowman will long be remembered as a generous supporter of many of the principal medical and scientific institutions of London, and the Royal Institution especially mourns the loss of one of its most valued officers.

OBSERVATIONS AND CORRESPONDENCE.

Spring-time in the Waters.

notes at the brighton aquarium.

The sudden outburst of animal life in the tanks of large marine aquaria is very remarkable at this season of the year. During the winter the environment is dark, dull, and void of life, and the fishes are languid and without lustre. A few warm sunny days pass and a great change is visible. It seems really as if there were a spring-time in the waters, for the rocks and sea-weeds now teem with myriad forms of life. Numberless specks of filmy substances adhere to the rocks, the thick glass fronts of the tanks, and the stones and sea-weeds placed in them.

The colours of the anemones brighten and deepen, and they appear to expand with greater freedom. That small, but beautiful species, the Daisy anemone (*Actinia bellis*), covers the tufa-rocks in thick profusion. The long, slender stems and swollen calices of the young forms now recall their budding namesakes of the meadows, and the more expanded crowns of the older, shorter, and thicker specimens resemble the full-blown flower. Delicate whorls of young serpulæ make their appearance, and the crimson-tipped plumes of innumerable *Sabellæ* wave from the mouths of their tubular dwellings, almost hidden in masses of flocculent confervæ, no longer of pallid hue but of a fresh green colour. Many beautiful young colonies of rufous, gray and purple compound ascidians of the cluster family (Botryllidæ) rapidly develop in their accustomed haunts. The violet species (*B. violaceus*) may be seen, as usual, growing on stones at the bottom of the tanks. One British species of this family (*Botrylloides fulgurale*) occurs at a depth of 530 fathoms between the Faröe Islands and the Scottish Coast.[1]

Sponges, like fungi, grow again and again in the same locality. For many seasons a sponge has appeared every spring time in a certain tank in the Aquarium. It develops and grows on the same rocky exposure year after year, splits into separate masses and falls away, leaving no apparent trace behind. When spring returns another sponge appears on that identical spot to go through similar processes of rapid growth, fission, and decay.

The first tank in the Brighton Aquarium, a very deep and light one, now presents a charming picture. The rocks at the sides are dotted, here and there, with filmy white juvenile Ascidia. Branches fixed to the floor are covered with compound forms. On one there is a purple star-like colony of *Botryllus*, and below that the young of a cream-coloured fluffy species of a solitary ascidian have settled down to pass their brief existence. On another, an encrusting compound form of the Didemnidæ may be recognised. Broad leaved

[1] W. A. Herdman, The Tunicata of the "Challenger Expedition," Part II., Ascideæ Compositæ, p. 53.

fucus of a golden brown colour and tangles of sea-weed rooted to the stones on which they grew formerly in the sea at "Black Rock" are coated here and there with a common species of moss-animals (*Flustra*) and cover the bottom of the tank. Among the broad leaves of fucus the graceful forms of the pipe fishes (*Syngnathus acus*) may be discerned on searching for them. In colour they so much resemble the weeds they frequent that it is difficult to see them, and it is not until we have watched one rise, with its central back fin revolving, like the screw of a steamer, and float slowly to the surface, that we can detect the presence of other specimens among the weeds. This is another instance of adaptive colouring subserving protective purposes, of which the soles, turbot and flounders—colourless as sand, or spotted as the shingle with which they cover respectively a great part of their flat bodies—afford more familiar examples.

Here also may be seen the male of the gleaming dark-banded "fifteen-spined stickleback" (*Gasterosteus spinachia*) which has the same propensity as its fresh-water relative, and binds together the fronds of weeds to form the nest in which the females of his kind deposit the lustrous blue vitreous ova he guards with so much pugnacity, driving away all approaching intruders. Now is the time for the males in general of the fish kind to put on their "wedding-garment" of bright colouring, like the birds of the air. The wrasse (*Labrus mixtus*), so aptly called the peacocks of the sea, are very resplendent.

Among the latest additions to the Brighton Aquarium may be chronicled the female sea-lion cub born in captivity on June 17th, 1891—the second event of the kind that has occurred during the history of the institution. In this instance, the mother had been acquired but a short time previously. This sea-lioness is an unusually fine and fully mature specimen of the Californian species, *Otaria californiana* (=*gillespii*). The night of her arrival she managed to clamber over a temporary wooden barrier raised in her enclosure, visited several tanks in succession, devouring the whiting and herrings kept in them, until her progress was arrested by a concrete party wall. She stands, when quite upright (as she has been well trained to do), with her hind limbs turned forward under her body, not less than five feet eight inches, and measures from the tip of her nose, with neck well extended, to the extremity of her hind flippers, when stretched backwards as in swimming, seven feet, and is probably one of the largest of her sex and species on record.

We are informed by Mr. W. Wells, Tank Superintendent at the Aquarium, that the cub measured eighteen inches at birth. We regret that it is impossible to furnish a portrait of the lively little animal. An early attempt was made to photograph, but owing to the extreme restlessness of the cub the "sitting" was a failure, and no proof could be developed. It was active, playful, and intelligent from the first, and took to the water six days after birth, whence its anxious parent occasionally removed it in her mouth, lifting it by the skin of the neck, as a cat does her kittens. The cub is now nine months old, and three feet six inches long, sleek and fat, and, when basking on the rocks, of a light mouse-colour. It is still nourished entirely by its most affectionate mother, who shows no sign of deserting it, and certainly never treated it with the parental indifference ascribed to the Pribyloff species *O. ursina* in the wild state, by Nordenskiöld in the "Voyage of the Vega." The parent of a male cub, born some years previously in the Aquarium, abandoned it after six months' care.

A young male sea-lion, possibly *Otaria stelleri*, a species found also off the coast of California, was purchased two years ago. It was about a year old at the time, docile, and very intelligent, and soon became a most finished performer. This animal, of a dark reddish-brown colour, with prominent eyes, has not yet acquired its full dentition. There are seven teeth in each upper jaw, but no signs of the prominent canines characteristic of the Patagonian sea-lion *Otaria jubata*.

The oldest captives are the sterlets (*Acipenser ruthenus*), first brought over from the Volga shortly after the Aquarium was opened nearly twenty years ago. Next in seniority is the Alligator Terrapin, from America (*Chelydra serpentina*) which has passed thirteen years of uneventful existence in congenial warm waters. Less than a foot long on arrival, it now measures between four and five feet from beak to tail, and its double ridged carapace is broad in proportion.

AGNES CRANE.

THE WRESTLER FROG OF LA PLATA.

MR. W. H. HUDSON, in his "Naturalist in La Plata," reviewed in the last number of this journal, relates his experience with a frog, which, instead of attempting to escape, sprang at and caught hold of the fingers of its pursuer. The frog was no doubt a breeding male of the common *Leptodactylus bullatus*. Our toad behaves just the same in early spring, and many males can be secured from the ponds, where they congregate for the purpose of breeding, by the simple method of holding out to them a walking stick which they will clasp in the manner witnessed by Mr. Hudson. But what is more extraordinary is to find our common frog (*Rana temporaria*) figured as the "Wrestler Frog" of La Plata, for no species of the genus *Rana*, nor any representative of the family Ranidæ, has yet been found in the Argentine Republic. As the specimen observed by Mr. Hudson was not preserved, the figure was probably executed at home from a British specimen. This reminds us of a similar want of accuracy in one of Gould's works, where an Australian bird is depicted in the act of seizing a newt.

G. A. BOULENGER.

EXALLAGE.

IN my article on Life-Zones in Lower Palæozoic Rocks, last month, I was in error in assigning a remark to Dr. Blanford concerning the desirability of a term for "time-varieties." Dr. Blanford has kindly called my attention to the account of a discussion in the Report of the International Geological Congress, held at Bologna in 1881 (Report, p. 168), in which the term "mutation" is suggested in the same sense as that in which the word "Exallage" was used in my article. I wish, therefore, to withdraw the term Exallage, though I still think the word "Exallagous" (not Exallogous) may be useful as an adjective.

J. E. MARR.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

The Organisation of Science.

PROBABLY the two features in our social organisation of which the average Englishman is proudest are the historical continuity in the development of our institutions and the minute division of labour which condemns thousands of our countrywomen to spend their lives tying knots in broken threads. Every now and then, however, some iconoclast makes the startling discovery that the two are inconsistent, and that a rigid acceptance of the commercial system would enable our social and scientific institutions to carry on their work with less unnecessary waste and fewer unsatisfied wants. A very weighty protest against the existing confusion has just been issued under the above title, by a writer who, we regret to say, hides himself behind the *nom de plume* of "a Free Lance."[1] This essay is suggestive and interesting, and is obviously written by a man who knows a good deal about the inner working of our scientific societies, and feels deeply the necessity for radical, or, rather, revolutionary reform. He maintains that the conditions of affairs have so much altered since the principal scientific societies were founded that at present their main use is as co-operative publishing companies; their libraries, and the opportunities they afford of social union, are (or ought to be) valuable secondary aids. Since he regards publishing as their main function, he has, of course, little difficulty in showing that the work is done extravagantly and badly: numerous societies and journals cover the same ground, and, hence, a person studying a particular subject is compelled to use many journals, whereas our

[1] The Organisation of Science. By a Free Lance. London: Williams and Norgate, May, 1892. Price 1s. net.

author maintains that one in each country ought to be sufficient. Thus, he says the Royal Society publishes biological papers which ought to go to the Linnean Society, with the result that the contribution is issued with a series on wholly different subjects; the younger biologists do not obtain the paper unless they can afford the heavy expense of buying the *Phil. Trans.*, while it is freely distributed to the Fellows of the other society, the majority of whom have no interest in the subject, and are unable to appreciate its importance. "This," says the Free Lance, "is worse than casting the children's bread to the dogs, for at least the dogs could digest it and benefit by it; we simply cast their bread away." The essayist maintains that we have the same confusion all round: several societies or journals are doing the work that one ought to do, and the resulting pecuniary waste and administrative inefficiency has grown to such an extent "that the chaotic rule of the Heptarchy was almost orderly compared with the state of things now existing in the domain of science."

To find the way out of this darkest scientific England the author proposes to proceed on the waste-not-want-not lines, and thus to rescue the younger scientists at present submerged beneath the deluge of innumerable journals. He would have the Royal Society cease the publication of memoirs that come within the scope of the specialist societies, and confine itself to the work of indexing and abstracting literature, attending to the business and educational work of science, and acting as Universal Bibliographer. Each other society should confine itself rigidly to its own sphere, and not encroach on that of its neighbour. The Zoological Society should give up its system "of flagrantly robbing the Linnean Society" by publishing zoological papers "of high scientific importance," and confine its attention to feeding the animals at the Zoological Gardens. The Microscopical Society should publish only on microscopes and microscopic appliances, while all the smaller provincial journals, and those not connected with any society, such as the *Quarterly Journal of Microscopical Science*, the *Annals of Botany*, &c., should promptly cease to exist. Against the unfortunate botanists who founded the last-mentioned journal the "Free Lance" runs his most furious tilt; he regards this as the mere "wanton extravagance of publication," and as "the most wanton and inexcusable instance of journalistic disorganisation that could be found." The local societies and their journals he proposes should be all affiliated to the London societies in a general federation, compared with which we fear the feat of the great Cape Amalgamator was but child's play. The idea, however, is a good one.

In all this there is, it must be admitted, a distinct measure of truth; and the author states the points with such vigour and incisiveness that he may be excused for exaggerating the evils of the case. Everyone who has seen much of the working of the English scientific societies has felt the same difficulties, while recognising also that as

they are the outcome of a century of progress they cannot be obviated in a day. The essayist's plan of one Science one Journal would also mean one Science one Editor, and the reign of a powerful but small centralised bureaucracy. The system of having papers examined by specialists before publication seems to be essential and to work well upon the whole, so long as there are several competing journals; but if there were only one, there might simply be a monopoly of publication in the hands of a small clique. No doubt the occasional publication of a biological memoir in the *Philosophical Transactions* is inconvenient to one who only takes in the *Journal of the Linnean Society*; but then so is the publication of a long telegram in the *Times* to one who trusts to a brief summary in the *Star*. That, however, is not so much an argument for the fusion of all the daily papers, as for improving our system of indexing periodical literature. If the Royal Society would do more of this in the future than it has done in the past, it would no doubt be well; but it seems to us it would be a misfortune to remove the stimulus of the issue of a journal in which, like the Belgian *Mémoires Couronnés*, a Naturalist may hope to publish his finest piece of work. In other departments there seems to be a tendency towards a better distribution of papers; palæontological papers that bear simply on systematic Zoology as apart from the problems of Geology are being contributed more to the Zoological, than to the Geological Society; while as the same society is receiving more of the Zoological work, the Linnean Society will in time probably find its true place as the Botanical Society of London.

By slow changes such as these more good is likely to be wrought than by any of the revolutionary methods advocated by our essayist. His proposal to abolish competition would retard progress and endanger liberty, and, like the late Archbishop McGee, we would rather have British science unorganised and free than economical and enslaved. The author, however, does not seem to regard his own proposals as likely to be accepted, and he might well have adopted as his motto Carlyle's lament that "Labour will vainly need to be somewhat 'organised' as they say—God knows with what difficulty."

The Life-History of the Lobsters.

During the past few weeks an unusual number of important contributions have been made to the discussion of some of the most fundamental problems of modern Biology. Fanciful speculations and "genealogical trees," of course, flourish as ever; but there is an obvious tendency in nearly all directions towards a more minute and laborious study of the facts and phenomena in which the processes of life must be ultimately revealed. Investigations like those of Brooks and Herrick on the life-history of the Macrurous Crustacea, of Jousseaume on the distribution of the varieties of Mollusca in the Red Sea, and of Kükenthal on the development of the mammalian

teeth, are substantial advances of which the importance cannot be over-estimated.

The researches of Professor W. K. Brooks and Mr. F. H. Herrick, of which a general preliminary account appears in the April *Johns Hopkins University Circular*, seem to have an especially important bearing on certain current doctrines in Evolution. If these authors' interpretation of the facts in the observed life-history of several highly organised Crustacea be correct, it would appear that a single specific type of animal may result from the development of two or more different series of immature stages—that either the life-history of the individual does not always recapitulate the history of the race, or else that any existing type of life may have been evolved from several distinct but gradually converging series of ancestors. We cannot do better, indeed, than quote from the Report to show the nature of the evidence:—

"One of the most remarkable results of our study of the various species of the genus *Alpheus* is the discovery that, while there is such a general similarity as we might expect between the larval stages of the different species, the individuals of a single species sometimes differ more from each other, as regards their metamorphosis, than the individuals of two very distinct species.

"This phenomenon has been observed by us and carefully studied in two species, *Alpheus heterochelis* and *Alpheus saulcyi*. . . . In the case of the first species, the difference seems to be geographical, for while all the individuals which live in the same locality pass through the same series of larval stages, the life-history of those which are found at Key West is very different from that of those which live on the coast of North Carolina, while those which we studied in the Bahama Islands present still another life-history. In the case of the second species, *A. saulcyi*, the difference stands in direct relation to the conditions of life. The individuals of this species inhabit the tubes and chambers of two species of sponges, which are often found growing side by side upon the bottom, and the metamorphosis of those which live in one of these sponges is quite different from that of those which inhabit the other. In this species the adults also are different from each other, but as we found a perfect series of transitional forms there is no good reason for regarding them as specifically distinct, and in the case of the other species, *A. heterochelis*, we were unable, after the most thorough and minute comparison, to find any difference whatever between adults from North Carolina and those from the Bahama Islands, although their life-histories exhibit a most surprising lack of agreement. In fact, the early stages in the life of *A. heterochelis* on the Bahama Islands differ much less from those of *A. minor* or *A. normani*, than they do from those of the North Carolina *A. heterochelis*, and, according to Packard, the Key West *heterochelis* presents still another life-history.

"The phenomenon is, however, a highly remarkable one, and worthy the most thorough examination, for it is a most surprising departure from one of the established laws of embryology; the law that the embryonic and larval stages of animals best exhibit their fundamental affinities and general resemblances while their specific characteristics and individual peculiarities make their appearance later."

We may add that partly analogous results have lately been obtained by Mr. G. A. Boulenger in the study of the tadpoles of frogs and toads (*Proc. Zool. Soc.*, 1891, pp. 593–627). The tadpoles of one and the same species found in different ponds often vary much in size and coloration, and in some other characters, whereas the adults evolved from these tadpoles exhibit no corresponding differences. The subject thus opened up is one of very wide import, and the appearance of Messrs. Brooks and Herrick's completed work in the *Memoirs of the National Academy of Sciences* will be anxiously awaited, both by zoologists and palæontologists.

The Mollusca of the Red Sea.

From its geographical position, the Red Sea exhibits features of perennial interest to biologists, and the new account of its molluscan fauna appearing in the last number of the *Annales des Sciences Naturelles* (vol. xii., pp. 343–363, 1892) is a welcome contribution to the literature of the subject. Dr. Jousseaume, the author of this memoir, has made extensive explorations in the Red Sea, with the result that he is able to add 200 species to the list of 800 species of mollusca already recorded by previous observers. He has also discovered a new species of *Cistella* which is the first Brachiopod obtained from the Red Sea. Dr. Jousseaume supports the belief that the molluscan fauna of this area presents no close affinities with that of the Mediterranean; and he believes that the species stated by some previous observers to be common to these two basins have been recorded by mistake. He especially instances the use of Mediterranean sponges at Suez, and shows how some of the shells supposed to occur here have been brought with these articles of commerce. The Red Sea is, indeed, as Mr. Edgar Smith pointed out last year (*Proc. Zool. Soc.*, June 16, 1891), to be regarded as a large gulf of the Indian Ocean.

The great value of Dr. Jousseaume's work, however, lies in the attention he has given to the variation of some of the commoner species of the mollusca in accordance with their surroundings. He finds that certain forms are absolutely stable, whatever be the nature of their environment; variations being confined exclusively to differences in the intensity of the coloration. On the other hand, there are many species that appear to exhibit almost as many varieties as can be observed in any species of domestic animals. These "modify their forms, vary their colour, multiply or simplify their ornaments" according to circumstances. At the same time, Dr. Jousseaume is a firm believer in the reality of "species," and his observations on this point are worthy of special quotation:—

"However great may be the variations I have observed in the different individuals, they have always appeared to me to be confined within certain limits for each species—limits which could not be

transgressed either by the influence of the surroundings or by slow and progressive evolution. These limits are so sharply defined that, notwithstanding the great number of species of mollusca founded upon insufficient materials, it has always been easy for me to distinguish one species from another. I would even add that among the twelve or fifteen thousand species I have studied, I have never found intermediate species establishing a gradation from one to another, and I have only recognised in the variability of mollusca a peculiar faculty by which certain species are enabled to assume the form and coloration of the bodies on which they are fixed—a provision through which they can live without danger in localities of different aspect and greater extent.

"Moreover, I cannot explain to my satisfaction the passage of one species to another, without assuming an abnormal cause, which, suddenly coming into operation at undetermined periods in certain parts of the globe, would change the direction of growth which each species inherits from its birth."

Palæontologists, we imagine, will dissent from the latter proposition most emphatically; and we may express the hope that Dr. Jousseaume will soon be able to publish some precise details as to the evidence on which his conclusions are based. So far as the shape and ornamentation of the shells are concerned, numerous examples of the most gradual modification of the forms of one geological stratum into those of an immediately overlying stratum, could be cited by almost any student of the Secondary and Tertiary mollusca.

The Succession of Teeth in the Mammalia.

It is a familiar fact that, whereas nearly all the lower vertebrates —fishes, batrachians, and reptiles—have an almost unlimited power of reproducing their teeth as occasion requires, the higher vertebrates, or mammals, are never provided with more than one change of teeth during their lifetime. It has also been for a long time well ascertained, that the lower mammals (Marsupialia) as a rule exhibit even less change of teeth than the higher (or placental) mammals. There has thus been great difficulty hitherto in explaining the manner in which the mammalian type of dentition became evolved from the primitive constantly-reproduced type such as we observe, for instance, in the crocodiles. Numerous theories have been proposed to account for the apparent anomaly that the lower mammals exhibit less tooth-change than the higher mammals; while, contrary to the teaching of the great pioneers in Comparative Anatomy such as Cuvier and Owen, the majority of the modern School has long held the belief that mammals originally possessed but one set of teeth and gradually acquired the power of reproducing part of this set once in a lifetime.

The remarkable investigations of Dr. Willy Kükenthal, treated in a lecture published last month,[1] and in previous, more detailed

[1] "Ueber den Ursprung und die Entwickelung der Säugetierzähne," *Jena. Zeitschr. f. Naturw.*, vol. xxvi, pp. 469–489 (1892).

communications to the *Anatomischer Anzeiger* for 1891, have now afforded a definite solution of the problem. It appears from an examination of some embryos of opossums that the replacing set of teeth is actually present in the jaw, although only one of these teeth ever completely develops and becomes functional. It is also proved rom a study of the embryos of certain armadillos and toothed whales in which no tooth-replacement has been observed, that the replacing set is actually formed though absorbed without use in the adult. In short, we may definitely conclude that mammals originally inherited two sets of teeth from their cold-blooded ancestry—that in some groups such as the toothed whales, some armadillos, and marsupials, the second set of teeth is almost or quite aborted, while in the majority of the higher mammals this set is functional and partly replaces the first set. As a matter of fact, the two series of teeth in the jaw of the embryo always originate by the division of a single series of germs; and in this respect the mammalian dentition presents some difference from that of the reptiles and batrachians. There are, moreover, other points still awaiting elucidation, as remarked in a critical article by Mr. Oldfield Thomas in the April number of the *Annals and Magazine of Natural History*.

Palæontology, as usual, may be expected sooner or later to assist in the solution of the remaining difficulties; and it is interesting to notice in the May number of the *American Journal of Science* (vol. xliii., p. 446) that Professor Marsh has already a small contribution to the subject. In describing the primitive Eocene mammals, *Hyracops* and *Meniscotherium*, the Professor remarks that the replaceable first set of teeth remains in use long after the appearance of the three permanent molars, thus suggesting that the latter are actually a retarded portion of this first set, and not part of the second series.

The Australian Marsupial Mole.

Last month we briefly quoted Professor Cope's observations on the affinities of the much-discussed *Notoryctes*,[1] but refrained from comment in view of Dr. Hans Gadow's promised contribution to the Zoological Society. The results of Dr. Gadow's personal examination of the original specimens brought to England by Professor Stirling were presented to the meeting of this society on May 3, and we congratulate Professor Stirling on the manner in which his work in a distant colony has been confirmed by the subsequent researches in the Museum of Comparative Anatomy at Cambridge. Dr. Gadow recognises the functional pouch and the mammæ placed within; he finds the peculiar arrangement of the urinogenital organs of the male so well known in the Marsupials; and he notes certain skeletal characters that indicate affinities with the same order. In short, Dr. Gadow finally disposes of all doubts as to the non-placental character

[1] *Supra*, p. 167.

of *Notoryctes*, while he shows that both the external resemblances of the animal to the Golden Mole (*Chrysochloris*), and the similarity between the skeleton of its trunk and that of the armadillos, are the result merely of adaptation to a corresponding mode of life. We may even go further and remark, that not only is Professor Cope mistaken in the views he bases on Professor Stirling's writings (he does not appear to have seen actual specimens) but he is also unfortunate in the concluding generalisation on the fauna of Australia as compared with that of South Africa. According to Günther (Catal. Fishes, vol. vii., p. 373), *Gonorhynchus greyi* is not a freshwater, but a marine fish, and occurs both on the coasts of Australia, South Africa, and Japan. The comparison of the Chelonian faunas of South Africa and Australia is particularly unfortunate, from the fact that the South African Pleurodiran tortoises exhibit affinity with the South American, and not with the Australian forms; while the discovery of Insectivores in Australia would only be comparable to the discovery of an emydian Chelonian in that part of the world.

The Glacial Period in the South of England.

The difficult problem of the condition of the south of England during the Glacial Period is again discussed by Professor Prestwich and by Mr. Clement Reid in the May number of the *Quarterly Journal of the Geological Society* (vol. xlviii., pp. 263–361). Professor Prestwich concludes that the raised beaches on the coast of the English and Bristol Channels are contemporaneous with the newer river-drifts of the Thames and Somme valleys; that an uplift not exceeding 120 feet followed the formation of these beaches; and then occurred the period of the Coast Caves and their fauna, and of certain Raised Dunes or Blown Sands. He further argues in favour of a subsequent submergence of the land in one of the latest phases of the Glacial Period. Evidence of this submergence, perhaps to an extent of 1,000 feet, is recognised in the remarkable accumulations of angular gravel or "rubble-drift" that cannot be explained by the ordinary action of weathering. Re-elevation followed after a short interval, and during the emergence there was formed the main portion of this "rubble-drift," which includes the accumulations known as "head," the "Coombe Rock" of Sussex, &c. Some of the fissures filled with ossiferous drift were also formed during the period of elevation. The deposits that followed this final upheaval are those grouped as Recent, including the Alluvium of our rivers. Hence it is that Professor Prestwich concludes "that the Glacial times came, geologically speaking, to within a measurable distance of our own times, and that the transition was short and almost abrupt."

Mr. Reid, on the other hand, deals not only with some of these irregular accumulations, but also with the deposits representing earlier phases of the Glacial Period, founding his remarks on a

careful examination of the Sussex coast in the neighbourhood of Selsea. At the base of the local Pleistocene deposits Mr. Reid has discovered great boulders—some of greenstone and granite, some of comparatively local rocks—squeezed into the underlying Eocene clay; and one block of Bognor Rock is recorded as showing a well-marked striated surface, a feature probably produced by the agency of shore-ice. Immediately above the layer of boulders, sands and clays yield evidence of a temperate fauna and flora; and above these again is the "Coombe Rock" which evidently betokens a return of glacial conditions. Mr. Reid, therefore, concludes that there is distinct proof of an interglacial or mild episode; and he considers that the succession of deposits in the Thames valley may be interpreted in precisely the same manner. The newer rough flint-gravel and stony brickearth in the Thames valley are to be regarded, in his opinion, neither as marine nor as fluviatile, but as frozen soil gravels accumulated on plains gently sloping towards the river. Mr. Reid further expresses his belief that the excavation of the Thames valley dates back to the period of the Chalky Boulder Clay, at a time when the river flowed parallel with the southern margin of the ice-sheet and received its sub-glacial drainage as well as that of its own catchment-basin. The conclusions thus suggested are highly interesting, even if they be heterodox, and they are, at any rate, calculated to stimulate enquiry.

Recent Progress in the Study of Algæ.

So much activity has been displayed during recent years in Phycology, that the anticipation may be safely entertained of a speedy journey over the remainder of the path to be traversed to the point where it shall be abreast of the study of Fungi. Much of this progress has been made abroad, and among recent writings, such papers as those of M. Bornet on *Ectocarpus* (*Bull. Soc. Bot. France*), Graf zu Solms Laubach on *Cymopolia*, *Neomeris*, and *Bornetella* (*Ann. Jard. Bot. Buitenzorg*), deserve mention as solid contributions to our knowledge. The excellent *Atlas deutscher Meeresalgen* of Professor Reinke has somewhat prematurely reached its conclusion. It now consists of 50 illustrations instead of the 100 we were led to expect, and it is greatly to be regretted that the encouragement offered did not warrant a continued existence. As it is, our knowledge of the Phaeophyceæ is greatly the better of the volume. The *Sylloge Algarum* of Dr. De Toni, proceeds without pause, though at present it is travelling over the waste fields of Diatomaceæ. This great systematic undertaking is distinguished more for its merits as a storehouse of reference than as a critical treatment of the Algæ. It is, in fact, anything but critical. Wille has completed his account of the Chlorophyceæ in Engler and Prantl's *Nat. Pflanzenfamilien*. This account is intended as a critical review of facts, but in this

respect it rises little above De Toni. In fact, it is studded with minor blunders and others of moment. For example, the figuring of *Chlorodictyon* as an alga near *Caulerpa* (while it is a lichen with abundant and well-marked apothecia) is scarcely excusable. Agardh had so published it, but Asa Gray promptly put the matter right in the *American Journal of Science*, and more recently Cramer confirmed the emendation.

In this country, which, in the days of Greville and Harvey, held the lead in the study of Algæ, it is very gratifying to report investigation of a thorough kind. Recent papers by Messrs. Murray, Boodle, Massee, Traill, Batters, Harvey Gibson, John B. Carruthers, and of a talented lady worker at this subject, Miss Barton, have appeared in the *Journal* and the *Transactions of the Linnean Society*, the *Journal of Botany*, the *Annals of Botany*, and the *Transactions of the Botanical Society of Edinburgh*. The most noteworthy recent publication is the *Phycological Memoirs* (Dulau & Co.) to which we briefly referred last month. These memoirs embody researches in Algæ conducted in the Department of Botany in the British Museum, where an active band of workers have leagued themselves together. The *Memoirs* are edited by Mr. George Murray, and if this first part is a specimen of future numbers we may be well satisfied as to its success. The first paper introduces a new type of alga into botanical literature. Miss Mitchell and Miss Whitting (both hailing from Newnham) have studied minutely and with judgment *Splachnidium rugosum*, an alga from the Cape, which they elevate into the type of a new order, Splachnidiaceæ, intermediate between Fucaceæ and Laminariaceæ. It bears sporangia like *Laminaria*, in conceptacles like those of the Fucaceæ, but developing after a different fashion, as becomes a new order. It may seem a courageous act on the part of two young botanists to found a new order in what we take to be their first essay in Phycology, but the step is abundantly justified, and they have earned the congratulations of sterner and more experienced workers.

The next two papers are by Mr. Murray. The first, on a fossil alga from the Oolite—a *Caulerpa* from the Kimmeridge Clay—contains also a brief and critical *resumé* of our knowledge of fossil Algæ, in which he takes the side of Nathorst against Saporta. This record appears, on his showing, to be the first evidence of a genuine alga from the secondary rocks. The other paper by Mr. Murray deals with an obscure alga, *Dictyosphaeria*, belonging to the Siphonocladaceæ, in continuation of previous work at this group. Miss Barton is author of a paper on malformations of *Ascophyllum* and *Desmarestia*, also in continuation of previous work on the pathology of Algæ, a subject of which very little is known. The malformation of *Ascophyllum* is due to the operations of a new marine Nematode worm.

Mr. Batters concludes the number with the description of a new genus of perforating Algæ found in the Clyde. His figures, like the other plates of this well-printed publication, are excellently rendered,

but we may well be excused if we fail to accept the statement that the bodies figured as spores are such—on the evidence of the figures. A phycologist of his experience is not likely to have been deceived on a point of this kind, but, as the matter stands, there is little to be said and more to be doubted. However, the authors of these *Memoirs* have certainly to be congratulated on the success of their first number from a scientific point of view. The publication deserves encouragement from botanists, and no doubt will meet with it by making itself indispensable to workers.

The geographical distribution of animal life in North America forms the subject of an interesting memoir by Dr. C. Hart Merriam in the last part of the *Proceedings of the Biological Society of Washington* (vol. vii., pp. 1–64), issued on April 13. With the aid of a coloured map, Dr. Merriam points out that a series of six irregular zones of life may be distinguished in succession on proceeding from the border of the Arctic region southwards. As the result of the investigation, he considers that Mr. Wallace underrates the importance of temperature in determining the distribution of life. He also remarks that the last-named authority is mistaken in supposing that on the two sides of the Rocky Mountains "almost all the mammals, birds, and insects are of distinct species." Dr. Merriam, indeed, entirely disagrees with Mr. Wallace in his views on the distribution of life in North America; and in the concluding section of the memoir he briefly touches on the wider question of the life-provinces of the Northern hemisphere as follows:—

"It now becomes evident that the so-called Palæarctic and Nearctic Regions are the result, in each case, of confounding and combining two wholly distinct regions—the Boreal with the Sonoran in America, and the Boreal with the analogue of the Sonoran in Eurasia. Eliminating these austral elements as wholly foreign to the region to which they have been so persistently attached, there remains a single great circumpolar Boreal region characterised by a remarkably homogeneous fauna, covering the northern parts of America and Eurasia."

For a mass of interesting details we must refer our readers to the memoir itself.

Dr. Georg Baur publishes a preliminary account of his recent visit to the Galapagos Islands in the current number of the *Biologisches Centralblatt* (vol. xii., Nos. 7, 8, April 30, 1892, pp. 221–250). Contrary to the usual belief, Dr. Baur concludes from his observations that these islands were directly connected with the mainland of the American Continent in the early part of the Tertiary period. He also remarks that some portions of the islands display a much more luxuriant vegetation than was observed by Darwin in the comparatively arid localities visited by the "Beagle." The promised detailed report will be awaited with interest.

Dr. D. G. Brinton, Professor of American Archæology in the University of Pennsylvania, has just issued a stirring appeal to the American seats of advanced learning, entitled "Anthropology as a Science, and as a Branch of University Education," pleading for the establishment of a branch of Anthropology in American Universities. It gives an account of the *status* of this question, so far as Europe is concerned, and is accompanied by a comprehensive and well-considered scheme for instruction in Anthropology in its various divisions of Somatology, Ethnology, Ethnography, and Archæology, including laboratory work, both physical and technological, and work in the library and in the field. Dr. Brinton has also published some interesting papers on South American Linguistics, "Native Languages from MSS. and rare printed sources," originally contributed to the *Proceedings of the American Philosophical Society* in the early months of the present year. The languages of the native tribes of South America are probably the least known of any on the globe.

In the last number of the *Zeitschrift für wissenschaftliche Zoologie* (vol. liii., suppl., pp. 43–66, pls. vi., vii., 1892) Professor R. Wiedersheim continues his important researches on the pelvic arch in vertebrates, by discussing its most anterior element or *epipubis*. In *Polypterus* and the Dipnoan fishes, the Professor points out this is merely a process of cartilage strengthening the ventral body-wall, and assisting in the fixation of the pelvis. In other cases—as in salamanders, *Amphiuma*, and reptiles—it serves as the origin of some of the muscles of the hind-limbs. Finally, as a pair of "marsupial bones" in the pouched animals the epipubis again appears to be of chief functional importance as a support of the body-wall. There is a rudiment of the cartilage that gives rise to the marsupial bones in the embryos of all mammals; but in almost all placental mammals it has degenerated in the adult.

The gradual evolution of the complex grinding teeth of the modern hoofed mammals has already been satisfactorily traced by the study of their extinct ancestry. Hitherto, however, with the exception of Klever's researches on the horse, we have had little detailed information as to the gradual development of the teeth in the immature stages of the various existing species. A small, beautifully illustrated memoir by Dr. Julius Taeker, "Zur Kenntniss der Odontogenese bei Ungulaten," just published as an Inaugural Dissertation at Dorpat, will thus be welcomed by specialists interested in the subject. A "bunodont" stage is always passed through however "selenodont" and "hypsodont" the dental crowns may ultimately become; and Dr. Taeker has been able to examine embryos of the horse, pig chevrotain, elk, roe-deer, sheep, and ox.

Entomologists, as a rule, are apt to despise fossil insects, from their ordinarily very fragmentary state of preservation; but we would commend to their special notice the latest discovery in the English Lias described by Dr. Henry Woodward in the May number of the *Geological Magazine*. This is an exquisite example of a Termite (*Palæotermes ellisii*) discovered by Mr. Montagu Browne at Barrow-on-Soar, Leicestershire, exhibiting even the patches of pigment on the wings.

It has long been a matter of astonishment that so few traces of the siliceous skeletons of Radiolaria should be found in the geological formations below those of the Tertiary period. At last, however, the explanation of the mystery is forthcoming, and in the newly-issued part of the *Palæontographica* (vol. xxxviii., pp. 107–200, pls. vi.–xxx., 1892), Dr. Rüst shows how these fossils can be met with almost anywhere, even in the Palæozoic rocks, provided the correct siliceous matrix (usually of only local occurrence) be examined. After a detailed study of thousands of thin sections of hornstones, jaspers, siliceous slates, &c., Dr. Rüst arrives at the conclusion that almost all the Palæozoic Radiolaria can be referred to the genera recognised by Haeckel in the seas of to-day. They afford no insight whatever into the evolution of the group; and the only noteworthy distinctive feature of the Palæozoic Radiolarian fauna appears to consist in the relative abundance of the complex Sphæridiidæ and the comparative absence of Cyrtidiidæ. We may add that Dr. Rüst has overlooked the discovery of Radiolaria in the Lower Silurian (Ordovician) of Scotland, investigated two years ago by Dr. G. J. Hinde.

The remarkable irregular loose surface-deposit known as *loess*, met with in so many parts of Central Asia, is again attracting attention. In the *Comptes Rendus* for April 19, 1892, M. Guillaume Capus records some observations on this formation, especially in Turkestan, and thinks that the greater part of the deposit must have been formed in water. The stratification, the variations in composition, and the occasional intercalation of conglomerates seem to indicate this mode of origin; while it is probable that many local accumulations of comparatively small extent are due to the re-arrangement and re-distribution of the materials by the wind.

The divining rod continues to be employed in this country for the purposes of finding water. In the *Daily Graphic* for April 23 there is a picture of "A Water Wizard in the Isle of Wight"; while on April 19, at Wells, in Somerset, a paper on the Divining Rod was read by Mr. Hippisley before a joint meeting of the local branch of the University Extension and the Wells Natural History Society, the

chair being taken by the Lord Bishop. Mr. Hippisley thought that the subject of "Dowsing" (or the use of the divining rod) might not improperly be classed among those connected with natural history. He mentioned some of his own successful experiences in finding water by means of the rod, and, in explanation, referred to the fact that running water is a good conductor of electricity; his theory being that persons having the "gift" are more susceptible to electric or magnetic action, and that the manner of holding the rod forms a circuit, whereby the operator is affected when passing over a subterranean water-course. We may add that an interesting historical account of the subject, by Mr. R. W. Raymond, was published by the United States Geological Survey ("Mineral Resources of the United States," 1883, pp. 610–626). In this article Mr. Raymond remarked that the application of the rod dated from a very early period, and that it had been used to divine character, and for various other purposes, as well as in the search after mineral treasures and water.

Some unknown fossil seeds from the Cromer Forest-bed, received from Mr. Clement Reid, have just been identified by Professor A. G. Nathorst as belonging to *Naias marina*, a plant now only living at one locality in Britain, in Hickling Broad, Norfolk. Dr. Gunnar Andersson, who first recognised the species in a fossil state, points out that, like *Trapa natans*, it was formerly more plentiful, for he has found the sub-fossil seeds in peat-mosses in various parts of southern Sweden, and has also identified specimens in collections from Schleswig-Holstein, Germany, and Switzerland.

Captain Bower and Dr. Thorold arrived last month at Simla, having completed their journey from India to China across the Tibet tableland, a distance of over 2,000 miles, all of which, except a few miles, has not before been explored. Journeying east, from the Lanakma Pass, they met with a chain of salt lakes, one of which, Hor-Ba-Too, proved to be 17,930 ft. above the sea. The travellers passed within a few marches of Lhassa, but, as is usual, they were required to make a detour to the north. Captain Bower is now engaged in writing his report and completing his maps. Another expedition to the same district (the Tibet tableland) and to the Chinese province of Szechuen, is being promoted by the Russian Government, who have voted 30,000 roubles for the purpose.

I.

The Antelopes of Somali-Land.

AFRICA is truly the land of Antelopes. Of about one hundred and twenty of these graceful Ruminants known to Science, nearly four-fifths are found only within the limits of that continent, and, with few exceptions, all the largest and most highly-developed forms of these animals are restricted to the Æthiopian Region. Antelopes are found over every part of Africa, from Algeria to the Cape, and from the Gambia to Zanzibar. They are met with in every variety of country, whether it be desert, swamp, grassy plain, or forest. Each kind of country has its peculiar forms of Antelope, whereas the Deer, which frequent corresponding situations in all other parts of the world except Australia, are entirely absent from the Æthiopian Region.

Although, as has just been stated, Antelopes are to be met with in every part of Africa, they are most numerous where the country is comparatively open, and where there are grassy plains interspersed with sheltering bushes. South of the Tropic of Capricorn this condition generally prevails, and throughout the Cape Colony and its adjoining territories Antelopes are—or, at all events, before the advent of a European population were—everywhere abundant. The early settlers at the Cape describe Antelopes as to be met with in herds of thousands on the "veldt," and in parts of Africa where the white man and his destructive firearms have not yet penetrated a similar condition prevails even at the present day. When we advance farther north and meet with the dense forests of the Congo and Niger basins we find the mass of Antelopes holding rather to the more open lands on the eastern coast, throughout which they are to be met with in great abundance up to Cape Guardafui. The vast plains traversed by the Upper Nile and its tributaries are likewise well-stocked with Antelope life; but, in the Great Sahara, only some of the more desert-loving forms are to be found. In Senegambia, again, and in the more open districts on the West Coast, many forms of Antelopes occur, but they cannot rival the numbers and varieties of those of Eastern and Southern Africa.

In former days, the sportsman or naturalist who was interested in the pursuit of Antelopes would undoubtedly have taken his passage to Cape Town, and proceeded thence into the interior. At the present time, however, as we have already mentioned, Cape-land is well-nigh

bereft of Antelopes, except some of the smaller and more ordinary species. It is consequently to the East Coast that the pursuer of large game would in these days rather betake himself.

On the East Coast of Africa, nearly under the Equator, at the base of snowy Kilima-njaro, there is a well-known "paradise for sportsmen," the praises of which have been sung by Sir John Willoughby.[1] Here Giraffes, Elephants, Rhinoceroses, Hippopotamuses, and some 16 kinds of Antelopes, amongst which are the Eland, the Koodoo, the Waterbuck, and others of the largest and finest sorts, may still be met with in a state of nature, though it is doubtful how long they will withstand the advance of the "sporting Saxon." But it is not to Taveta and the dominions of the Imperial British East African Company that I now wish to invite special attention. Further north, on the East Coast, and even of easier access to the European Naturalist, what Mr. James calls the "Unknown Horn of Africa" projects into the Indian Ocean. Here, as our sporting friends have lately shown us, a still more remarkable assemblage of Antelopes prevails than is found even round Taveta. Of the 12 Antelopes to be met with within reach of the port of Berbera, four belong to recently discovered species, and two of these are so different in essential characters from their nearest allies that it has been found necessary to institute new genera for their reception. It is on these remarkable forms that we wish to say a few words on the present occasion.

Amongst the newly-discovered Antelopes of Somali-land the most remarkable is certainly the "Gerenouk," or Waller's Gazelle (*Lithocranius walleri*). This singular form was first described by the late Sir Victor Brooke, in 1878,[2] from some skulls belonging to Mr. Gerald Waller, and obtained by that gentleman on the coast of Eastern Africa. Sir Victor described and figured this very peculiar cranium, which differs from that of all other Gazelles in the great backward prolongation of the occiput. Sir Victor likewise pointed out that the skull is much more depressed, that the facial axis forms a more obtuse angle with the cranial axis, and that the rami of the lower jaw are more slender in this than in any other Gazelle. While, however, he did not remove the species from the genus *Gazella*, he expressed much doubt as to "whether it should not constitute the type of a new sub-genus."

In 1884, I first obtained two flat skins of Waller's Gazelle among a series of specimens from Somali-land, sent to me by Mr. C. Hagenbeck, of Hamburg. At the same time, Mr. F. L. James was kind enough to favour me with the loan of a mounted head of the same Antelope from a specimen which he had shot in Northern Somali-land. It was not without much difficulty that I was enabled

[1] "East Africa and its Big Game." London: Longmans, 1889.

[2] *Proc. Zool. Soc.*, 1878, p. 929.

to associate the head and the flat skins with the skull previously described by Brooke, and thus, for the first time, to give some account of the external form of this Antelope. The skins, as I pointed out,[3] are at once recognisable by the very wide and well-marked dark brown dorsal line, which measures seven or eight inches across the back in its widest part, and stands out in strong contrast to the lighter rufous of the sides. Waller's Gazelle, however, is still more noticeable externally on account of its long thin neck, which, as

Fig. 1.—Waller's Gazelle, *Lithocranius walleri* (*Proc. Zool. Soc.*,'1892, p. 101).

Mr. Lort Phillips has stated,[4] has considerable resemblance to that of a Giraffe. This peculiar feature is well shown in the mounted specimens of this Antelope lately placed in the Gallery of the British Museum, from one of which the accompanying drawing (Fig. 1) has been taken.

In 1886, Herr Kohl, in an article upon "new or rare Antelopes in the Natural History Museum of Vienna," proposed to elevate

[3] *Proc. Zool. Soc.*, 1884, pl. xlix., fig. 1. [4] *Proc. Zool. Soc.*, 1885, p. 931.

Waller's Gazelle to generic rank under the title *Lithocranius*,[5] a course which was fully justified, and has been since adopted by Mr. Thomas and myself.

Waller's Gazelle, I should add, appears to extend throughout Somali-land down to the Tana River, where Sir John Willoughby and his companions found it "numerous."[6] It also occurs, though rarely, in the Kilima-njaro district.

In Northern Somali-land, Captain Swayne tells us the Gerenouk is one of the commonest and most widely distributed Antelopes, and is readily distinguishable by its long neck, large giraffe-like eyes, and long mobile muzzle. It is more of a browser of bushes than a grass-feeder, and may be sometimes seen standing on its hind legs with its neck extended, and its fore feet lodged against the trunk of a tree in order to get at the tender shoots and young branches.

The Gerenouk is found all over Northern Somali-land in small families, never in large herds. It is generally met with among scattered bushes, in ravines, and on rocky ground. Its trot is awkward, looking like that of a camel. This Antelope seldom gallops, and its pace is never very fast.

Nearly allied to the Gerenouk, and in some respects intermediate between that form and the typical Gazelles, comes the second newly-discovered Antelope-type of Somali-land—the "Dibatag" or Clarke's Gazelle (*Ammodorcas clarkei*). The first noticeable peculiarity of this form is that, unlike all its relatives of the gazelline subdivision, it has its horns curved and projected straight forwards, like those of the Reed-bucks (*Cervicapra*). So striking is this conformation, that when Mr. Oldfield Thomas received the first pair of horns of this species from his correspondent, Mr. T. W. H. Clarke, he called it *Cervicapra clarkei*.[7] About the same time, however, Captain Swayne kindly forwarded to me two pairs of horns with face-skins attached to them. The examination of these at once showed that *Ammodorcas*, as Mr. Thomas subsequently proposed to call it, had nothing to do with the Reed-bucks, and was most nearly allied to the Gazelles. *Ammodorcas*, in fact, as Mr. Thomas[8] has stated, combines the horns of a Reed-buck with the essential characters of a Gazelle, and shows a special relationship to *Lithocranius* in the form of the skull, which is unusually long in the occipital region, and has a remarkably small and slender lower jaw. Like *Lithocranius*, also, *Ammodorcas* has an extraordinarily long neck, and the female is hornless.

Clarke's Antelope is very local in Somali-land, and is not found at all in the neighbourhood of Berbera. Mr. Clarke met with it in the Dolbahanti country, between 46° and 47° east longitude in the interior, where, however, it is said to be common.

[5] Kohl, *Ann. Mus. Wien*, i., p. 82

[6] "East Africa and its Big Game," p. 289.

[7] *Ann. Mag. Nat. Hist.*, ser. vi., vol. vii., p. 304. [8] *Proc. Zool. Soc.*, 1891, p. 207.

Besides these two aberrant Gazelles, at least three members of the typical genus *Gazella* are found in Northern Somali-land. These are the common Gazelle of the coast (*Gazella pelzelni*), the Flabby-nosed Gazelle of the interior (*G. spekei*), and the Sœmmerring's Gazelle (*G. sœmmerringi*). The ordinary Gazelle of the coast, known to the natives as "Dèro," is a close ally of the Dorcas Gazelle, and other typical forms. It seems to be abundant all over the low northern sea-board of Somali-land, which extends some 30 or 40 miles into the interior. On reaching the elevated plateau which borders this district on the south, the short-coated and brightly-coloured *Gazella pelzelni* is replaced by the long-coated and duller-coloured *Gazella*

Fig. 2.—Clarke's Gazelle, *Ammodorcas clarkei* (Reduced from *Proc. Zool. Soc.*, 1891, pl. xxi.).

spekei, which is further distinguished by the loose, flabby skin which covers the nose.

The habits of these two Gazelles, according to Captain Swayne, are nearly similar. They go about in moderate-sized herds of from six to fifteen in number, and keep to stony or sandy undulating ground, which is thinly dotted over with mimosas.

But the finest of the true Gazelles in Somali-land is the Sœmmerring's Gazelle, known to the natives as "Aoul," which attains to large size and proportions in this district. Five years ago, Captain Swayne tells us, when staying in quarters at Bulhar, he remembers seeing the Aoul, from his bungalow at that port, grazing on the maritime plain. Owing to the persecution of sportsmen they have

now retired further into the interior, but still appear to be one of the commonest of the Somali Antelopes, especially in the district of Bulhar, which lies to the west of Berbera.

We will now turn from the Gazelles to the Bubaline or Cow-like Antelopes, readily distinguishable by their long bovine heads, of which two remarkable new species have lately been discovered in Somali-land.

That an Antelope allied to the Hartebeest of the Cape, and commonly known to the hunters by that name, occurred in Somali-

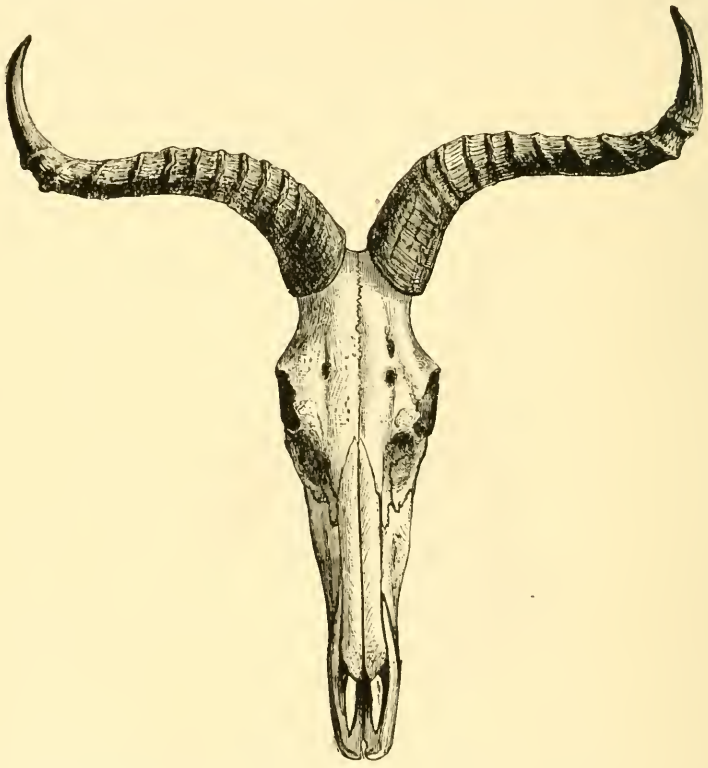

FIG. 3.—Swayne's Bubal, *Bubalis swaynei* (*Proc. Zool. Soc.*, 1892, p. 99).

land and the adjoining districts, I have long been aware, but I have only recently succeeded in obtaining specimens of it, and sufficient information to warrant the bestowal upon it of a distinct name. Swayne's Bubal,[9] *Bubalis swaynei*, as I have proposed to call it, is, in regard to the shape of its horns, as will be seen from our figure, perfectly distinct from the Hartebeest of the Cape (*B. caama*), and, in fact, more nearly allied to the Tora (*B. tora*) of Upper Nubia, and to Coke's Hartebeest (*B. cokei*) of British and German East Africa, being in some respects intermediate between these two species.

[9] *Proc. Zool. Soc.*, 1892, p. 99, pl. v.

I have called this new species after Captain Swayne, who has done so much to increase our knowledge of the fauna of Somali-land, and who has supplied me with the following notes on his discovery of it :—

"South of the highest ranges of Somali-land, and at a distance of about 100 miles from the coast, are open plains, some four or five thousand feet above sea-level, alternating with broken ground covered with thorn-jungle, with an undergrowth of aloes growing sometimes to a height of six feet.

"This elevated country, called the 'Hand,' is waterless for three months, from January to March; it was crossed by Mr. James' party in 1884, when their camels were thirteen days without water.

"Much of the Hand is bush-covered wilderness or open semi-desert, but some of the higher plains are, at the proper season, in early summer, covered as far as the eye can reach with a beautiful carpet of green grass, like English pasture-land. At this time of the year pools of water may be found, as the rainfall is abundant.

"This kind of open grass country is called the 'Ban.' Not a bush is to be seen, and some of these plains are thirty or forty miles in extent each way.

"There is not always much game to be got in the Hand, but a year ago, coming on to ground which had not yet been visited by Europeans, I found one of these plains covered with herds of Harte-beestes, there being perhaps a dozen herds in sight at one time, each herd containing three or four hundred individuals.

"Hundreds of bulls were scattered singly on the outskirts, and in the spaces between the herds, grazing, fighting, or lying down.

"The scene I describe was at a distance of over a hundred miles from Berbera, and the game has probably been driven far beyond that point by now."

In Southern Somali-land, on the northern banks of the great River Tana, which may be taken as the southern boundary of this division of Africa, a still finer and more distinct representative of the Bubaline Antelopes has recently become known to science. This is Hunter's Antelope (*Bubalis hunteri*), which I first described in 1889,[10] and named after its discoverer, Mr. H. C. V. Hunter, F.Z.S. The peculiar form of the long, elevated horns of this species, and the white mark which crosses the forehead and surrounds the eyes, renders this Antelope at once distinguishable amongst its relatives. Mr. Hunter met with it on the grassy plains on the north bank of the River Tana, in herds of from 15 to 25 individuals. It frequents the plains principally, but is also met with in thin, thorny bush. Its native name is "Herota." Mr. Hunter describes it as considerably larger than the Pallah (*Œpyceros melampus*), but standing rather lower than Coke's Hartebeest (*Bubalis cokei*). Though not sloping away at

[10] *Proc. Zool. Soc.*, 1889, pp. 58, 372.

the hind quarters like a Hartebeest, it resembles that species in its heavy gallop, and belongs essentially to the same group.[11]

A third leading feature in the Antelopes of Somali-land is the presence of two Koodoos (*Strepsiceros kudu* and *S. imberbis*). The Greater Koodoo has long been known to us, and is extensively diffused over the greater part of Southern and Eastern Africa, from

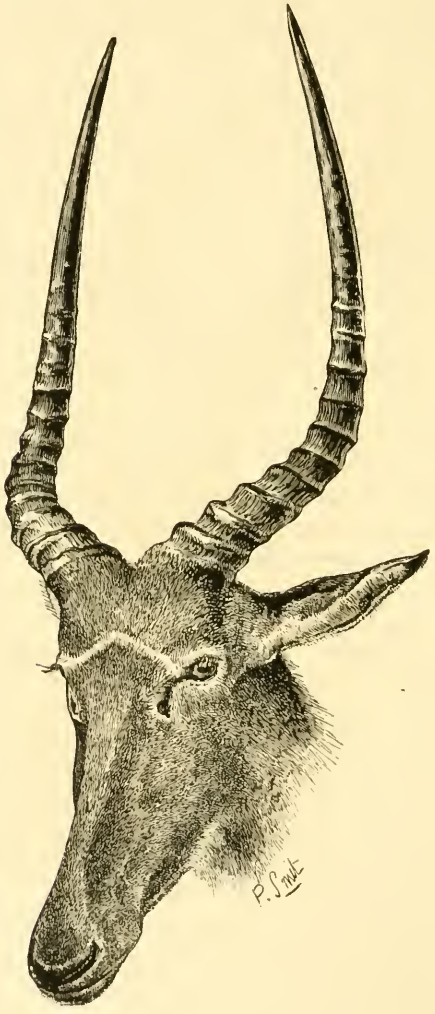

FIG. 4.—Hunter's Bubal, *Bubalis hunteri* (*Proc. Zool. Soc.*, 1889, p. 58).

the Cape to the Abyssinian Highlands. Its enormous spiral horns are admired by everyone. But not so familiar to most people is the Lesser Koodoo (*Strepsiceros imberbis*), a smaller representative of the same beautiful type, which appears to be restricted to Somali-land

[11] *See* Mr. Hunter's Appendix to Willoughby's "East Africa and its Big Game," p. 290.

and the district of Kilima-njaro. The Lesser Koodoo was first discriminated as a species by Blyth in 1869. Blyth pointed out that this Koodoo differs from the larger and better known form (*S. kudu*) in its smaller size, in the absence of the fringe of long hair down the neck in front, and in the much more compressed spiral of the curvature of the horns. From the second of these characteristics he proposed to call the species *Strepsiceros imberbis*.[12] Since Blyth's time I have had the pleasure of making this beautiful antelope better

FIG. 5.—Senegal Antelope, *Bubalis senegalensis* (*Proc. Zool. Soc.*, 1890, p. 355).

known, both from preserved specimens, and from examples living in the Society's Gardens.[13]

In Somali-land, Captain Swayne tells us, the Great Koodoo keeps to the mountain ranges, while the Lesser Koodoo resorts to the bush-covered slopes at their bases. The Greater Koodoo is the largest of all the Somali Antelopes, a large bull standing about 52 inches in height at the shoulders. A good pair of its horns will measure

[12] *Proc. Zool. Soc.*, 1869, p. 51.

[13] "On the Lesser Koodoo," *Proc. Zool. Soc.*, 1884, p. 45, pl. iv.

48 inches round the spiral, but Captain Swayne has seen a pair which measured as much as 56 inches.

As already stated, the Lesser Koodoo lives on much lower ground than its larger brother, and likes to be near water if possible. It has strong hind quarters, and is a great jumper, the white bushy tail flashing over the aloe clumps as it courses away in great bounds. Captain Swayne considers the Lesser Koodoo, with its brilliant markings, as quite the most beautiful of all the Somali Antelopes.

The remaining feature in the Antelope fauna of Somali-land to which I propose to call attention is the presence of a fine species of the *Oryx* group, known as the Beisa (*Oryx beisa*), which is widely distributed all over the country. The Beisa, a very well-marked species of the group, was first discovered by Rüppell in the mountains of Abyssinia, but is now ascertained to extend all over Somali-land. In the Kilima-njaro district, where it was formerly supposed to be also found, it is replaced by a nearly allied form with tufted ears, which Mr. Oldfield Thomas has recently proposed to call *Oryx calotis*.[14]

The Beisa, as Captain Swayne informs us, keeps chiefly to open stony grounds and grassy plains, but may be found in all kinds of country except thick jungles and cedar forests. They are met with in herds of moderate size, which are chiefly composed of cows, while the bulls are frequently seen scattered singly about the country. Captain Swayne gives us an interesting account of the hunting of the Beisa by the Midgans, an outcast race, who pursue this Antelope with packs of savage yellow pariah dogs, for the details of which we must refer our readers to his interesting paper.[15]

Besides the conspicuous and more noticeable Antelopes already mentioned, several other smaller forms of this group of Ruminants are met with in Northern Somali-land, as will be seen by the following list, in which the names are given in systematic order :—

List of the Antelopes of Northern Somali-land.

1. Swayne's Hartebeest.	*Bubalis swaynei.*	"Sig."
2. Salt's Antelope.	*Neotragus saltianus.*	"Sagaro."
3. Klipspringer.	*Oreotragus saltator.*	"Alikhud."
4. Waterbuck.	*Cobus* sp. inc.	
5. Sœmmerring's Gazelle.	*Gazella sœmmerringi.*	"Aoul."
6. Pelzeln's Gazelle.	——— *pelzelni.*	"Dèro."
7. Speke's Gazelle.	——— *spekei.*	"Dèro."
8. Clarke's Gazelle.	*Ammodorcas clarkei.*	"Dibatag."
9. Waller's Gazelle.	*Lithocranius walleri.*	"Gerenouk."
10. Beisa Antelope.	*Oryx beisa.*	"Baet."
11. Greater Koodoo.	*Strepsiceros kudu.*	"Goriali Gôdir."
12. Lesser Koodoo.	——— *imberbis.*	"Arreh Gôdir."

[14] *Proc. Zool. Soc.*, 1892, p. 195.

[15] "Field Notes on the Antelopes of Northern Somali-land," *Proc. Zool. Soc.*, 1892 (May 3rd).

In Southern Somali-land, as already shown, other species also occur, the most conspicuous of which, as yet known, is perhaps the newly-discovered Hunter's Antelope. Another fine and interesting species to be met with in the southern districts of Somali-land is the Senegal Antelope (*Bubalis senegalensis*), which appears to extend from Senegal all across Central Africa up to the Eastern Coast.[16] It has been obtained by several naturalists on Formosa Bay, and northwards as far as Kisamoyo. Sir John Kirk tells me he found it abundant on the open plains of the former locality. Grant's Gazelle and other species are likewise stated to occur on the northern bank of the River Tana ;[17] but we require much further information about the fauna of this district before a correct list of its Antelopes can be made. There can, however, be no question, I think, that, as regards Antelopes, the fauna of Somali-land is more rich and more varied than that of any other portion of the Æthiopian Region, and it seems highly probable that further discoveries have yet to be made before our knowledge of this interesting subject can be deemed complete.

P. L. Sclater.

[16] *Proc. Zool. Soc*, 1890, p. 354.

Willoughby's "East Africa and its Big Game," p. 283.

II.

Recent Researches in Fossil Birds.

PARTLY, perhaps, owing to the difficulty of the subject itself, and in part to the comparative scarcity of their remains, our knowledge of fossil birds has not of late years advanced at the same rapid rate as has that of other branches of zoological science. The appended list of works and memoirs indicates, however, that the past year has been more than an average one as regards the work done in this branch of research.

The main difficulty connected with the subject of fossil birds lies in the circumstance that the great majority of living members of the class present such a general similarity in their plan of structure, that the differences in the skeletons of the various groups are of comparatively small degree, and are totally unlike those which obtain between the skeletons of the various orders of mammals and reptiles. It thus happens that, although it is a comparatively easy matter to assign to their proper serial position remains from the later Tertiary deposits belonging to species more or less closely allied to existing birds, yet when we descend lower in the geological scale and come across bird-remains indicating genera unlike any now living, the task of determining their affinity becomes an exceedingly difficult one. Hence we find considerable discrepancy of view as to the serial position of many extinct birds, even when considerable portions of their skeletons are known.

The aim of the work standing fifth on our list was primarily to revise and name the large series of bird-remains belonging to our National Palæontological Collection, but a survey was also taken of the whole of the known fossil birds of Europe, exclusive of those belonging to the groups of the Passeres and Picariæ. It was thus hoped that a definite land-mark might be established, from which future researches could readily take their start. Perhaps the most important point established in this work is that all the Tertiary Flying Birds may be included in the primary groups into which the Carinate order is subdivided by students of recent ornithology. The Toothed Birds of the Cretaceous rocks of North America and Europe are, however, regarded as constituting a separate series of the same Carinate order, which ranks as of equivalent value with the one containing all the modern toothless birds of flight; this conclusion being strongly supported by the view propounded by Professor D'Arcy Thompson in 1890, that the gigantic American Toothed Bird known

as *Hesperornis* is closely allied to the Divers and Loons, and has no sort of affinity with the Ostrich-like or Ratite birds, among which it had been placed.

The pride of the British Museum collection of fossil birds is (if we except the far-famed skeleton of *Archæopteryx* from the lithographic slates of Bavaria) the magnificent series of Moa bones from the superficial deposits of New Zealand, this series including a large proportion of the type specimens described by our veteran comparative anatomist, Sir Richard Owen. There was, of course, no question but that all these giant birds (the tallest of which probably exceeded twelve feet in height) belonged to the great Ratite group, which includes the modern ostriches, emus, and kiwis; but there has been considerable diversity of view as to whether all the moas should be included in a single genus, or whether they represent several generic groups. A full examination of the magnificent series of moa-bones in the Museum left, however, no doubt as to the absolute necessity of adopting the latter view. These birds were accordingly divided into five genera, of which the true long-legged moas (*Dinornis*) form one end of the series, while the short-legged elephant-footed moa occupies the opposite position, under the generic name of *Pachyornis*. The intermediate, and mostly smaller, modifications of the moa-type are arranged under the generic names of *Megalapteryx*, *Anomalopteryx*, and *Emeus*. It is of one of these smaller moas that the Museum possesses an absolutely perfect skeleton; while of others there are remains showing the skin and dried muscles and tendons still clinging to the bones. In the paper standing as No. 4 in the list given below, a somewhat different arrangement of the moas has been proposed; and there are certain points of difference in the views of the respective writers which have yet to be reconciled.

It would have been beyond the province of the work we are considering to enter into any discussion of the conflicting opinions obtaining as to the date at which the moas were exterminated. We may, however, take this opportunity of observing that it appears to us quite clear that this event was a comparatively recent one, and that if it was not brought about by the Maories themselves, it must have been accomplished by the race which immediately preceded them in New Zealand; Mr. H. O. Forbes's[1] recent researches pointing strongly to the conclusion that it was the work of the Maories.

The extraordinary profusion in which moa-bones formerly occurred in various parts of New Zealand, either strewing the surface of the ground, or buried in swamps, like the celebrated Glemmark swamp near Canterbury, or entombed in the accumulations around the ancient native cooking-places, have been long known. It has, however, been reserved for the year 1891 to reveal what appears to

[1] *Trans. New Zealand Inst.*, vol. xxiii., pp. 373, 375 (1891).

be the largest accumulation of these remains yet discovered. This "find," as we learn from a newspaper report, occurred in a cultivated field near Oamaru point in the South Island. Here, on removing the top layer of soil, an accumulation of bones was revealed, which are estimated to have belonged to several hundred individuals of various ages and species. What could have been the cause of such a vast accumulation of bird-bones in so small an area, remains at present a complete mystery.

In addition to the moas of New Zealand, the discovery of the remains of the huge *Æpyornis* of Madagascar—first made known to us by its monstrous eggs—and of those of the somewhat smaller *Dromornis* of Australia, has for many years made it a matter of common knowledge that huge Ratite birds were widely spread over the islands of the Southern hemisphere at a comparatively recent epoch. To find evidence of the existence of such gigantic birds in the European area, we have, however, to go back to the very commencement of the Eocene division of the Tertiary period—viz., the London Clay and the underlying Woolwich and Reading beds, or their continental equivalents. Thus, in England we have the genus *Dasornis* represented by a skull from the London Clay, and the allied *Gastornis* by limb-bones from the Woolwich and Reading beds near Croydon; while on the continent, in Belgium and France, the birds of the latter genus are so well represented that we now know nearly the whole of their skeleton. In spite, however, of this comparatively full knowledge of their osteology, there has been great doubt as to whether we ought to regard *Gastornis* as a gigantic swan-like bird deprived of wings, or as a member of the Ratite group. The available evidence points very strongly in favour of the latter reference; and accordingly in the Museum Catalogue these Eocene giants succeed the moas and *Dromornis*.

For some years it has been known that the Lower Eocene of the United States yields remains of an ally of *Gastornis*, described under the name of *Diatryma*, but it is only during the latter half of the past year that we have acquired definite information as to the existence of a number of apparently allied, and equally gigantic, birds in the lower Tertiaries of South America. For our knowledge of these birds we are indebted to the memoirs marked 1, 2, and 7 in our list.

In the first memoir on our list, Professor Ameghino tells us that as far back as the year 1887 he described a peculiar mandible from the lower Tertiary beds of Patagonia, under the name of *Phorusrhachos* (now amended to *Phororhachos*) which he then believed to belong to an Edentate Mammal. It turns out, however, as clearly shown by the figure given in the memoir, that this specimen is really the anterior half of the mandible of a gigantic bird; and, from the same deposits, there have now been obtained a number of the limb-bones of this and other gigantic birds, which have been provisionally described by Professor Ameghino. These remains, it is stated, indicate birds of

a totally abnormal type, and larger than any hitherto known, by the side of which the moas may be regarded as dwarfs. These huge dimensions appear to be supported by the figure of the mandible, in which the portion in front of the vacuity in the side measures more than 10 inches in length; so that the length of the whole lower jaw would probably exceed a foot. The tip of this gigantic mandible is turned upwards, somewhat after the fashion obtaining in the Agami (*Psophia*) and Chauna (*Dicholophus*) of South America. Professor Ameghino admits that he is unable to assign a definite systematic position to the bird to which these remains pertained.

In the finely-illustrated folio memoir standing seventh on our list, MM. Moreno and Mercerat, of the Museum of La Plata, give us figures of the bones of a large number of these gigantic extinct flightless birds, from the Lower Tertiaries of Argentina, which are referred to no less than nine distinct genera.

The largest of the birds described by MM. Moreno and Mercerat is designated *Brontornis*, and has a tibia measuring 30·5 inches in length. Although a bird with such a leg-bone is indisputably entitled to be called gigantic, yet such dimensions scarcely bear out Professor Ameghino's assertion that, by the side of these South American birds, the moas were but dwarfs, when we bear in mind that the largest moa has a tibia of upwards of 39 inches in length. The whole of these birds are referred to a new order, designated Stereornithes, said to be allied to the Anseres (Ducks and Geese), Herodiones (Storks and Herons), and Accipitres (Diurnal Birds of Prey). In our own opinion, these so-called Stereornithes were probably allied to the European *Gastornis*; and they serve, in any case, to indicate that, early in the Tertiary period, gigantic flightless birds were very widely spread over the continents of the world, from which they would appear, in most instances, to have retired, in later epochs, to the security of the larger islands of the warmer regions of the globe. Further light on this subject is thrown by the memoir standing second on our list, which is an especially valuable one, as showing the present state of our knowledge of the fossil birds of Argentina and Patagonia. Among the points calling for special notice, we may mention that the bird described by MM. Moreno and Mercerat as a gigantic stork, under the name of *Palæociconia*, is really a Ratite; and the name of *Prociconia* is consequently proposed for the undoubted stork from the cavern deposits of Brazil, which had been referred in the British Museum Catalogue to *Palæociconia*. A new family (Pelecyornithidæ) is proposed for a group of birds allied to the American Rheas; these having been incorrectly referred by Moreno and Mercerat to the Cathartidæ. The so-called Stereornithes of the latter writers are definitely included in the Ratitæ, and are all referred to the single family Phororhacosidæ, with only the two genera *Phororhacos* and *Brontornis*, in lieu of the host of names we have referred to in our notice of the large memoir.

The description by MM. Moreno and Mercerat of an extinct genus of Penguin (*Palæospheniscus*) in the Lower Tertiary of Patagonia is very significant, when we bear in mind that another (*Palæeudyptes*) occurs in the Eocene of New Zealand, and tends to support the view of these writers that South America was connected with New Zealand and Australia by means of an antarctic continent.

From the earlier Tertiary birds of South America, we pass by an easy transition to those of the higher Tertiary of a district in North America treated of in Dr. Shufeldt's memoir. The bird-remains there noticed were obtained from Pliocene deposits in the Silver Lake region in the south-west of Oregon. The collection is unusually extensive, comprising remains referred to upwards of fifty-one species, thirteen of which are regarded as new. Since Dr. Shufeldt's memoir is only preliminary to a fully illustrated monograph, we must reserve our judgment as to the affinities of these extinct types until the appearance of the latter. Of the collection as a whole, the author observes that it is worthy of attention that even in an horizon of Pliocene age, "many of the species were identical with those now existing, and in the case of the extinct ones, they were forms that, in the majority of instances, would not be out of place even in our present day avi-fauna, belonging as they did, in most instances, to modern genera and groups." So far, indeed, as we can gather from a perusal of the memoir, the only new genus (*Palæotetrix*) is formed for the reception of a large grouse belonging to a hitherto unknown type.

As being of a faunistic nature, the paper marked No. 6 appropriately follows the preceding one. Instead, however, of being the preliminary notice of fuller work to come, it is a *resumé* of the present state of our knowledge of the fossil birds of Britain. A reference (in many cases necessarily very brief) will be found to every species hitherto recorded from that area. As the total number of species mentioned but slightly exceeds sixty, of which some forty-five are still existing, it is manifest that there is yet much work remaining to be done (if materials are but forthcoming) before we can be said to have acquired any adequate knowledge of the British fossil birds of even one geological epoch.

We have reserved to the last our notice of the memoir standing third on our list, not for the reason that it is by any means lacking in interest, but because it treats of only two species which have but little connection with any of those hitherto mentioned. More or less nearly complete skeletons of birds from the Upper Eocene gypsum quarries of Montmartre, on the outskirts of Paris, have been known since the time of Cuvier. These are, however, of comparative rarity, and in some cases are in a condition not admitting of satisfactory determination. The two skeletons described by Dr. Flot are, on the other hand, in an excellent state of preservation, and are of special interest from a distributional point of view.

In his classic work on the fossil birds of France, Professor A.

Milne-Edwards proposed the name *Laurillardia* for a bird represented by an imperfect skeleton from Montmartre, which had been tentatively placed by Cuvier among the Picarians. That this bird was not a Picarian appeared indisputable to M. Milne-Edwards, by whom it was transferred to the Passeres, with the suggestion that it might be allied to *Promerops*. The two skeletons described in the memoir before us are found by Dr. Flot to belong to the genus *Laurillardia*, of which they indicate two new species. A careful study of their structure reveals the fact that these skeletons belonged to a bird closely allied to the thrushes, but with shorter feet. On comparison with recent birds, the only one which presents a similarity is the rare *Hartlaubia* of Madagascar, commonly known as the Madagascar Thrush. That bird is a peculiar intermediate type, having the habits of a starling and the structure of a thrush, although the feet are smaller than in the true thrushes. There is, accordingly, no doubt as to the close alliance of *Hartlaubia* and *Laurillardia*, the latter being distinguished by its more powerful beak and shorter leg-bones: the proportions of the latter approximating the extinct genus to the Picarians. Similar Picarian features are exhibited by the limbs of the aberrant Madagascar starling-like bird known as *Euryceros*.

We thus have evidence, observes Dr. Flot, "at the epoch of the Paris gypsum, of the existence of birds intermediate between the thrushes and the starlings, allied to the syndactylous Picarians by the proportions of the leg and foot, but true Passerines as regards the arrangement of the toes, and represented at the present day by the Madagascar Thrush." *Hartlaubia*, it may be added, is a bird of slow and clumsy build, which could only be expected to survive in a region where the struggle for existence was not so severe as it is on most of the continents. The lemurs and civets of Madagascar have already suggested a comparison of the mammalia of that island with those of the Upper Eocene of France; and we now have evidence of a distinct connection between the bird-faunas of the two areas.

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R. LYDEKKER.

III.

The Cave Men of Mentone.

THE Mentone Caves and their contents have been well-known to Geological students as subjects of controversy during the past twenty-five years, and the recent discovery of new skeletons there will arouse a fresh interest in a locality that has been regarded hitherto as somewhat unsatisfactory. As the future of the specimens in question is very doubtful, owing to disputed ownership, atmospheric influence, and the destructive agency of the tourist's umbrella, it may be of some use to record at once the leading facts concerning the remains discovered.

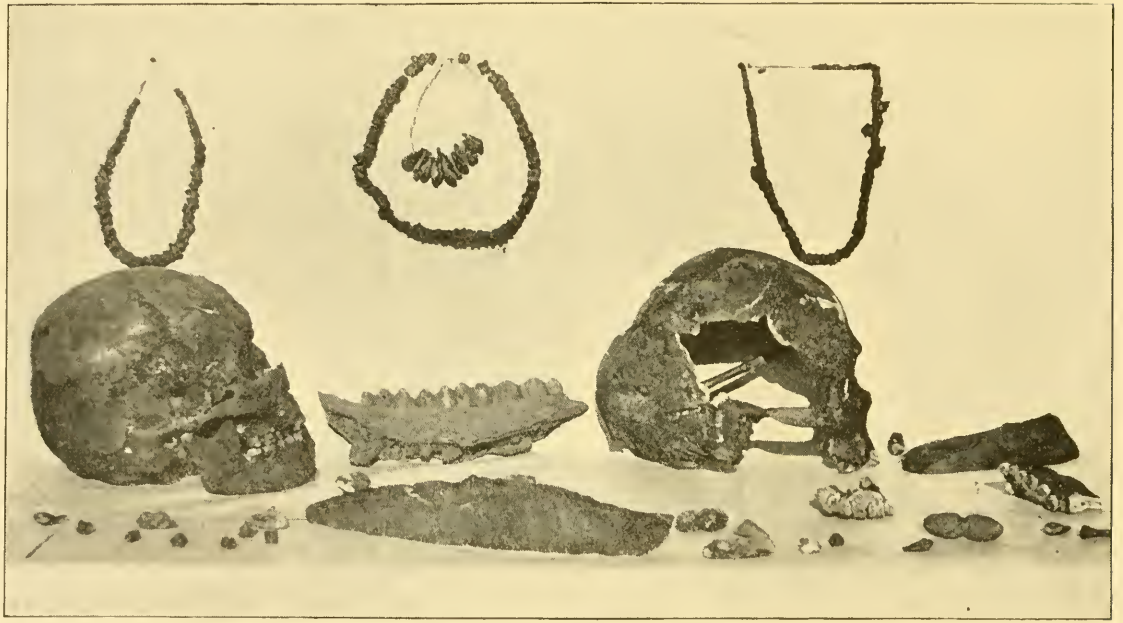
In the mass of Lower Cretaceous Limestone to the east of Mentone, forming the cliff called the Red Rocks, lies the series of Caves of Baoussé Roussé[1] (Balzi Rossi), situated on the Italian side of the stream and valley that constitute the frontier. The best known skeleton from this district, found by M. Rivière in 1873 and figured in many text-books, was discovered in Cave 3, counting from west to east (Cave 4 of M. Rivière). It is Cave 4 (referred to as 5 by M. Rivière) that has yielded the skeletons of 1884 and 1892. We are at present concerned with this one only.

The mouth of this cave is in a wall of sea-cliff, quarried for stone, about 30 or more metres from the present sea-margin. It seems, on inquiry, that the west side of the present cave-mouth has been in much the same position for many years, though the east side has been a good deal cut back by quarrying.[2] The cave itself is a great vertical cleft, measuring some 25 metres in length by 3 to 6 metres in width. The walls converge till they meet about 15 metres above the present floor. Before any excavations were made the cave-floor was at the mouth six metres higher than now, and the present floor is simply the level at which former excavations ceased. Moreover, as noted by M. Forel in 1858, the floor had even then been lowered by natural agencies, as shown by ossiferous incrustations clinging to the walls.

Of the nature of the mass of material already removed, and the

[1] A description of the contents of the caves will be found in M. Rivière's papers, "De l'antiquité de l'homme dans les Alpes Maritimes." Paris, 1878–1886 (Ballière).

[2] This has to be borne in mind in trying to fix now the position in which the 1884 skeleton was found.



HUMAN SKULLS, ORNAMENTS, IMPLEMENTS, AND ANIMAL REMAINS FOUND IN THE CAVERNS OF MENTONE, FEBRUARY, 1892.

[From a Photograph by M. Bertrand, of Mentone.]

The skull on the right belongs to the largest skeleton (No. 1), and that on the left to No. 3. Above are shown the necklaces of perforated teeth and fish-vertebræ; in front is the largest of the flint flakes; and to the right is the bone ornament. There are also teeth and fragments of jaws of auroch found near the human skeleton.

level at which bones and implements have been found, there is little accurate information to be obtained on the spot. M. Forel's specimens (1858) were scattered through a superficial layer half a yard thick, and included remains of the stag, roe, sheep, antelope (?), auroch (*Bos primigenius*), horse, boar, wolf, fox, cat, rabbit, cachalot (?), with mollusca (*Dentalium*, *Patella*, *Pecten*, and *Pectunculus*), also arrow-heads, fish-hooks, spear-heads, and knives of rough make and not more than two or three inches long. All these were in the surface layers.

Between the years 1866–1886 the great mass of the cave-contents was removed to a depth of six to eight metres by MM. Rivière, Bonfils, and others. The whole thickness consisted of loose earth, much of it of vegetable origin, without any layers of stalagmitic deposit. We have no data for estimating the rate of accumulation of such material, but it is evident that the time required must be very great. Flint implements were found in numbers throughout, mostly small and rough flakes of uncertain age; but it is important to note that no pottery of any kind occurred. In this connection the occurrence of remains of extinct animals, even in the higher layers, should also be borne in mind. One of the most interesting of these is the skull of an ancient species of Marmot (*Arctomys primigenia*), indicating a far colder climate.

The skeleton of 1884 was found just inside the cave-mouth (close to the position of the present relics) at a depth below the original floor of 8 metres 40 cm. It was lying on its back, between the cave-wall and a large stone, parallel to the long axis of the cave and with the head towards the entrance. The depth from the old floor-line is somewhat more than my estimate for the present discovery; but as the cave is here only 3 metres wide, it is very difficult to see how the discoverers of the older example could have missed the present specimens if they excavated to a greater depth. Allowing the maximum depth for the position of this skeleton and the minimum for those of 1892, there is not more than a yard of difference in position between them.

It was on February 7th, 1892, that the quarrymen then at work discovered on the east side of the cave at a depth of $7\frac{1}{2}$ metres (below the original floor) three skeletons close together near the cave-mouth. They lie only $1\frac{1}{2}$ metres below the present floor, and this thickness is composed of soft earth and stones which have probably been turned over before, though, curiously enough, without discovering the skeletons. The bed on which they rest is scarcely harder in texture, but redder in colour, and the bones and ornaments are strongly tinged with red. Below the skeletons is still a deposit of dark earth with stones, similar to that above. There is an evident stratification, and the deposit has not been disturbed before. Here have been found several flint-flakes and some rough, pointed and polished bone awls; but as there is little of this stratum left, not much is to be expected from its further excavation.

Although the skeletons had doubtless been buried, there is nothing to show that any special tomb had been made; and owing to the removal of almost all superincumbent strata and the *remanié* character of what is left, it is impossible to say how far the overlying layers had been disturbed. Still, it is not likely that neolithic sextons with flint spades could dig more than five or six feet to dispose of the remains of a fellow-man even of seven feet length, and one may take it as certain that the period that has elapsed since the interment is represented by at least 15 or 16 feet of deposit.

The outermost skeleton is lying on its back, and measures just 2 metres from the end of the toes to the position of the collar-bone. Deducting for the foot and measuring from the heel, with addition for the head and neck, the height must have been seven feet at a low estimate.[3] The arms were lying at the side, the left hand resting over the head of the femur and enclosing a flint implement. By March 15th, when the present writer first saw the remains, the skull had been removed; the arms had disappeared, all but part of the left humerus and ulna, and there was no trace of shoulder bones or ribs. The pelvic bones had also been removed, as well as the sacrum; but 14 vertebræ could be counted in position.

The other two skeletons are smaller, measuring little over six feet in length, and both lie on the left side, the legs turned forward and bent slightly at the knee. The arms are bent so that the right hands come to a level with the shoulder. The second implement was found in the hand of the middle skeleton, the third under the head of the innermost.

The left arm of the third was not bent up, but the hand was lying by the ilium of No. 2. This arm has now disappeared.

As regards the skulls, the middle one has been entirely destroyed, while the others are much damaged. The first (1) is of size proportionate to the skeleton, and enough remains to show some general characters, though the left side, the jaw, and the basis cranii are wanting. The maximum length is 21 cm.; and the greatest width in the parietal region is 15 cm.

The cephalic indices are therefore 71·5 and 66·5, the former very near that of the 1884 skull, determined by Mr. Wilson[4] as 71·35, and near those of Cro-Magnon. The craniofacial angle is not easy to determine, owing to the destruction of the occiput and basis cranii, but it seems to be approximately 90°. There is a distinct prognathism about the former.

The contour of the two crania is different. The larger skull (1)

[3] The height measurement given by M. Rivière, 1·85 (=6 ft. 2 in.), is certainly below the mark, and was calculated from the femur. That given in some papers of 2·0 metres (or 6 ft. 8 in.) without the skull, is too large, as it is taken from the end of the toes. The exact measure will probably never be known, as the neck and shoulder region is now destroyed. M. Rivière's note on the 1892 skeletons was read to the Paris Academy of Sciences on March 7. I know it only from a newspaper report containing several inaccuracies.

[4] Brit. Assoc. Rep., 1885, p. 1218.

is higher in the parietal region, and projects to a remarkable extent above the occipital region, so much so that it is, perhaps, abnormal. The smaller skull is highest in the posterior parietal region; it is, moreover, curiously asymmetrical, the right parietal region being much flattened, and the left higher and more convex.

All the skulls, then, are dolichocephalic, and one of the new ones closely approaches that of 1884. The three may be placed in series. That of 1884 has the more prominent supra-orbital ridges and projecting chin, and is widest in the frontal region. The large skull of 1892 has the prominent eyebrow-ridge, but is slightly narrower in the frontal region. The smaller skull has these prominences less marked, and is narrower across the frontal bones, but still stronger, thicker, and more definitely ridged than the neolithic skull of Final Marina referred to below. All have the somewhat quadrangular-shaped orbit relied on by M. Rivière as evidence of antiquity.

The lower jaw (complete only in No. 3) is very broad in the chin-region, and wide at the side in the area of attachment of the masseter muscle.

The teeth in the larger skull, so far as left, are well worn down, and the crowns are flat, indicating an advanced age for the individual in question. In skull No. 3, on the other hand, the teeth are little worn, and the last molar has not yet come into use, showing that the age of the individual was not more than 20 years. The third skeleton has been said to be that of a child, but as it is quite as large, there seems no reason for attributing to it a younger age than the second. The skull has been lost.

Whatever, then, are the differences between the individual skulls found at different times in the Mentone Caves, it is of importance to note that they are as a whole widely separated from such types as that found further east in the Final Marina Caves, and now in the Museum at Bordighera. Though full details as to the position in which this specimen was found are not yet obtainable, it may be mentioned that the skull, while dolichocephalic, is much smaller, the supra-occipital region weak, the zygoma slender, and the jaw feeble in comparison; a series of craniological deficiencies which may or may not be correlated with the fact that the skeleton is associated with well-made and ornamented pottery, of which no trace seems to have been found at Mentone. The beautiful preservation of this specimen, which I have had the opportunity of examining through the kindness of C. Bicknell, Esq., suggests that it may be of very late age; it is only referred to here as a type of skull without any of the "Cro-Magnon" characters.

The implements found with the skeletons are larger than those discovered round the 1884 cranium,[5] and also thinner and

[5] These were 6–7 inches long, thicker and without curvature, but at the same time not much wider in proportion, as is the case with so many typical Palæolithic flints.

more curved. They are also two or three times as large as anything found in overlying deposits. The largest measures $10\frac{3}{8}$ inches (26·3 cm.) in length by $1\frac{3}{4}$ (4·5 cm.) in width, and is remarkable both for its thinness (not reaching $\frac{3}{4}$ in.) and its strong curvature. The others measure respectively 23 by 5 by 1·5 cm. and 67 by 5·5 by 1·3 cm., and are also thin, though the curvature is less marked. All are roughly chipped, and show no trace of polishing. Though implements of this character occur with relics of Neolithic date, yet they are by no means distinctively Neolithic. Similar ones occur in France in the Magdalénian period, and in the British Museum there are undoubted Palæolithic fragments of equally large flakes.

A necklace of perforated teeth was discovered round the head of the large skeleton. All the teeth are ground down and polished, and many are ornamented by transverse striæ, so that all distinctive characters in the crowns are obliterated. M. Rivière's opinion that they are canines of deer is probably true of the greater number. The perforations are central between crown and fang; they are cleanly cut and polished, considerably wider at the surface than the centre, as is usually the case with holes bored by flint flakes.

The fish-vertebræ are of rather small size, scarcely exceeding a centimetre in diameter, and are perforated through the axis by holes a millimetre in width. They were found round the necks of all three skeletons and seem to belong to the genus *Salmo*.

Two Cowrie shells perforated just at the anterior end of the mouth were also found, and seem from their position to have been used as anklets. The species is not, I believe, a Mediterranean one, but seems to be identical with an Atlantic form in the Mentone Museum, labelled *Cypræa millepunctata*, though colour and marking have disappeared. M. Rivière refers also to a necklace of small shells of *Nassa* similar to those found with the skeleton of 1873, but it has now disappeared.

The bone ornaments discovered were three in number, lying on the forehead of the skulls, and had been, doubtless, attached to the thong on which the teeth were threaded. All have the form of a spindle, deeply constricted in the middle, with a smoothed surface ornamented by rows of short transverse lines. One measured 2 by $\frac{5}{8}$ in., the others, one larger and one smaller. Internally the texture is somewhat cancellous. M. Rivière is of opinion that they are made from stag's horn.

Coming to the question of the age of the new skeletons, one is confronted with the same difficulties that have attended other discoveries in this region. What importance is to be attached to great thicknesses of cave-deposit when there are no stalagmitic layers to mark particular levels? To what depth are we to suppose Neolithic man could dig graves? How much value is to be attached to the manufacture of ornaments, and how much, on the other hand, to cranial characteristics?

Such skeletons as that already referred to of Final Marina, are of distinctly later type, and associated with a considerable quantity of pottery, grinding stones, &c. Those of Mentone have, it is true, well-worked ornaments and long, narrow flints; but no pottery has been found, and the only relic suggesting a Neolithic age is an axe in the Museum of St. Germain, noticed by Boyd Dawkins in his "Early Man in Britain," p. 229.

The skeleton of 1884 has been regarded by Mr. White, M. Bonfils, and M. Vaucardard as Palæolithic, and they say rightly enough that it is very difficult to suppose the Neolithic men could have dug graves with hands or flints even to five feet, much less to twenty feet. Mr. White, who lays much stress on this point, seems to suggest that the interment must have taken place from the recent floor; but, although that is, of course, not the case, still allowing five feet for the grave there remains an accumulation of some sixteen feet as a monument to the passage of years in Neolithic times.

The cranial characters of the 1884 skull have much in common with the older types of skulls elsewhere. The strong supra-orbital crests, the projecting occiput, wide facial angle, and powerful mandible, are all such features as occur in well-known skulls of older periods. M. Mortillet passed over these characters lightly, attributing the skeleton to the Neolithic time on the strength of the pointed bone implement found near it; and many English writers have similarly denied the antiquity of the Mentone specimens.

Whatever arguments were used for and against the ancient date of the 1884 skeleton, might be used with regard to the present ones in general. The mere fact that the superincumbent 25 feet consisted of soft, more or less overturned earth, so that different horizons either never existed or are gone past recovery, may justify writers in neglecting to attach importance to the remains. Still, we must feel either that post-Neolithic deposits may be of very great thickness, or that the relative value of osteological characteristics and manufactured ornaments is not yet well defined. Now that new skeletons have been found at a well-ascertained level (though interred), it is important that their cranial and all other characters should be carefully investigated. Cave-specialists who are accustomed to have their stratigraphical horizons neatly marked out for them by providential layers of stalagmite-flooring should not be allowed to avoid giving an opinion on other characters when such data are unattainable.

The size of the 1884 skeleton might be exceptional, but the present examples suggest that it may have been characteristic of the race buried at a certain period. The 1884 skeleton had buried with it a large number of short, wide flint flakes about $1\frac{1}{2}$ inches in diameter, as well as the three large implements already referred to; and these smaller ones have a distinctly Palæolithic aspect. With the

1892 skeletons the ornaments are much more finely polished and bored than would be expected in association with the Palæolithic type; but at the same time it must be borne in mind that if Palæolithic man had such artistic capacity as we know he possessed from the classic drawings on bone, it is not difficult to suppose he could perforate teeth for necklaces. It is, on the whole, easier to imagine that man learned the art of making necklaces at an early date, than to suppose that the Mentone skulls belong to the age of polished celts and pottery. One can more readily believe that relatively sudden changes took place in man's technical power in one region (for instance, by learning of other races) than that any rapid change in the forms of skulls occurred. The mere absence of ornaments from the skeleton of 1884 is of little value in discussing its age, though it may tend to indicate the poverty or inartistic character of the subject. The implements, again, though not of the type which we in England know as Palæolithic, are certainly not any of the usual Neolithic patterns. In size and shape they differ markedly from anything found in the overlying deposits, and their value was, doubtless, the reason for burial with their owners.

That the new skeletons, therefore, may well belong to the Neolithic age is possible, and the worked ornaments with them point to that conclusion. But at the same time they show osteological affinities to more ancient types. It seems probable, also, that the 1884 skeleton belongs to the same period, and that its great size was not an isolated accident; while it appears that the deposits which can be formed in a cave where there is no stalagmitic deposition may reach a thickness of 16 feet or more, accumulating since the age when the art of working bones had been perfected. Furthermore, it is likely that the habit of speaking of Palæolithic and Neolithic times, and attempting to draw sharp distinctions between them, carries with it more than the usual evils of all formal nomenclature.

The Mentone skeletons, perhaps, belong to a transition period, when the race of cave-dwellers were of large size and of ancient skeletal build, when they made roughly-chipped implements, and were ignorant of the art of pottery manufacture. On the other hand, they knew how to work bones and to make necklaces of them, and they had, apparently, a definite custom of burial.

It should be stated, in conclusion, that these notes were written on the spot, where reference to books and comparison with other skulls were alike impossible, and that, therefore, too much stress may be laid here on cranial characters. On the other hand, the fact that precisely similar perforated teeth, vertebræ, and shells of *Cypræa* have been found in the Dordogne caves of the same age as the classic drawings on bone of mammoth and reindeer (a fact of which at the time the writer was ignorant), strengthens very much the argument for the Palæolithic age of the Cave Men of Mentone.

A. VAUGHAN JENNINGS.

IV.

Facts and Theories in the Development of Insects.

REFERENCE was made in NATURAL SCIENCE for March (pp. 55, 56) to the Stylopidæ. These insects were formerly regarded as a special order (Strepsiptera), but most entomologists now consider them to be a very aberrant group of Beetles (Coleoptera) related to the Oil Beetles (Cantharidæ) and the Rhipiphoridæ. In both these families the larvæ live parasitically on the grubs of bees and wasps. In the Stylopidæ we see the parasitic habit continued in the female imago, and such modifications in the male that its coleopterous affinities are much obscured. Moreover, the host is not prevented from attaining its imaginal state; indeed, it must be allowed to do so to secure the fertilisation of the parasitic female *Stylops* by her free-flying mate.

Although many species of Cantharidæ and Rhipiphoridæ are known, the life-history of few only has been worked out. The classical researches of Newport and Fabre long ago made known the "hypermetamorphosis" of *Meloë* and *Cantharis*, which belong to the former family. In 1870, Dr. Chapman and Mr. Murray (1) discovered the transformations of *Rhipiphorus paradoxus*, the larva of which they proved to be first a minute active hexapod, then an internal and then an external parasite on the grubs of the social wasps (Vespidæ). And now Dr. A. Chobaut (2) has worked out most of the life-history of another beetle of the same family, *Emenadia flabellata*, belonging to a genus unknown in Britain, and with very few European species. He has shown the great similarity of its transformations to those of *R. paradoxus*, and finds that its hosts are the grubs of a species of solitary wasp (*Odynerus*).

Three *Odynerus* grubs were found within the cavity of a reed, each in its cell closed by earthen walls, as is the habit of the wasp in making its nest. The grubs did not pupate in May, the usual season; but early in June a parasitic larva emerged from each, and fed incessantly for twelve days, sucking at the ventral region of its host until only the skin and mandibles of the latter were left. This parasitic larva is described as entirely without legs, eyes, or antennæ, and with a suctorial mouth.

Four days later these larvæ pupated, and from the form of the pupa Dr. Chobaut decided that they were beetles of the genus *Emenadia*. Early in July the perfect insects were produced—a male and two females, of the species *E. flabellata*. One of the females laid fertile eggs in the middle of July, from which, early in August, were hatched active larvæ of extremely small size, with large head, with a pair of antennæ inserted near the base, the thorax with six well-developed legs, and the abdomen of nine segments, the last two being each provided with a pair of long filaments.

The place in which the female lays her eggs in the natural state, and the details of the transition from the hexapod larva to the legless parasite, have yet to be determined. Dr. Chobaut suggests that the eggs are laid in the earth, and that the hexapod larva attaches itself to the hair of the wasp, which carries it to the nest. The researches of Messrs. Chapman and Murray, however, on the life-history of *Rhipiphorus paradoxus* tend to the conclusion that the beetle lays her eggs in the cells of the wasp's nest.

The hexapod larva of *Emenadia* closely resembles that of *Rhipiphorus*, as described by Dr. Chapman, except that the latter appears to be without the long filaments on the two hinder abdominal segments. Dr. Chapman describes the *Rhipiphorus* grub in its parasitic stage as carrying on its white skin the black chitinous plates of its former hexapod condition, widely separated by the growth of the insect. "These plates," he writes, "may be regarded as a beautiful display of the dermal anatomy of the little active larva." Such plates, however, were not observed by Dr. Chobaut on the grub of *Emenadia*. In all these beetles we find an active hexapod larva preceding the legless parasitic grub. This indicates that the larvæ of the ancestors of these beetles were free-living. A similar hexapod larva is found in some other groups of insects; from its likeness to the adult *Campodea* it is often called "campodiform." As *Campodea* belongs to the most primitive group of living insects (Thysanura), it has been held by Brauer, Lubbock, and most other entomologists, that the occurrence of a campodiform larva as the earliest stage in the post-embryonic life-history of various insects points to the descent of our modern insect groups from a campodean ancestral stock. Further, the fact that some Myriapods have but six legs in their early stages has suggested the derivation of that class also from a similar stock. The vermiform insect larva, of which the caterpillar of a moth is a familiar example, has been generally regarded as a form which has become secondarily modified to meet the requirements of its special mode of life. The pairs of pro-legs present on some of the abdominal segments of a caterpillar have been thought to be not homologous with the six thoracic legs; and it has been considered that moths, no less than beetles, are descended from campodean ancestors. The primitive campodean itself must have been derived from a vermiform ancestor, and therefore the caterpillar has

been regarded as an instance of atavism, of retrogression towards the form of an extremely remote progenitor.

The primitive nature of the campodiform larva has, however, now been called in question by Cholodkowsky. In the concluding section (on the morphology and phylogeny of insects) of his recent paper (3) he reviews the latest researches on insect embryology. He states that Kowalevsky and Tichomirow have described appendages on the abdominal segments of the embryos of moths (*Smerinthus populi* and *Bombyx mori*). Graber also described abdominal appendages in the embryo of *Gastropacha pini*, but considered them secondary structures because of their late appearance. Now, Cholodkowsky believes that these embryonic appendages disprove the derivation of the caterpillar from the campodean larva, and establish the homology of the pro-legs with the legs of the thorax. On the other hand, he does not consider the vermiform larva to have given rise to the campodean, because the most primitive winged insects (Orthoptera) have only six legs in their young stage. The conclusions seem to be, that the two kinds of larva have arisen independently of each other, that all insects are derived from a remote many-footed vermiform ancestor (as forms with a campodean larva have abdominal appendages in the *embryonic* stage), but that only some groups can be descended from a campodean ancestor. The entire class of insects is more nearly related to the Myriapods than to any other group of Arthropods.

Many questions of great interest are discussed in this work of Cholodkowsky. He has observed appendages on all the eleven abdominal segments of the embryo cockroach, and he concludes that all these are homologous with the legs and jaws. The appendages of the four hinder abdominal segments become the gonapophyses and cerci of the adult insect, the cerci being the limbs of the hindermost segment. It hence appears that these organs had an ambulatory function in the remote worm-like ancestors of the class.

One of the most startling of Cholodkowsky's conclusions is his interpretation of the insect-head. He states that this consists of certainly more than four, probably six, segments of which one only is pre-oral, and that the antennæ are the appendages of the first post-oral segment. The pre-oral origin of the antennæ of insects has seemed so firmly established during the last few years that this statement will not be received without further confirmation. The contradictory statements already made by various observers on this point are proof of the difficulty of determining whether the embryonic antennæ really arise before or behind the mouth, and Cholodkowsky's own figures do not seem conclusive on the subject. However, he states that there is doubtless an antennal mesodermal somite (clearly figured), and that the rudiment of the antennal ganglion arises behind that of the pre-oral ganglion with which it afterwards fuses.

Should this post-oral origin of the antennæ of insects be established, they will have to be regarded as the homologues of one of the

pairs of antennæ of the crustacea. Cholodkowsky suggests the second pair, though the first pair have been regarded as post-oral by most observers. The generally received view is that insect antennæ, being pre-oral, have nothing to do with those of crustaceans. We must also consider the chelicerae of arachnids homologous with the antennæ of insects, if the latter are really post-oral, instead of with the mandibles, as has hitherto been believed. Indeed, our comparison of arthropod limbs seems to be once again uncertain, especially as small appendages have been described by various observers between the antennæ and mandibles of some insect embryos. Further and more careful researches on a number of types must be undertaken before this fascinating subject of arthropod homology will be settled. Fascinating, indeed, it is to see an undifferentiated limb of the far-off ancestor of the race in one group changed into a jaw for seizing and poisoning prey, in another into an organ of exquisite and mysterious sense.

The fate of the primitive body-cavity, or cœlome, is also discussed by Cholodkowsky. In each segment the cavity divides into three parts. The dorsal part (in some abdominal segments) contains the rudiments of the genital glands;[1] from its wall is formed the muscular coating of the intestine. The lateral part disappears. The ventral part becomes filled with the developing fat-body and loses its boundaries; it is believed to be homologous with the segmental organ of *Peripatus*. This seems probable, and, if established, will be a highly interesting result. It seems certain that the body-cavity of the adult cockroach has nothing to do with the primitive cœlome, but is a "pseudocoel." In Sedgwick's well-known paper, in which he proved this result for *Peripatus* (4), he suggested that the same would probably be established for other Arthropods, and we now have a striking confirmation of his views.

In the cockroach and most other insects, the ventral surface of the embryo where the appendages are being developed, is covered by two membranes, the outer of which is known as the serous membrane and the inner as the amnion. In the embryo of the ichneumon fly (*Mesochorus splendidulus*, Grav.) Kulagin (5) states that these membranes are not formed. In that of another ichneumon (*Microgaster glomeratus*, L.) they are represented only by a small fold at the hinder end of the body. This latter fly lays its eggs in the bodies of the caterpillars of the Cabbage White Butterfly (*Pieris brassicæ*, L.), and hence it has a strong economic as well as scientific interest. The last segment of the parasitic larva has the shape of an inflated bladder, and seems to possess an excretory function.

Observations on the life-history of another ichneumon fly (*Perilitus brevicollis*, Hal.) were made in Algeria by MM. Künckel

[1] The genital cells are believed by Cholodkowsky to be formed from yolk cells, but, as stated in NATURAL SCIENCE for March (p. 55), this is contradicted by Heymons.

d'Herculais and C. Langlois (6). This fly was described by Haliday in 1836 from a single specimen captured in the north of Ireland. Since then only four more examples had been taken, one in Germany and three in England; but now the insect has been found in myriads in Algeria, attacking with great effect the larval and perfect stages of a beetle (*Haltica ampelophaga*, Guèr.) which is extremely destructive to vines.

The parasitic grub of the ichneumon feeds singly within the larva of the beetle, avoiding the vital parts but devouring the fat-body, and thus rendering further development impossible. Fifty to eighty out of every hundred beetle-larvæ examined were affected, and the adult beetles are also liable to be attacked. The value of this parasite to owners of vineyards is evident, and we have another instance of man's best allies against the insect-destroyers of his crops being themselves insects. The study of the development of insects not only enables us to speculate on the nature of their far-distant ancestors, but teaches us how in some measure to save from destruction "the kindly fruits of the earth."

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Geo. H. Carpenter.

V.

Phosphatic Chalk.

NEARLY a year has elapsed since the occurrence of phosphatic chalk in England was first announced (1). Though an exactly similar rock had been extensively worked for some years in the north of France and Belgium, with the result of an enormous increase in the value of the land where it occurred, yet it had never been recognised on our side of the Channel; notwithstanding which it had lain open to view for many years in an old chalk-pit hardly more than twenty miles from London. This seems the more strange, in that the appearance of the rock to the naked eye is sufficiently marked to attract attention, while under the microscope its character is unmistakable; for the deposit consists almost entirely of phosphatised foraminifera in beautiful preservation, together with fragmentary fish-bones, fish-scales, and minute coprolites. These organisms are embedded in chalky mud containing coccoliths and discoliths, and impart to the rock a granular or sandy appearance, and a pale-brown tinge which at once distinguish it from smooth white chalk of the ordinary type.

New discoveries of phosphatic chalk are being made from time to time on the Continent, yet an old chalk-pit at Taplow remains the one known section of such a rock in England. That this is actually its sole occurrence in this country seems scarcely probable. Yet our Chalk, of late years especially, has received much attention from both Foreign and English geologists, and the principal subdivisions into which it can be separated are being traced throughout the country, not only by the Geological Survey, but by men who have made its study a speciality, and who are unlikely to overlook the least variation in its lithological character. Many landowners, moreover, keenly alive to the possibility of increasing the value of their property, from time to time bring forward specimens which seem to correspond to the description given of the Taplow Chalk. All this notwithstanding, we cannot produce so much as a single phosphatised foraminifer outside the limits of the old Taplow chalk-pit. Why this should be, and what should be our guide in the search for fresh deposits, are questions that await an answer.

From the accounts given of the Continental deposits, we learn that phosphatic beds are never continuous at any horizon in the Chalk for more than a short distance. Thus, in one of the latest

papers on the subject (2), we are told that of all the deposits yet discovered, even those with a cubic capacity of more than a million metres, every one has been proved to be lenticular in form. The greatest length of these lenticular beds rarely exceeds one kilometre (1,100 yards), and their breadth two or three hundred metres, while in thickness the phosphatic rock ranges from nothing at the edges of the lenticle to upwards of twenty metres in its central portions. This, then, is the probable explanation of the fact that even at Taplow the deposit has not been recognised beyond the pit and its immediate surroundings. It is, doubtless, of a lenticular form, and probably of no great extent. The other pits and sections in the neighbourhood, which are neither numerous nor conveniently situated, seem in the few cases where they expose the right zone in the Chalk to lie beyond the limits of the lenticle. What these limits are, and in what directions the lenticle extends can only be determined by borings or by tunnelling from the known outcrop, either of which operations would entail a greater expense than would be repaid by the working of the deposit in the present state of the phosphate market.

The study of a rock, however, by geologists is little influenced by its price per ton, and any new discoveries of this interesting variation of the ordinary type of Chalk will receive due attention. But where the next find will be made becomes more uncertain as time passes. The Belgian phosphate occurs at a higher zone in the Chalk than is known to exist in this country. The French deposits, on the other hand, though not strictly on one horizon, lie high up in the Upper Chalk of English geologists, and at or near the horizon of the Taplow Chalk. In our locality this horizon lies close beneath the Tertiary beds, but in other districts, and even in the immediate neighbourhood of Taplow, these strata rest upon other parts of the Upper Chalk, either higher or lower than that in which the phosphatic beds are situated. For the country generally, therefore, we require a surer guide than that afforded by the Tertiary boundary.

The Chalk at first sight presents so great a sameness from top to bottom that an unpractised observer would have little chance of distinguishing one portion of it from another. It can, however, be divided by lithological characters and fossil-contents into three generally well-marked subdivisions, known as the Upper, Middle, and Lower Chalk. The lower limit of the Upper Chalk is drawn at a nodular bed, not only conspicuous from its hardness and other peculiarities, but capable of being traced over a large part of the country, and of being laid down with accuracy on the Geological Survey maps; which, in fact, has already been done over considerable areas. In the Chalk Rock, therefore, we shall have a definite base-line from which to reckon upwards in a search for the zone where phosphatic chalk may be expected to occur. Some further assistance may be rendered by the existence of zones in the Upper

Chalk, characterised by certain fossils. On the Continent this subdivision of the Upper Chalk has been carried out with some completeness, but in England the boundaries of these zones are too indefinite for use by the geological map-maker. So far, therefore, as maps are concerned, we shall have to depend on the Chalk Rock as our principal guide in estimating our position in the Upper Chalk. At Taplow, the phosphatic zone is believed to lie about 250 feet above the Chalk Rock.

The much debated question as to the origin of the phosphatic chalk has not received much light from the most recent communications. We are asked to believe that the phosphoric acid rose from the rocks below the Chalk through hypothetical fissures, along anticlines which have not been proved to exist. Why, if the acid rose from below, should portions of the Upper Chalk have been acted on, and all the lower parts of the formation left unaltered? Nor, on the other hand, has any connection between the Tertiary beds and the phosphatised zones been established. At Taplow, twenty feet of ordinary non-phosphatic chalk intervenes between the Tertiary base and the highest phosphatic beds. We are driven, therefore, to the conclusion that the phosphoric acid is one of the original constituents of the rock in which it now occurs. We may infer, moreover, from the fact that foraminifera in the same condition of mineralisation as those which compose the phosphatic rock have been detected in flints in some of the foreign deposits, that the shells were phosphatised while they lay at the bottom of the chalk-sea, and probably soon after the death of the occupants.

MM. Renard and Cornet, in their exhaustive paper on the Continental phosphatic chalks (3), remark that the mode of occurrence of the granules points to their having been accumulated under the action of currents. The minute fragments of scales and bones, being of the same material and of about the same size as the phosphatised foraminifera, would be carried with them. The Chalk, in their opinion, is not a deep-sea deposit, but a sediment laid down in comparatively shallow water at no great distance from land; and these granules are such as could be transported a considerable distance by ordinary marine currents. Animal-life occurs in the greatest abundance near shores; and to the reptiles and fishes of the littoral zone these authors attribute the concentration of phosphatic material, and the subsequent phosphatisation of the microscopic organisms with which it was mingled.

The Taplow Chalk also is clearly a sediment such as might have been distributed by marine currents. To the naked eye, indeed, it much resembles a somewhat chalky sand. The theory that it was drifted from a distance to its present situation explains also the fact that the larger organisms, such as Belemnites and fragments of *Inoceramus*-shell, though embedded amongst phosphatic granules, are not themselves phosphatised. Yet the characteristically-shaped

prisms, yielded by the complete disintegration of the *Inoceramus*-shell, form separately a large proportion of the phosphatic granules. Hence, we may suppose that phosphatisation took place before the drifting of the materials, and that only those organisms which were originally small, or which had been broken down to the requisite size, were transported.

On the other hand, we find difficulty in supposing the Taplow Chalk to have been derived by any process of sifting from a coast-sediment. It is believed by many that dry land lay in the West of England during the Chalk Period; but, even if this were so, what sort of land can it have been that yielded neither mud nor mineral fragments, both of which are almost entirely absent from the Taplow Chalk? Even the finer constituent of the rock consists either of minute organisms or of fine powder derived from the breaking up of the larger shells; and from whatever direction it was drifted, it must have been from a region almost unreached by mineral sediments.

In considering the effects of a current on these granules we should remember that phosphate of lime has a greater specific gravity than the carbonate, and that, consequently, there would be a tendency for the unaltered granules to separate themselves out from a partially phosphatised assemblage of microscopic organisms. In a current of suitable velocity the phosphatic granules would be naturally concentrated, much as they are artificially in the washing of the phosphatic chalk for the market. If we suppose some such natural washing as this to have taken place, we need not imagine the granules to have travelled far, and can understand the absence of far-transported mineral fragments. We could also explain the existence of the abundant little coprolites, which have been noticed in the Taplow Chalk, on the supposition that, whether from a set of the tides or other cause, there were banks or hollows in the bottom of the Chalk-sea that were especially frequented by small fishes, to whose efforts was due the phosphatisation of the organic deposits of those particular spots. What fishes these were, and why their actions are observable in the Upper Chalk only, we have not yet ascertained.

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A. STRAHAN.

VI.

The Sequence of Plutonic Rocks.

THE fact that the lavas and ashes ejected in any one district vary in composition in the course of geological time has attracted the attention of those geologists who have devoted themselves to the study of volcanic regions from very early times, and attempts have been made to explain this variation by hypotheses as to the distribution of igneous magmas beneath the superficial crust of the globe. The chemical researches of Bunsen on the volcanic rocks of Iceland in 1851, led him to the view that the variations in composition could be explained by assuming the separate existence of two magmas, to which he applied the terms normal-pyroxenic and normal-trachytic. Any igneous rock would, on this theory, have the composition of one or other of the original magmas, or else an intermediate composition due to an admixture of the two magmas in some definite proportion. The mixture theory of Bunsen is probably not correct as an explanation of the origin of igneous rocks, but that it expresses with a very fair approximation to truth the variations in composition of volcanic rocks belonging to the basalt-, andesite-, and liparite-families has been established by hosts of analyses.

The "theory of the two magmas" is, however, usually associated with the name of Durocher, who, in his celebrated "Essai de Pétrologie Comparée," endeavoured to explain volcanic phenomena, and especially the sequence of volcanic rocks, by reference to two universal magmas which were supposed to form concentric spheres beneath the solid crust, and to be kept apart by reason of their difference in density. The upper and lighter magma was supposed to be practically identical with Bunsen's normal-trachytic magma, and the lower and heavier with his normal-pyroxenic magma. At the time when the theory was proposed it was commonly believed that the earlier eruptions were always of a trachytic, and the later of a basaltic character. This Durocher explained by supposing that the erupted material was at first drawn from the upper or trachytic layer, and finally, after this had been locally exhausted, from the under or basaltic layer. He recognised, however, that there were many facts which the theory as above stated could not account for, and these he endeavoured to explain by "liquation," by the accession of sea-water, and by the alteration of the magmas in course

of geological time. He also supposed that in some cases eruptions might take place from local basins in the earth's crust, and not from the universal concentric zones.

The theory of the two magmas possesses, at present, little more than an antiquarian interest. It was a bold attempt to solve, with wholly insufficient data, the problem of the sequence of volcanic rocks. The principal interest for us lies in that part of the paper dealing with what the author terms "liquation." He says:—"The magmas which have produced the igneous rocks are comparable to fused masses of several metals which separate in cooling into different alloys, according to the circumstances of solidification." Here we have a clear and distinct recognition of the important fact that a homogeneous magma may, in the process of cooling, become differentiated, and so give rise to igneous rocks of varying composition. This is the mode of viewing the matter which is at present receiving a considerable amount of attention.

The attentive study of any crystalline igneous rock is sufficient to convince one that the consolidation of a molten magma is accompanied by differentiation. The original magma was homogeneous; the resulting rock is heterogeneous. As the molten mass cooled molecular groups of magnetite, augite, felspar, and the like were built up, became locally concentrated, and finally coalesced to form solid crystals. The whole rock did not consolidate at once. The minerals commenced to form at different stages. There was a sequence in this process of crystal building. If, after consolidation had progressed to a certain extent, the crystals had been filtered away from the mother liquor, the two portions thus obtained would have been found to differ widely in composition from each other and from the original magma. Suppose the original magma to have had the composition of an andesite or diorite, then the two portions would differ from each other as an acid rock differs from a basic rock. The crystalline aggregate would be poor in silica, rich in lime, iron and magnesia, and poor in alkalies, especially potash; the mother liquor would be rich in silica, poor in lime, iron and magnesia, and rich in alkalies, especially potash. That this is no mere speculation, but an actual fact, can be proved by observing the order in which the minerals form, or by separating the ground-mass from the porphyritic crystals in some typical andesite, and determining its composition by direct analysis. Thus, the ground-mass of one of the Cheviot andesites has been shown by Petersen to have the composition of a quartz-felsite or liparite.

Differentiation by crystal-building in magmas of intermediate composition may therefore give rise to secondary magmas of increasing acidity. Are there any facts which such a differentiation would explain?

Again, the phenomena of crystal-building seem to imply that differentiation must precede the actual formation of a crystal.

There is probably a local concentration of molecules before a crystal is actually produced. This leads us to consider whether magmas may not become differentiated quite apart from the actual separation of crystals.

It follows, as a necessary consequence of Van't Hoff's theorem (4) relating to osmotic pressure, that if the different parts of a dilute solution are at different temperatures, the degree of concentration will vary inversely as the absolute temperature. Thus, to take a numerical example, the amounts of salt in a given mass corresponding to temperatures of 80° and 20° C., after equilibrium has been established, will be as

$$(273+20):(273+80)=293:353=1:1\cdot205.$$

In other words, a given weight of the colder portion will contain about one-fifth more salt than the same weight of the warmer portion. This had been experimentally established by Soret before Van't Hoff proved it to be a consequence of a more general law, and it may therefore still be conveniently referred to as Soret's principle. Seeing that igneous magmas are of the nature of solutions in which the mineral on the point of separating out may be supposed to be dissolved in the rest of the magma, we have every reason to believe that Soret's principle may have important geological applications.

The general order in which constituents separate out in magmas of intermediate composition is as follows. First, iron-ores, then the ferro-magnesian constituents, and, lastly, felspars and quartz. Among the felspars, the lime-soda members of the group precede oligoclase, orthoclase, and microcline. From this we see that the compounds liable to be concentrated by Soret's principle are, first, those of the iron-ores, and, secondly, those of the ferro-magnesian silicates. Later on we shall call attention to certain facts which receive a simple interpretation by the application of this principle. At present, however, we have to notice another cause which tends to destroy the homogeneity of a solution. Gouy and Chapéron, by applying a well-established principle of thermo-dynamics, have deduced the conclusion that a solution standing at rest under the influence of gravity will become more concentrated in the lower than in the upper part. It is difficult to verify this conclusion experimentally, because the height of the liquid column must be very great—something like 100 metres—in order that any sensible differences may be produced. Another cause which may operate to intensify the concentration started in some other way is magnetic attraction; at least this is suggested by Professor Vogt (4), although it cannot be said to rest on evidence as satisfactory as that which can be advanced in support of the principle of Soret or that of Gouy and Chapéron. The idea is that the molecules of ferriferous compounds are themselves liable to be affected by magnetic attractions, so that if a local accumulation of such molecules be produced in any way, this attraction will intensify the concentration and extend the space in which it is going on.

With these general and more or less disconnected remarks, let us now consider some cases in which a definite sequence of igneous rocks has been established. That we are very far from being able to explain all the facts must, of course, be admitted at once, but at the same time we may be allowed to remark that there are not wanting indications that some of these facts are due to the operation of those causes to which attention has been drawn.

We commenced this article by referring to volcanic rocks. Now the magmas from which such rocks are derived must either exist independently in the interior of the earth, or else be formed by differentiation from some pre-existing magma, or in some other and unknown way. In any case, seeing that plutonic rocks originate in the earth's interior, and represent, so to speak, the subterranean manifestations of volcanic energy, it seems probable that a careful study of the relations of complex plutonic areas will throw light on the origin of different magmas. It is possible that some plutonic areas may represent consolidated magma-basins that have never been tapped. At any rate, if there be anything in the differentiation theory, it is in plutonic masses that we shall find evidence in its support. In what follows we shall therefore deal almost exclusively with plutonic phenomena.

The first area to which we shall refer is the one which has been so exhaustively studied by Professor Brögger (1). Between Lake Mjösen on the N.N.E. of Christiania and the Langesundfjord on the S.S.W. is a tract of country, about 230 kilometres in length and varying from 35 to 70 kilometres in breadth, composed of lower Palæozoic strata and post-Silurian eruptive rocks. This vast area of about 10,000 square kilometres is separated from the surrounding Archæan rocks by a series of important faults. Sometimes there is one main fault, with a throw of over 1,000 metres, at other times there is a group of smaller faults. In all cases the effect of the faulting has been to depress the rocks within the area above mentioned. This sunken district has been the site of important igneous phenomena, and more than half of it is now covered by igneous rocks, some of which were poured out at the surface, while others were intruded in the form of huge laccolites amongst the sedimentary strata. There is every reason to believe that the sinking of this portion of the earth's surface and the intrusion and extrusion of igneous material are correlated phenomena; and this district may therefore be quoted in support of the opinion so largely held by the Vienna-school of geologists, that volcanic action is especially characteristic of sinking areas. The fossil-bearing strata attain a thickness of from 700 to 850 metres, and they represent an unbroken succession of Cambrian, Ordovician, and Silurian rocks. A grey or red sandstone, without fossils, probably representing the Old Red Sandstone, overlies the fossiliferous rocks conformably, and attains a thickness of 380 metres. The total thickness of the

Palæozoic formations is thus over 1,000 metres. The oldest igneous rocks are interstratified with the uppermost members of the Palæozoic sequence, and, therefore, probably belong to the Old Red Sandstone period. They are basic rocks of the diabase family, containing from 45 to 47 per cent. of silica. The plutonic rocks of the same age and character occur as small but well-characterised laccolites at Sölvsberg and Brandbokampen on the east side of the Randsfjord. The rock of the former locality is an olivine-diabase of granitic structure (gabbro-diabase), composed of plagioclase, olivine, violet pyroxene, biotite, and iron ores; that of the latter a coarse-grained proterobase (gabbro-proterobase) containing a basic plagioclase, pyroxene, brown basaltic hornblende, and biotite, together with idiomorphic titanite, magnetite, and pyrite. An apophysis from the proterobase of Brandbokampen shows a marked porphyritic structure. The phenocrysts are formed of brown hornblende and pyroxene; the groundmass of lath-shaped plagioclase, prisms of brown hornblende, and iron ores. This rock is regarded by the author as a typical camptonite. The area occupied by intrusive basic rocks is, however, very small compared with that covered by the sheets. The rocks of these sheets are variable in character, and are grouped under the terms augite-porphyrite, melaphyre and labrador-porphyrite.

The plutonic rocks of the second group include basic augite-syenites (laurvikite), mica-syenites, and nepheline-syenites (laurdalite). They are especially developed in the south-western portion of the district, and form the coast-line between Tönsberg and Langesund. The augite-syenite is a coarse-grained rock with a peculiar structure due to the abundance of a felspar which occurs in large individuals, and shows a marked tendency to idiomorphism, with development of the forms $\{110\}$ and $\{\bar{2}01\}$. These forms give rhombic sections in almost all directions. The other constituents occur in smaller individuals, and are wedged in between the conspicuous felspars. The constituents are soda-orthoclase or soda-microcline (anorthoclase), violet (titaniferous) or green (alkali-bearing) pyroxene, biotite, brown hornblende, olivine (usually but not invariably present), titaniferous magnetite or ilmenite, apatite, nepheline, and sodalite (both common, but only present in small quantity). For this special type of augite-syenite, which is distinguished from other types by the character of the felspar and the presence of nepheline and sodalite as accessory constituents, Brögger proposes the name of laurvikite.

The mica-syenites are closely related to the above, but the felspar is mostly oligoclase, and gives rectangular instead of rhombic sections. Biotite, as the name implies, is abundant.

The nepheline-syenites have the same structure, and are composed of the same minerals as laurvikite, but nepheline (elæolite) occurs as an essential constituent, and sodalite is often present. The texture is very coarse, and the individuals of nepheline are sometimes

as large as one's fist. Secondary products, after nepheline (cancrinite, analcime, natrolite, &c.), are very common. For this special type of rock the author proposes the term laurdalite.

The contact-facies of the rocks of this group is represented by the well-known rhomben-porphyry, and the same rock forms sheets which overlie the augite-porphyrites. Boulders of this rock transported by the ice of the glacial period may be found on our own coast near Cromer. The silica of the plutonic members of the second group ranges from 51 to 58 per cent., and the alkalies from 9·5 or 10·5 per cent. in the laurvikites, to as much as 16 per cent. in the laurdalites.

The third group is composed of quartziferous augite-syenites. These rocks have a typical granitic structure, and the felspars no longer show the rhombic sections so characteristic of most of the rocks belonging to the second group. They differ from the laurvikites in containing plagioclase as well as orthoclase, and in the abundance of brown biotite; also in the more pronounced idiomorphism of the green pyroxene, in the absence of nepheline, sodalite, and usually olivine, and in the almost constant presence of quartz. They occur in much smaller masses and have a much finer grain than the typical laurvikites. The author proposes the term akerite for rocks having the above characters. The silica ranges from 58 to 62·5 per cent.

The rocks which come next in point of time cover a large area between Christiania and Lake Mjösen. They are described by the author as red quartz-syenites, and the silica ranges from 60·45 to 66·39 per cent. The alkali percentage is still very high (12 to 13 per cent.), and the two alkalies are present in nearly equal proportions, soda, however, being slightly in excess. Felspar is by far the most important constituent. Orthoclase, oligoclase, and microcline are present. Microperthitic intergrowths of orthoclase and albite are common. The ferro-magnesian constituents include biotite, green pyroxene, ægirine, green hornblende, arfvedsonite, and sometimes glaucophane. Titanite, zircon, apatite, and iron-ores are accessory constituents. Quartz is always present, and in greater abundance than in the akerites. The structure is granitic; but a special feature is the occurrence of small angular drusy cavities, often lined with beautiful crystals of felspar, albite, quartz, ægirine, and titanite. The presence of the minerals ægirine, arfvedsonite, and glaucophane is evidently connected with the high percentage of soda. These rocks, like so many others from this remarkable district, are sufficiently peculiar to deserve a special name, and the author accordingly proposes to call them nordmarkites.

The rocks of the fifth group are termed soda-granites. They include hornblende-granite (rich in soda) arfvedsonite-granite, and ægirine-granite. The silica varies from 69 to 75 per cent. The dominant rock is a hornblende-granite rich in quartz. The horn-

blende is markedly idiomorphic, and often belongs to the arfvedsonite type. This type is frequently associated with a little glaucophane or ægirine. Biotite is much less developed in the rocks of this group than it is in those of the preceding and succeeding groups, and it is frequently absent altogether. The felspar is often microcline, with which albite is associated in microperthitic intergrowths. Oligoclase is also present. Quartz is always present in considerable abundance. The accessory minerals include titanite, zircon, orthite, magnetite, and pyrite. The structure is that of a medium-grained granite. The drusy or miarolitic cavities so common in nordmarkite are found also in the rocks of this group.

About half-way between Lake Mjösen and the Langesundfjord is an area of 700 square kilometres occupied by normal biotite-granite (granitite). In all the preceding rocks soda is in excess of potash, although in some cases the difference in amount of the two alkalies is only slight. In the rocks of this, the sixth group, the potash is in excess, and we have to deal with normal granites, such as are widely distributed over the earth's surface. The silica varies from 75 to 77 per cent. The constituents, mentioned in the order of their formation, are apatite, zircon, magnetite, titanite, biotite, felspar (orthoclase, microperthite, oligoclase), and quartz. Fluorspar and albite occur in drusy cavities; and epidote and chlorite are amongst the secondary products. This granite occurs in laccolitic masses, and is the latest rock of the first great series of eruptions.

The only igneous rocks which remain to be considered occur as thin vertical dykes. They are extremely abundant, and as a rule vary from a half to three metres in thickness, very rarely reaching ten metres. They are basic in composition, and are grouped by the author under the terms diabase, diabase-porphyrite, proterobase, and proterobase-porphyrite. Amygdaloidal structures are common.

Leaving these basic dykes out of account, we have in the first six groups a continuous series of eruptive rocks; that is, a series connected by intermediate forms. The oldest basic rocks pass into the laurvikites, and these into the laurdalites. Again, from the laurvikites we pass through the mica-syenites, akerites, nordmarkites, and soda-granites to the normal granitites. This petrographical sequence corresponds in the closest manner with the geological sequence; in other words with the order in which the rocks have been formed. The oldest rocks are the most basic, and the youngest rocks, excluding the dykes, are the most acid. Between the two extremes we have a continuous series. The volcanic series is not so complete as the plutonic, but so far as it goes the sequence is the same. The fact that most of the rocks are of a peculiar type clearly indicates that they must have originated in some enclosed magma-basin. It is quite impossible to explain their local character and the chronological sequence of the different types on the assumption that the rocks are the result of the consolidation of material derived from

any universal concentric zones. The distinguishing character of the local basin from which these rocks were derived was its extraordinary richness in soda. The author attributes the petrological sequence to the differentiation of an originally homogeneous magma, and refers especially to Soret's principle and to the effects of successive crystallisation as possible causes of this differentiation.

The facts above described may be roughly grouped together by the following hypothesis. A magma-basin existed beneath the Christiania district. During the Cambrian, Ordovician, Silurian, and part at least of the Devonian periods, a sea covered the area, and sedimentary deposits attaining a thickness of about 4,000 ft. were laid down. Then the first faulting commenced, and the district began to subside into the underlying magma-basin. Basic rocks were intruded and extruded. The faulting and subsidence were continued at intervals, and magmas of increasing acidity were successively squeezed out until the great granitic laccolites were formed. Then, finally, numerous cracks were formed, and basic material injected into them.

The phenomena seen along the south-west margin of the laurvikite district, that is, in the immediate neighbourhood of the great boundary faults of the Langesundfjord, have such an important bearing on the origin of certain structures in gneisses and schists that some reference to them seems desirable, although they do not bear on the subjects we are especially considering.

Along the north-eastern margin of the district in question, where the consolidation of the laurvikite-magma was not accompanied by movement, a massive rock, closely resembling the rhomben-porphyry of the sheets, was produced. Along the south-western margin the rhomben-porphyry type of rock is entirely absent. The contact phenomena are of a much more complicated character. The coarse-grained massive laurvikite is here intimately associated with a finer grained rock, which is usually striped or banded, and sometimes even schistose. For this rock, which is somewhat richer in nepheline and sodalite, and possesses a more uniformly granular structure than the laurvikite, the author proposes the general term ditroite.

In many places, and especially in the islands at the entrance of the Langesundfjord, the fine-grained banded ditroite and the coarse-grained laurvikite are blended in such a way as to suggest that they are merely "schliere" of slightly different composition. This, however, is not the case, for closer examination proves that the laurvikite was solid when the ditroite was intruded. The foliation of the ditroite is a kind of fluidal structure. Differential movement arranged the darker constituents in stripes parallel to the bounding surfaces, and fractured the larger constituents, especially the felspar and nepheline. Moreover, the angles of these constituents were often rounded off by the same movement, so that after final consolidation a rock with a typical primary augen-structure was

produced. The microscope confirms these conclusions, and shows that crystals have not only been rounded, but also reduced to the condition of fine detritus. It is clear, therefore, that when the ditroite was pressed upwards it consisted of a magma crowded with already formed crystals. The author proposes the term protoclastic for structures of the above kind, to distinguish them from the cataclastic structures of Professor Kjerulf, which are due to the deformation of a rock subsequent to final consolidation.

In the immediate neighbourhood of the junction the laurvikite has frequently been so broken up as to constitute a true breccia. Lenses of laurvikite measuring from a half to one metre across, sometimes smaller, however, and sometimes much larger, lie in a matrix of ditroite. This remarkable complex appears to have been formed in the following way. After the laurvikite had been consolidated it became broken up into large and small fragments by a renewal of the sinking process along the junction, and, at the same time, the ditroite-magma, with its already formed crystals, was intruded. The fragments of the laurvikite became rounded, and the ditroite-magma caught up the detritus of the rock into which it was injected. The fluidal structure in the ditroite is related to the lenticles of laurvikite exactly in the same way as the fluidal structure in a rhyolite is to the phenocrysts of felspar and quartz. If we use the term rhyolite in its etymological sense, we may speak of the complex rock here referred to as a rhyolitic breccia. There is no reason to believe that any important interval of time elapsed between the consolidation of the laurvikite and the intrusion of the ditroite; indeed, the occasional blending of the two rocks seems to imply that the laurvikite had sometimes scarcely solidified before the intrusion took place. The figures and descriptions of the author show clearly that the phenomena here referred to are exactly similar to those which may so often be seen in any extensive gneissose region.

In the same district pegmatitic injections may also be observed, and this still further increases the resemblance to gneissose areas. The author remarks that the phenomena above referred to can scarcely be described by the term structure, inasmuch as we are here dealing with the mutual relations of rock-masses and not with the mutual relations of minerals. He accordingly proposes that the term architecture should be employed instead.

The laurvikite, however, is not the only rock which occurs as inclusions along the south-west margin of the plutonic area. Lenticular masses of augite-porphyrite and rhomben-porphyry are also found. In one locality fragments of augite-porphyrite are surrounded partly by pegmatitic syenite and partly by a very dark variety of schistose ditroite, which is termed by the author ægirine-ditroite-schist. This basic ditroite forms a narrow zone, about half a metre broad, round the lenticles of augite-porphyrite, and, therefore, probably owes its basic character to the absorption of certain

constituents, especially iron-oxide and lime, from the included rock. The lenticles of rhomben-porphyry show very interesting structural features in their external portions. In the interior they are massive, and the large porphyritic felspars give the characteristic rhombic sections; near the margins they are foliated, and the porphyritic felspars have been deformed into long narrow lenticular folia. The structural modification is exactly similar to that which would be produced by dynamo-metamorphism of a regional character. Here it is associated with contact-metamorphism and is a purely local phenomenon.

We must now return to the consideration of the mutual relations of the rocks of plutonic masses. The general question of the differentiation of plutonic magmas has been discussed in a recent paper by another Scandinavian geologist, Professor Vogt (4). The immediate object of this paper is to explain the origin of certain ore-deposits, and especially those of the Ekersund district in Norway, and of Taberg in Sweden. The one in the former case is ilmenite, and in the latter titano-magnetite. In both cases the ores are intimately associated with basic eruptive rocks, in which the same minerals occur as accessory constituents; but the actual relations of the ore-deposits to the surrounding rocks are not the same in the two cases. At Taberg the ore occurs in the centre of a mass of olivine-hyperite into which it passes by insensible gradations. The ore itself contains a small quantity of olivine and felspar. As the rock is traced outwards, felspar increases in quantity, and pyroxene makes its appearance. Thus it passes into a hyperite rich in magnetite and olivine, and finally into normal olivine-hyperite. Such relations clearly imply that the ore-deposit has been formed by a local concentration of ferriferous molecules in a plutonic magma, or else by the local accumulation of magnetite grains or crystals after separation. The author inclines to the former hypothesis.

The Ekersund ilmenite deposits form dyke-like masses in a rock mainly composed of labradorite. The other rocks in the same eruptive district are hypersthene- and biotite-norites, and enstatite granite. The relative ages of the different plutonic masses have not yet been determined, but the ore-deposits appear to be distinctly later than the surrounding labradorite-rock, for they occasionally contain included fragments. Microscopic examination of the mutual relations of the individual constituents of the ore-deposit does not support the view that it has been formed by the mechanical accumulation of solid crystals or grains of ilmenite, and the author concludes that the concentration was produced in the magma without the actual separation of minerals. In other words, he supposes that a magma having the composition of the ore was produced by diffusion processes such as those referred to at the commencement of this article. As illustrating the nature of these diffusion processes, he refers to a remarkable dyke occurring near Huk in the Christiania

district. This dyke is ten metres thick. The centre is composed of porphyritic crystals of orthoclase, and a fine-grained matrix of biotite, orthoclase, plagioclase and quartz. Pyrite, magnetite and apatite occur as accessory constituents. As the border is approached, magnetite, pyrite and biotite increase, plagioclase takes the place of orthoclase, and the rock becomes finer in grain and darker in colour. Quartz is entirely absent from the marginal rock. The rock of the centre contains 61·71 per cent. of silica, and is a porphyritic mica-syenite; that of the margin contains 43·33 per cent. of silica, and is a basic kersantite. A narrow vein runs off from the main dyke at one point. The rock of this vein becomes more and more basic as it is traced away from the junction, and at the termination contains 15 per cent. of magnetite. There is no evidence whatever of successive eruptions in this dyke. The transition from the acid to the basic rock is perfectly gradual. The minerals are concentrated in the order of their formation, and the phenomena are, therefore, capable of explanation by Soret's principle. Seeing that the basic portions of the dyke are vertical, that is, parallel with dyke-walls, it is impossible to explain the differentiation by the local accumulation of the first-formed crystals. Magmatic differentiation appears to be necessitated by the facts of the case.

Somewhat similar phenomena have been observed by Professor Lawson (3) in the basic dykes of the Rainy Lake region of Canada. The differences are not so strongly pronounced, but they are of the same general character. Thus, in five dykes, the percentage of silica in the rock from the centre was found to be in excess of that in the rock from the margin. In the White-fish Bay dyke the marginal rock contained 47·5 per cent., and the rock from the centre 52·47 per cent. Along with the increase in silica there is a decrease in lime, iron, and magnesia, and an increase in alkalies. The texture of the marginal rock is trachytic, that of the intermediate rock ophitic, and that of the centre granitoid.

Mr. Iddings has described a set of related plutonic and volcanic rocks (2) occurring in the Yellowstone National Park. The sequence in both cases is the same, and the latest rocks are the most acid. Thus, referring only to the intrusive rocks, occurring as dykes and stocks, pyroxene-porphyrites with a holocrystalline ground-mass pass into, and are succeeded by, pyroxene- and hornblende-diorites, these, again, by hornblende-biotite-diorites, and these by porphyritic quartz-biotite-diorites. As the author remarks, the whole series is a single, irregularly interrupted succession of outbursts of magma that gradually changed its composition and character. Eleven analyses are given, showing a gradual increase in silica from 56·28 per cent. to 69·24 per cent.

The writer of the present article, in conjunction with Mr. Dakyns, has recently described an area of plutonic rocks occurring near the head of Loch Lomond (5). The oldest rocks of the complex are

peridotites, and the newest are eurites, composed almost entirely of quartz and orthoclase. Between these two extremes occur augite-diorites, diorites, quartz-diorites, and biotite-granites. The silica ranges from 38·6 per cent. to 75·8 per cent. It seems impossible to avoid the conclusion that this complex owes its origin to the differentiation of an originally homogeneous magma and that the cause of the differentiation is to be sought for in the order in which the minerals successively form in igneous magmas.

So far we have been mainly concerned with large areas of plutonic rock; but intrusive masses of no great size often show a want of uniformity in composition such as can only be accounted for by differentiation before or during consolidation. As illustrations we may mention the felsitic and granophyric patches and veins in the Carboniferous dolerites of Scotland, as, for example, those in the neighbourhood of Stirling, and the corresponding phenomena in many mica-traps. A rude kind of cellular structure may often be observed in the latter rocks, the cell-contents being richer in ferro-magnesian constituents than the cell-walls.

We see, therefore, in this, as in so many other branches of geology, that illustrations of the principles which enable us to explain the structure of large areas may be observed in hand specimens or even microscopic slides. If the differentiation theory be correct, the micro-pegmatitic interstitial matter in an augite-diorite has originated in the same way as the mountain-making granophyres of the Inner Hebrides. The facts to which reference has been made appear to show that a more or less gradual sequence from basic to acid rocks is of very frequent occurrence, and that such a sequence can be correlated with the order in which minerals are known to form in igneous magmas.

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VII.

The Grammar of Science.

ON first glancing at the pages of Professor Karl Pearson's new book[1] the biologist might be tempted to suppose that, apart from the chapter on Life, there was little therein with any bearing on his special studies. He might be inclined to regard it as a mixture of physics and metaphysics. And such, indeed, it is in part. I am not using the word metaphysics as a term of reproach. By metaphysics I mean a discussion of the ultimate nature of the phenomenal world and of the conscious Ego. Physics as such accepts the phenomenal world as external to, and for its purposes independent of, the mind of the investigator. It leaves to psychology the analysis of the human mind, and to metaphysics the perhaps insoluble question of the ultimate nature of the Ego and of the external occasion of its perceptions. Professor Pearson does not enter into the metaphysics of the Ego. He is content naïvely to accept the investigating mind as he finds it. But he is not content to accept with equal *naïveté* the phenomenal world; he enquires what it really is, and how we come to perceive it and to reason about it. He is forced to a position which is largely idealistic. The physical world is in a sense the product of the percipient mind, and Mr. Pearson contends that this is a fact which we must not only not lose sight of, but must introduce into all our physical considerations. But biology and geology are sciences which investigate peculiarly complex and intricate manifestations in the physical and phenomenal world; and what Mr. Pearson says of physics therefore applies in large degree to these sciences also. I propose, therefore, to endeavour to explain Mr. Pearson's position, and to offer some criticisms thereon.

In an introductory chapter Mr. Pearson vindicates the right of Science to claim the whole sphere of the knowable as its province. Over this we need not linger. That which follows, however, on The Facts of Science, demands our closer attention.

Here it is pointed out that, in the perception of a so-called "external object," all we are justified in predicating is that we experience certain sense-impressions which suggest the given percept. Thus, a retinal impression may suggest the percept "book," to the making of which there goes a good deal more than the simple

[1] The Grammar of Science. By Karl Pearson, M.A., Sir Thomas Gresham's Professor of Geometry. *Contemporary Science Series.* London: Walter Scott, 1892.

sense-impression. Since, therefore, the percept contains elements not given in the immediate sense-impression, but contributed from the stored sense-impresses of the percipient mind, we may say that it is, in part at least, and often to a large extent, *constructed* by the percipient in the moment of perception. The polished Ammonite which I use as a letter-weight is a percept suggested by certain retinal sense-impressions. As an object, it is resisting to the touch, heavy, and so forth. I can readily verify the reality of the object in these respects through the appropriate channels of tactual and muscular sense-impression. It is, moreover, a reality not only for me, but for any normal human being who may chance to be by my side. As a "construct" it is not only individual, but social: a perceptual product not only of me, but of man. Such, in brief, are the conclusions which result from the psychological analysis of the process of perception, and which are accepted and endorsed by Mr. Pearson. Here psychology as a science is content to leave the matter. But if we go further and enquire whether there is an external "reality" which is the occasion of my percept and that of my neighbour, we enter upon a metaphysical problem. We must note that the word "reality" is here used in a sense different from that in which I used the word *real* above. There I meant by *real* common to me, you, and all normally constituted human beings. Here by "reality" is meant an existence independent of human perception.

Let us hear what Mr. Pearson says with regard to this "reality." "Turn the problem round, and ponder over it as we may," he says, "beyond the sense-impressions . . . we cannot get. Of what is beyond them, of 'things-in-themselves' . . . we can know but one characteristic, and this we can only describe as a capacity for producing sense-impressions . . . This is the sole scientific statement which can be made with regard to what lies beyond sense-impression." Even this he qualifies, if he does not nullify, a little later, when he says: "There is no necessity, not even logic, in the statement that behind sense-impressions there are 'things-in-themselves' *producing* sense-impressions." If I understand him rightly, Mr. Pearson's view is that the very utmost we can predicate concerning the occasion of sense-impressions is its existence; of its "real" nature we can assert nothing, not even its externality to the percipient. Presumably, however, he would not deny that there is something about it which we *symbolise* as externality or outness from ourselves as percipients.

Before leaving this question of perception, or the "construction" of external objects, there is one further remark I have to offer. We may agree with the result of the analysis, and yet we may hold that its constant obtrusion in the consideration of the phenomena of what *for perception* is the external world, is likely to lead rather to the confusion of than to the simplification of our scientific treatment. These conceptual results of the analysis of the perceptual process are on a

different plane of investigation from that which deals with the phenomena themselves. And the introduction of results obtained on one plane of investigation among the results obtained on other planes is apt to be confusing, if not misleading.

We pass now from the object to the "eject." To some of our readers the term "eject" may be unfamiliar. Suppose that my neighbour and I are both looking at some object, say my Ammonite letter-weight. I have a percept; and I have every reason to suppose that he too has a similar percept. But his percept can never be an object to me; nor my percept an object to him. His states of consciousness are for me, as mine are for him, in Clifford's phrase "ejects." Clifford himself puts it shortly thus: "When I come to the conclusion that *you* are conscious, and that there are objects in your consciousness similar to those in mine, I am not inferring any actual or possible feelings of my own, but *your* feelings, which are not, and cannot by any possibility become, objects in my consciousness." This, of course, was no new discovery of Clifford's, but he coined the convenient word "eject," by which each of us may denote our neighbour's consciousness. Whether we use Clifford's word or not matters comparatively little; but few deny the fact which suggested to him the necessity for such a word, namely, that my neighbour's consciousness can never be to me an object of perception or produce in me a direct sense-impression. That is reserved for Professor Pearson. "To this [the statement I have quoted from Clifford] it may be replied," he says, "that were our physiological knowledge and surgical manipulation sufficiently complete, it is conceivable that it would be possible for me to be conscious of your feelings, to recognise your consciousness as a direct sense-impression; let us say, for example, by connecting the *cortex* of your brain with that of mine through a suitable commissure of nerve-substance." Mr. Pearson refutes himself, however, on the very next page, when he says: "Psychical effects are, without doubt, excited by physical action, and our only assumption is the not unreasonable one that a suitable physical link might transfer an appreciation of psychical activity from one psychical centre to another." In other words, the recognition of one's neighbour's consciousness, in the supposed case, is not direct but through the intervention of a physical nexus. But there is no difference in principle between the excitation of the sensorium through such a supposed *physical link* and the excitation of the sensorium through the known physical links involved in oral communication. No one questions that the molecular vibrations of one brain may, through physical links, give rise to similar molecular vibrations in another brain, and that the consciousness which accompanies the one may run parallel with the consciousness which accompanies the other. But this is something wholly different from my recognising your consciousness "as a direct sense-impression."

In the next chapter, on The Scientific Law, a distinction is drawn, first, between the laws of nature and civil laws, and, secondly, between *natural law*, descriptive of the normal order of sequence in perception, and *scientific law*, the product of our conceptual thought as exercised on the materials given through the normal routine of perception. This latter distinction answers fairly well to that between what Mr. Herbert Spencer calls the laws of phenomena and the laws of the factors of phenomena. The former are general but concrete; the latter general and abstract. We must, however, pass over this chapter and that which follows (on Cause and Effect, and Probability) without further comment, though we could break a lance with Mr. Pearson on the grounds of his statement that "among the myriad planetary systems we see on a clear night, there surely must be myriad planets which have reached our own stage of development, and teem, or have teemed, with human life." It may be so. But seeing how enormously complex are the influences which have conspired to bring about man as the crowning product of evolution on this earth, we should judge it to be indefinitely improbable that human beings, as such, have been evolved on any one of these myriad planets.

The succeeding chapter on Space and Time is an important one. Space is described as the mode in which we perceive co-existing things apart, while time is the mode in which we perceive successive things apart. Mr. Pearson illustrates this Kantian contention by an analogy. The letters of the alphabet may be said to have a real existence, like the groups of sense-impressions we term objects; but the *order* of the letters is merely the mode in which we perceive them to co-exist as an alphabet. Space, in like manner, is an order or mode of perceiving objects; but it has no more existence if objects are withdrawn than the alphabet would have if there were no letters. So, too, with time. "Let the reader endeavour to realise empty time, or time with no sequence of events, and he will soon be ready to grant that time is a mode of his own perception, and is limited by the contents of his experience." Now, it may at once be granted that, in the absence of the sense-impressions which, for example, now suggest to me the percept which I call my clock, I could not recognise it as an object extended and distant; and that in the absence of the ticking sounds I could not recognise them as successive. But surely that is not the question. The question is whether, given certain sense-impressions which suggest the percept clock, there are any objective relationships which I can describe by using the adjectives distant, extended, and successive. I am prepared to maintain that the relationships which I thus describe are just as objective as are the sense-impressions themselves. Neither they nor the sense-impressions have any existence, *as such*, in the absence of the percipient mind. But whatever objective existence may be predicated of the sense-impressions, may be predicated also of their

spatial and temporal relationships. I know no more than Professor Pearson does of the nature of things-in-themselves, or ding-an-siches, or suchlike; but I do, as man, know somewhat of the world of phenomena, which is, for perception, external to me, and has extension, and in which my clock is ticking in rhythmic sequence; and I maintain that, *for perception*, the relationships of these objects are just as much external to me (and just as *little*, be it noted, in conceptual analysis) as are the objects themselves. I venture to think, therefore, that to regard space and time as "modes of perception" is, to say the least of it, inconvenient when we are dealing with the field of practical perception. Physics, as a science, is wise, I take it, in dealing with space and time in frankly objective terms, and I think the biologist may still discuss the distribution of organisms in space, and the geologist their distribution in time, without pausing to remind their readers that after all they are only dealing with sense-impressions, and stored sense-impressions, and certain forms of perception. All this may be true enough, but it is out of place either in physics or biology.

I may here devote a short paragraph to the antinomy, or seeming contradiction, to which we are introduced in this chapter through a consideration of geometrical boundaries and the atomic constitution of matter. In geometry a plane surface is regarded as absolutely continuous: in molecular physics as composed of discontinuous molecules. These views seem to clash. "The conception atom, when applied to our perceptions, is opposed to the conception of surface as the continuous boundary of a body. We have here," says Mr. Pearson, "an important example of what is not an uncommon occurrence in science, namely, two conceptions which cannot both correspond to realities in the perceptual world. Either perceptual bodies have continuous boundaries, and the atomic theory has no perceptual validity; or, conversely, bodies have an atomic structure, and geometrical surfaces are perceptually impossible. At first sight this result might appear to the reader to involve a contradiction between geometry and physics; it might seem that either physical or geometrical conceptions must be false. But the whole difficulty really lies in the habit we have formed of considering bodies as objective realities, unconditioned by our perceptive faculty." I should myself explain the apparent difficulty in a somewhat different way. Scientific conceptions are general and abstract, and are reached, as Mr. Pearson well points out, by idealising beyond the limits of perceptual experience. Each investigator neglects, or intentionally excludes, such elements of perceptual experience as are disturbing or unnecessary for his special enquiry. He then idealises, or conceives in absolute perfection—a perfection far beyond the limits of experimental verification—the results he obtains. If these different investigators carry this process along different lines, each excluding the element the other is idealising, the results will not

only be divergent from each other, but divergent from the perceptual experience from which each started. This *divergent abstraction*, as we may term it, may be illustrated by an analogy. Two investigators study the same substance, say carbon disulphide, a dense, colourless liquid. One abstracts the sulphur and formulates its properties, neglecting the carbon: the other neglects the sulphur and investigates the carbon, formulating its properties. The resulting properties in the two cases are different, or even contradictory. One obtains a yellow solid with these properties; the other a black solid with those properties; and the phenomenal substance with which each started is neither the one nor the other, but a colourless liquid! So the geometrician obtains a geometrical conception of continuous surface, neglecting atomic structure: and the physicist obtains a molecular conception of discrete particles. But the perceptual conception, *as such*, has neither a continuous boundary *as idealised in geometry*, nor a molecular constitution *as idealised in physics*. And to predicate these idealised conceptions of "perceptual bodies," therefore, leads to antinomy and confusion; the moral of which is that we must not introduce among the concepts of one line of investigation the concepts which result from another line of investigation.

The next chapter, on The Geometry of Motion, starts by describing motion as "the *mixed* mode under which all perception takes place." This way of regarding the problems of motion is the natural outcome of the conception of space and time as modes of perception. How far the discussion of the geometry of motion in this chapter, and of Matter and the Laws of Motion in succeeding chapters, will be satisfactory to specialists in physics, it is not for me to say. Regarding the matter merely from the general standpoint of natural science, I must confess that it appears to me more simple, and more scientifically accurate, to regard matter and motion as frankly objective and (perceptually) external. This is, however, not Professor Pearson's view. "If we confine ourselves," he says, "to the field of logical inference, we see in the phenomenal universe not matter in motion, but sense-impressions and changes of sense-impressions, co-existence and sequence, correlation and routine." Now although direct perception does not give us abstract matter in ideal motion, it does most certainly give us particular moving things, animals and men going from place to place, unsupported bodies falling towards the earth, the balloon ascending through the air, the clouds drifting across the face of the moon. All these are for adult perception occurrences in the external phenomenal world. If the psychologist, as the result of his analytic thought, reaches the conception of "sense-impressions and changes of sense-impressions," well and good; but this does not alter one whit the fact that for perception the movements we observe are occurrences in the world of phenomena. It is perfectly true that the sense-impression is that which starts the perceptual process and calls the percept into being. But it is equally true that the sense-

impression is not given, as such, in the percept but is only disclosed by psychological analysis. The sense-impression is that which remains when we have stripped from the percept all that gives it perceptual validity. If the phenomenal universe, therefore, is the world of perception, I am disposed to reverse Professor Pearson's statement, and to say that "we see in the phenomenal universe not sense-impressions and changes of sense-impressions but objective bodies in objective motion."

In a word, the mistake which Mr. Pearson appears to me to make is that he attempts to combine the abstract results of psychological analysis with the abstract results of physical analysis. And since the results of these two modes of enquiry are reached by what I indicated above as divergent abstraction, they do not readily coalesce. The better plan is, I think, to keep the results separate, just as the geometrician and the molecular physicist keep separate their conception of continuous surface on the one hand, and discrete particles on the other.

Passing to the chapter on Life, we find that Mr. Pearson wisely abandons the attempt to give a brief definition of the distinction between living and lifeless, holding that consciousness and the phenomena of the will give us little or no assistance in the matter. "The distinction between the inorganic and the organic," he says, "cannot be defined by saying that the one is mechanical and the other is not. We are ultimately obliged, in order to define life, to take secondary characteristics—to describe the structure by which we conceptualise the organic corpuscle, the motions which are peculiar to it, and the environment in which alone we perceive life to exist." Some of these characteristics are then enumerated. They suffice, it is held, to mark off the organic from the inorganic, "and the distinction thus drawn appears to be absolutely rigid. There is, at the present time, so far as we know, no generation of living from lifeless substance." By saying, however, that the distinction is absolutely rigid, and that there is no generation of living from lifeless substance, Mr. Pearson does not mean to exclude the passage of the non-living into the living at some period of evolution. Nay, further, he expressly adopts this view. He indicates the three possible hypotheses with regard to the origin of life on this earth; first, that life might have arisen from the operation of some ultra-scientific cause (special creation); secondly, that its germs might have been introduced in the crevice of a meteorite; thirdly, that living matter might have been generated by a special union of inorganic corpuscles under a suitable environment (spontaneous generation), and he adopts that hypothesis which we have placed last.

With respect to the origin of consciousness, Mr. Pearson is not very explicit; but he somewhat scornfully rejects the view, held in some form by several thinkers, and recently urged by the present

writer,[2] that the consciousness that we know has been developed from something which is of the same nature as consciousness, but which has not yet risen to the level of consciousness. This infra-consciousness I hold to be associated with all forms of energy. Only when the manifestations of energy reach the complexity of the molecular vibration in nerve-centres does the infra-consciousness rise to the level of consciousness. Mr. Pearson suggests that we might as well say that there is in matter "something-which-is-not-yet-life-but-which-may-develop-into-life" and call it *supermateriality*. He has, however, I venture to think, missed the point of my contention. He objects that there is no sense-impression of such infra-consciousness. Of course not. I hold with most philosophers that consciousness is something of a wholly different kind from the energy through which we receive sense-impressions. If Professor Pearson holds that some mode of material existence or of energy has been developed into consciousness, let him adduce some satisfactory, or at least plausible, grounds for this belief. If not, let him tell us from what it has developed. Either consciousness has been reached by a process of mental evolution, or it has been specially created. Mr. Pearson will not, I take it, adopt the latter alternative. But if he accepts the former, I would ask him to inform us from what it has been developed. I venture to think that the conception of infra-consciousness co-extensive with energy is arrived at by the application of principles laid down by Mr. Pearson himself, namely, the carrying to a conceptual limit of that which is suggested in the sphere of the perceptual and ejective world. In any case, the analogy with life is beside the mark; for I hold with Professor Pearson that the distinction between the inorganic and the organic cannot be defined by saying that the one is mechanical and the other is not. From the inorganic to the organic we can conceivably pass by successive advances in complexity of structure and of associated modes of energy; but by no such advances can we conceivably reach consciousness, if, as I hold, consciousness is not mechanical, and is neither a mode of matter nor of energy. From what, then, has consciousness been evolved, if not from infra-consciousness associated with lower modes of energy?

In conclusion (for we must pass over the last chapter, on The Classification of the Sciences, without comment) I may say that the problems discussed with great ability and lucidity, and often in a most suggestive manner, by Professor Pearson, are such as should interest *all* students of natural science. Modern biology and geology are full of physical problems, and the students of these sciences must have some acquaintance with such physical conceptions as Mr. Pearson discusses. They should also have some acquaintance with the wider problems of the general philosophy of science. The central

[2] *Animal Life and Intelligence*, p. 467, and more recently in the *Monist* for January, 1892.

question raised by the *Grammar of Science* seems to be this: Are we, in biology, geology, and physical science, to abandon the frankly objective mode of regarding the phenomena we have to study, and to frame our descriptions and definitions in accordance with the idealistic results of psychology and philosophy? Fully impressed as I am with the general truth and value of these idealistic results in their proper place, I hold that their introduction into the objective study of nature is misleading, and tends to confusion. I advocate, therefore, the retention of what I have termed the frankly objective attitude, which is the attitude now assumed by students of natural science, and which even the eloquence and clear logical treatment (and it is both eloquent and logical) of the *Grammar of Science* will, I believe, fail to reverse.

C. LLOYD MORGAN.

SOME NEW BOOKS.

ISLAND LIFE: or the Phenomena and Causes of Insular Faunas and Floras, including a Revision and attempted Solution of the Problem of Geological Climates. By Alfred Russel Wallace. 2nd edition. 8vo. Pp. 563. London: Macmillan and Co., 1892. Price 6s.

WE can now congratulate our readers on the appearance of a second edition of this well-known book. It is, of course, difficult to criticise a volume by such a master of the subject as Mr. Wallace; but, notwithstanding this, we feel that it is due to naturalists, who will hope for a third edition, to point out in what respects "Island Life" might possibly be improved. For the general plan and execution of the book we have nothing but praise, and we must thank the publishers for the reduction of the volume to a more handy size than that adopted for the first edition. Those who often use a book of reference such as this, are well aware that weight is a serious consideration, and that it is an undoubted advantage to have the same material in a smaller compass, especially when there is no loss of legibility through the reduction.

In the new edition of his work, Mr. Wallace has most carefully revised the accounts of the natural history of the various islands described, for eleven years have added considerably to our knowledge of insular faunas. Turning first to Chapter XVI. (the British Isles), as referring to a district especially interesting to home-staying naturalists, we cannot help feeling that Mr. Wallace, with his extensive experience of tropical islands and their wonderful endemic faunas, is, perhaps, a little inclined to over-estimate the evidence in favour of the existence of various endemic species in Britain. No doubt we have a few such; but that fifteen species of freshwater fishes are peculiar to our islands we should be inclined to question, especially when we remember the remarks made by Dr. Francis Day on many of these so-called species. The passage (pp. 341–344) relating to the various forms of *Salmo* and *Corregonus* remains as it stood in the first edition, but some reference should have been made to Dr. Day's opinion. Nowhere, however, except in the present volume, can we find a list of all the animals and plants peculiar to Britain, and this alone would make it a useful book of reference, whatever may be our opinion as to the validity of certain of the species.

The large additions which have recently been made to our knowledge of the fauna of Borneo, Japan, Madagascar, the Galapagos, and the Sandwich Islands, are all embodied in the present edition. We should like, however, to have seen a fuller account of the Malay Archipelago, and of various islands in temperate and arctic seas, even if to introduce these, without increasing the bulk of the book, it had been necessary to omit the chapters on Glacial Epochs. Chapters VII. to X. seem to us to be the least valuable parts of the book, and we cannot help feeling that the author's position is often considerably weakened by his excursions into the

very speculative subject of the astronomical causes of changes of climate. No doubt we have abundant evidence of one "Glacial Epoch," apparently with a mild interval, and, so far, the naturalist is entitled to speculate on the influence of climatic changes on the migration of animals and plants. But when he hears of alternating cold and warm periods, extending throughout geological time, the geologist feels that the facts, as far as he has yet been able to ascertain them, do not yield clear evidence of any cold period older than that of Pleistocene date. When a book on "Island Life" contains a section on the "State of the Planet Mars as bearing on the Theory of Excentricity as a Cause of Glacial Epochs," one cannot help feeling that the author is perhaps wandering a little from the subject.

One other request we should like to make to Mr. Wallace, in view of a third edition. Much has lately been learnt as to the means by which animals and plants are transported across the sea, and valuable accounts have been published of the re-establishment of life on Krakatoa after total extermination by a volcanic eruption. Such observations are most important for the right understanding of insular faunas and floras. Could not the author devote a little more space to these subjects, so as to make his book a complete manual for students of Island Life? C. R.

KALM'S ACCOUNT OF HIS VISIT TO ENGLAND ON HIS WAY TO AMERICA IN 1748. Translated by Joseph Lucas. 8vo. Pp. xvi., 480. London: Macmillan and Co., 1892. Price 12s. net.

THE Naturalist may sometimes rejoice with the Historian in accounts of journeys made in this country before the present century. Thus the "Travels in England, Scotland, and the Hebrides," of Faujas Saint-Fond (translated and published in 1799), contain many interesting notes on the minerals and rocks of the districts visited, the author being Professor of Geology in the Museum of Natural History at Paris.

Pehr Kalm, the account of whose visit is now before us, was born in Sweden in 1716; he became a pupil of Linnæus, and subsequently assisted his master with contributions to the *Flora Suecica*. After travelling over much of eastern Europe, Kalm was, in 1747, nominated as the first Professor of Œconomy at Abo, and in the same year he was commissioned by the Swedish Government (aided by the Universities) to visit North America. He was instructed to observe and describe the natural productions of that country, the special object being the introduction into Sweden of useful plants. He arrived in England in February, 1748, but for want of a vessel to cross to America, he was obliged to wait in this country until the 5th August. To this delay we owe his notes on England, which have been happily translated from the Swedish by Mr. Lucas. Kalm's original work occupied three volumes, which are now very scarce, but the American portion was rendered into English in the last century.

While delayed in this country, Kalm made observations from four centres—London, Woodford, Little Gaddesden in Hertfordshire, and Gravesend. His notes are of a very miscellaneous character. They relate largely to agriculture, but also include many notes of the strata, especially of the Chalk and Totternhoe Stone, and of the Tertiary gravels and clays. He records sections in considerable

detail,[1] and notes the fossils, observing in one case, "This was an unfailing sign that the chalk-formation had in former times been sea, as well as that the chalk is a child of later times, unless these so-called mussel-shells are *lusus naturæ*." He observes, in particular, a coarse sand near Ivinghoe "which was not such as our most common sand in Sweden, viz., of quartz, but it consisted of bare flint, such as is found everywhere about here, which had been reduced to a coarser or finer sand."

It is interesting to learn that in the suburbs of London the fences around meadows and market gardens were frequently made of banks of earth interstratified with "a multitude of the quicks or inner parts of Ox-horns," often with part of the skull attached. There were sometimes as many as six layers of these fragmentary horns. The fact is of importance in connection with the abundant bovine remains that are sometimes dug up in the district mentioned; and we hear that in one instance Kalm's remarks have quite recently explained what might have been regarded as a curious geological discovery.

There are many notes on the trees and hedgerows, and on various plants met with. Kalm states that one day "we amused ourselves by seeking out and describing the plants which occurred in a hay-stack at *Hudnall*." He gives a list of 28 species, but of some only single examples were found. The translator tells us that the verification of the names of the plants would have been a hopeless task, but for the existence in the British Museum of two volumes of Kalm's work with MS. notes, from the library of Sir Joseph Banks.

The date of Kalm's visit was five years prior to the foundation of the British Museum. The collection of Sir Hans Sloane formed the first portion of the Natural History objects that were then brought together. Kalm records his introduction to Sir Hans, then reported to be in his 94th year, and to his collections "in all three Natural Kingdoms, *Antiquities*, *Anatomy*, and many *Curiosities*." Some account is also given of the "Chelsea Physick Garden," and Kalm notes that in a room up in the Orangery there was preserved as a great rarity the collection of plants formed by John Ray. This collection had been given by Ray to Samuel Dale, who presented it to the Physick Garden: but it is remarked: "In some places the plants had been cut out, for *Dr. Sherard* had borrowed this collection from Mr. Dale, and when he had found any plant, which was either rare, or he thought much of, it was said that he had either clipped or cut it out, so that the books had been sufficiently mutilated." Ray's herbarium is now in the British Museum.

The value of this work is of course to a large extent antiquarian, but, as we have noted, there are many facts that will interest both Geologists and Botanists. Furthermore, there is a good General index and a separate Botanical index.

H. B. W.

THE *Zoological Record* for 1890 was issued in March by the Zoological Society. The volume is arranged as usual, and edited by Mr. F. E. Beddard, who now retires from the work. The next volume, in progress, is under the editorial direction of Dr. D. Sharp, F.R.S.

[1] Records of two sections, communicated by Mr. Lucas, were published in Mr. Whitaker's "Geology of London," vol. i., 1889, pp. 174, 175.

THE APODIDÆ; a Morphological Study. By H. M. Bernard. 8vo. Pp. 316. London: Macmillan & Co., 1892. Price 7s. 6d.

WE cannot altogether congratulate the publishers upon the latest volume of the "Nature Series." The writer has set himself the "ambitious task" of proving that *Apus* and its immediate allies can be derived from a "bent carnivorous Annelid." It is the general opinion of naturalists that these Crustaceans stand nearer to the base of the Crustacean series than any other forms. There is, therefore, nothing particularly new in Mr. Bernard's contention; nor is there much evidence in the work before us added to what is already known concerning this possible origin of the Phyllopod Crustaceans. An ideally intermediate form between the Annelids and the Crustaceans should show that essential Annelid feature—a metamerism of the internal organs; and *Apus* is in this respect not in the least more worm-like than other Crustaceans. It is true that Mr. Bernard cannot be blamed for this; but he attempts to show that *Apus* does show traces of this internal metamerism in the sexual organs. The gonads are figured as being of an elongated form, with crenations along the margin; but it is putting too great a strain upon this comparatively unimportant fact to adduce it as evidence of a fusion between a series of segmentally repeated gonads. Zaddach described the gonads of this animal as forming a kind of network, while Mr. Bernard did not observe this, though believing, nevertheless, that it may exist; and this fact, if definitely proved, would be unquestionably of interest, for Dr. Benham, in an important paper upon the testis of the King Crab (*Limulus*), particularly dwelt upon the similarity which the retiform condition of that organ in *Limulus* shows to the Arachnids. As Mr. Bernard believes that *Apus* and *Limulus* are not far apart, it is surprising that he has not seized upon this point.

The principal new facts in the book concern the "water sacs" and the "neck gland." The former are paired sacs that spread out over the eyes, and appear to contain a watery fluid during life. Mr. Bernard thinks that they are an indication of the backward movement of the eyes. They represent, he believes, the soft non-calcified integument which lies between the dorsal sclerites. As the author is so eager to show affinities between *Apus* and worms, he might have made use of these structures as an argument by pointing out their possible identity with the ciliated tubes of the Gephyreans and of other Annelids which are related to the central nervous system. The neck-gland is considered by Mr. Bernard to be an excretory organ; and it is quite possible that he is right, though here again he has omitted to cite what might have been of use in assisting him to prove one of his views.

A special chapter of the book is devoted to a consideration of the resemblances shown by *Apus* to the extinct Trilobites. In many Trilobites, at any rate, there is a pair of pores upon the head-shield, which are very possibly comparable to these organs in *Apus*. Dr. Henry Woodward has lately, in the Palæontographical Society's memoir on the Carboniferous Trilobites, particularly emphasised the existence of these pores as evidence of a likeness between the Trilobites and certain Isopod Crustaceans, especially *Serolis*, where they also exist. There is probably something of interest here, though at present details are wanting.

The alleged hermaphrodite condition of *Apus* is the last remaining matter of special interest in the volume; and we do not think that

this hermaphroditism is as yet definitely proved. The figures given by the author are exceedingly poor; so too, it may be remarked, are many of the illustrations in the book. It is, in short, not possible to accept, without further proof, the statement that *Apus* is really hermaphrodite; but, considering that Dr. Bullar's discovery of the hermaphroditism of the Cymohoidæ was received with so much suspicion, though ultimately proved to be perfectly correct, it would be rash to commit oneself.

ENGLISH BOTANY, or Coloured Figures of British Plants.—Supplement to the Third Edition. Part I. (Orders I.–XXII.) Compiled and illustrated by N. E. Brown. 4to. Pp. 56 and viii., with six plates. London: George Bell and Sons, 1892. Price 5s.

THIS, the first portion of a Supplement to English Botany, has been in preparation for several years. Two similar parts, we are told, are now nearly ready for publication, Mr. Brown having carried it as far as the Dipsaceæ, but pressure of other work having compelled him to resign this, Mr. Arthur Bennett has undertaken the continuation.

Students of the British Flora will turn with great interest to the Supplement; for though our island has been well botanised, new forms are constantly being brought forward, as well as new ideas on the old well-known ones, and a synoptical account of the information every few years is a necessity. Mr. N. E. Brown gives us the result of his evidently careful investigation on the earlier orders, from Ranunculaceæ to Celastraceæ. New forms are fully described, and the localities carefully indicated; the critical points are, moreover, discussed at length, and the author does not hesitate to give his own opinion, though not always in the best of English.

The value of a work like that before us lies, of course, in its practical use, and must be estimated by the practical student, who, though differing in opinion on minor points, can always appreciate and use the facts recorded by a fellow-worker. In one point, however, and that by no means of small importance, we fear Mr. Brown will not obtain universal approbation. We refer to the much-vexed question of Nomenclature. The bewildered botanist has, probably, already learned that the white water-lily is no longer *Nymphæa alba*, but *Castalia speciosa*, while *Nuphar lutea* and *N. pumila* must be put under *Nymphæa*; the genera *Castalia* and *Nymphæa* of Salisbury being three years older than the *Nymphæa* and *Nuphar* introduced by Smith in 1808 or 1809, and used without question for our well-known water-lilies until Messrs. Green and James raked up their proper names in 1887. A service of nearly fourscore years gives no right of way where a name is concerned.

Study of the sublime may, however, lead to the ridiculous, and surely something of the sort has happened here; for turning to page 1 of the additions and corrections we find *Corydalis bulbosa* and *C. claviculata* of De Candolle replaced by *Neckeria bulbosa* and *N. claviculata* of N. E. Brown, while De Candolle's *Corydalis lutea* becomes *Neckeria lutea*, Neck. *Neckeria* (1777) certainly has priority by twelve years, but by Mr. Brown's own showing is also forestalled by two other names, *Capnoides* (1763) and *Split* (1745), the two last being rejected "as names ending in *oides* and others such as *Split* are by common consent considered untenable for generic purposes." This seems rather arbitrary. If common consent availed aught, the water-lilies might have been left undisturbed. Besides, have we not *Castanopsis*,

and why is "oides" worse than "opsis"? Moreover, is *Quamoclit* much worse than *Split?* If the æsthetic sense is to be considered there is no knowing where we shall stop, for tastes differ so widely, and sounds which irritate one author may be as music to another. In the present case, we doubt not soon to see *Split bulbosa*, &c., while if we are first led through *Capnoides*, synonymy will be still further burdened. Much ink and paper were wasted on the rights of *Tissa* and *Buda* not very long ago, but these are now set aside in favour of Mitchell's *Corion* for *Spergularia*. Mr. Brown, has, however, no right to claim these species, for as he in fact points out, the valuable discovery is due to Dr. Britton (*Journal of Botany*, 1891).

The text of the book is good and clear, but the same cannot be said of the plates. Those of *Arabis alpina* and *Polygala amara* are very poor; the former, moreover, is marked 117*a*, but referred to in the text on page 24 as 37*a*. It is a pity not to mention the name of the work on the plates forming part of it, the bare name of a plant and a number giving no clue to its origin should it become detached.

GEOGRAPHY OF AFRICA SOUTH OF THE ZAMBESI: with Notes on the Industries, Wealth, and Social Progress of the States and Peoples. By Rev. William Parr Gresswell, M.A. 8vo. Pp. 400, with three maps. Oxford: Clarendon Press, 1892. Price 7s. 6d.

THIS little work is published under the auspices of the Royal Colonial Institute, and though primarily adapted for use in schools and colleges, contains a great deal of information of interest to the general reader. It is prefaced by a brief historical introduction, relating to the gradual progress of the colonisation of South Africa; and there are thirty-two appendices of statistical information and matters interesting to the politician. A good general description of the Physical Features and Natural History of the country is compressed into the space of seventy pages; and the detailed account of the several colonies and territories will prove very useful for reference.

CATALOGUE OF THE TYPE FOSSILS IN THE WOODWARDIAN MUSEUM, CAMBRIDGE. By Henry Woods, B.A., F.G.S. 8vo. Pp. 180. Cambridge: University Press, 1891 [1892]. Price 7s. 6d.

THIS is a valuable list of the fossils in the Woodwardian Museum to which reference has been made in published works. It is carefully prepared, beautifully printed, and will be much consulted by Stratigraphical Geologists and Palæontologists. In a brief Preface, Professor Hughes remarks upon the historical interest of the older collections in the Museum, comprising those of Agostino Scilla (1670), Lister (1688), and of John Woodward, the founder.

AN elaborate work on the Cuckoo has just appeared in Berlin (*Das Leben der europäischen Kuckucke, nebst Beiträgen zur Lebenskunde der übrigen parasitischen Kuckucke und Stärlinge*. By A. C. Eduard Baldamus. Roy. 8vo. Pp. 224. Pls. viii. Berlin: Paul Parey, 1892.). About 100 pages are devoted to the common European cuckoo (*Cuculus canorus*); and the remainder of the work deals with the other cuckoos and starlings that lay their eggs in the nests of different birds. Eight coloured plates of eggs are given.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The Committee formed under the presidency of the Marquis of Lorne for the restoration of the Library of the Toronto University have satisfactorily concluded their labours. Having already despatched about 30,000 volumes, they have just forwarded, as a last instalment, four additional cases, containing some 350 volumes of Prussian scientific works, the gift of the German Emperor.

Dr. James D. Dana has retired from the Professorship of Geology in Yale University, which he has held since 1855. He is succeeded by Professor H. S. Williams, whose removal to Yale we noticed last month. Professor Dana is this year the recipient of the Walker Prize of the Boston Society of Natural History.

Professor C. H. Gilbert and his colleagues of the new Leland Stanford Junior University, Palo Alto, California, propose to establish a Marine Biological Station on the Pacific Coast, in the Bay of Monterey. It is hoped to open the laboratory during the coming summer, and facilities will be given for work both by teachers of Natural Science and by specialists engaged in original research.

The Pennsylvanian University in Philadelphia will shortly be extended by the addition of a new Museum of Human and Comparative Anatomy, with fully-equipped laboratories for original research. The museum will contain the Wister and Horner collection, and is the gift of General Wister, who also provides an endowment fund for the curatorship.

The University of Dublin will celebrate its Tercentenary from the 5th to the 8th of July. There will be a commemoration service in St. Patrick's Cathedral, a reception and ball by the Lord Mayor, a banquet in the Leinster Hall, a dramatic performance by graduates and undergraduates at the Gaiety Theatre, and other festivities. The graduates of the University are marking the event by forming a Union, for which a special building will be erected. Delegates have been invited from the Universities and leading colleges of the world. Natural Science will be well represented at the gathering. Professor T. G. Bonney, Dr. J. Burdon Sanderson, Rev. W. H. Dallinger, Mr. Thiselton Dyer, Mr. John Evans, Dr. D. Ferrier, Professor M. Foster, Sir A. Geikie, Professor V. Horsley, Professor J. W. Judd, Professor E. Ray Lankester, Sir J. Lister, Sir J. Lubbock, Professor A. Macalister, Sir L. F. McClintock, Professor H. A. Newton, Baron Nordenksjöld, Sir J. Paget, the Abbé Renard, and Professor H. Reusch are among those who have accepted invitations to be present. Numerous honorary degrees will be conferred, and some of the delegates will be asked to address the students. A movement is on foot to petition the University to signalise its Tercentenary by granting degrees to women.

The fiftieth anniversary of the Doctorate of Professor Albert von Kölliker has just been commemorated by the issue of a special supplement to the fifty-third

volume of Siebold and Kölliker's *Zeitschrift für wissenschaftliche Zoologie*. The supplement consists of ten beautifully illustrated memoirs by pupils and admirers of the Professor. C. Hasse discusses the development of the vertebral column of *Triton taeniatus*, and there are notes by H. Ludwig on an abnormal *Cucumaria planci*, and by W. Voigt on a parasitic copepod found in a *Synapta*. R. Wiedersheim treats of the marsupial bones and epipubic cartilages in vertebrates. G. H. Th. Eimer has an elaborate investigation of the structure of muscle-fibres, while J. Eberth and K. Müller discuss the pancreas. O. Bütschli attempts to show how echinoderms may have been derived from a primitive bilaterally symmetrical form; and there are also contributions from H. Virchow, Th. W. Engelmann, and E. Ehlers.

THE name of M. Hamy is placed first on the list of candidates nominated by the Academy of Sciences for the vacant Professorship of Anthropology at the Paris Museum of Natural History.

IMPORTANT additions are being made to the exhibited collection of Comparative Anatomy in the University Museum, Oxford, and several new cases have been added on the plan of those in the Central Hall of the British Museum (Natural History). Mr. W. P. Pycraft, for some years assistant in the Leicester Town Museum, has recently been appointed to assist in the preparation of the new specimens.

IN a communication supplementary to last month's "Notes," Miss Agnes Crane alludes to the unfortunate circumstance that no facilities are provided at the Brighton Aquarium for biological students to make use of the interesting small organisms that so frequently make their appearance in the tanks. It would not involve much outlay on the part of the directors to develop to some extent the Natural History resources of the institution; and, by taking some steps in this direction, they would merely be carrying out one of the overlooked aims of the original founders. Professor Newton announces that there are vacancies at the Cambridge University tables at the Marine Stations both of Plymouth and Naples.

THE collection of Carboniferous and other fossils made by the late Mr. E. W. Binney, F.R.S., of Manchester, has been presented by his son to the Woodwardian Museum, Cambridge.

SEVERAL societies in London have lately held their Anniversary Meeting. At the *Society of Antiquaries*, on April 23, Mr. A. W. Franks, of the British Museum, was elected President in succession to Dr. John Evans, who has occupied the chair for the customary term of seven years. The *Zoological Society* met on April 29, and re-elected Professor Flower as President. In the Annual Report the Council regret that, for the first time for many years, the total number of Fellows is reduced to less than 3,000. Considering the interest of the collections to the general public and their great importance to the scientific student, we can only feel surprised that the former take so little part in the Society's affairs, and that so many of the latter avail themselves of the advantages offered by the Society while not contributing to its funds. At the Annual Meeting of the *Royal Geographical Society*, on May 23, Sir M. E. Grant Duff was re-elected President. The Founder's Medal was presented to Mr. Alfred Russel Wallace, and the Patron's Medal to Mr. Edward Whymper. On the following day, Mr. Wallace was again the honoured recipient of the Gold Medal of the *Linnean Society*. At this meeting Professor Charles Stewart accepted the Presidency for another year.

THE annual meeting of the Leeds Philosophical and Literary Society was held on May 3. The report referred to the retirement of Professor Miall from the

curatorship of the Society's Museum last October, after a service of 20 years. Mr. Edgar R. Waite has been appointed to the vacant position. Among the more noteworthy additions to the collection of the Museum during the year, are a series of British birds' eggs and a case to illustrate the transformation of insects. Photographs of temporary sections of the Coal-measures in and near Leeds are also a noteworthy acquisition.

Natural Science is well represented in this year's list of selected candidates for the Fellowship of the Royal Society, which includes the names of Frank Evers Beddard, Hans Gadow, Francis Gotch, William Abbot Herdman, Frederick Wollaston Hutton, Louis Compton Miall, Benjamin Neve Peach, and Augustus D. Waller. The remaining names representing Physical Science are those of Robert Young Armstrong, John Ambrose Fleming, Clement Le Neve Foster, John Joly, Joseph Larmor, and Alexander Pedler; while Political Economy is honoured in the person of Mr. Robert Giffen.

By the death of Mr. William Reed, M.R.C.S., F.G.S., of York, at the advanced age of eighty-two years, on May 9, the Yorkshire Philosophical Society has lost its greatest benefactor. Not only did Mr. Reed give to the Museum of that Society his own magnificent general collection of fossils, but with rare liberality he purchased and presented the Whincopp and Baker collections of Crag fossils, the Wood collection of Palæozoic fossils (especially rich in Yorkshire Carboniferous Limestone species), part of the Bean collection (chiefly Jurassic), and the Elwes collection of Tertiary fossils. So far as we remember, Mr. Reed's only paper was a short note on a boring at Masham.

Two parts of the *Transactions of the Hertfordshire Natural History Society and Field Club* (vol. vi., pt. 7, vol. vii., pt. 1) have just been issued, the first occupied by abstracts of proceedings, the second by Mr. Hopkinson's Presidential Address on Francis Bacon. Vol. v. (1891) of the *Essex Naturalist* has also been completed, and the first four numbers of vol. vi. (for 1892) have reached us. This publication is now up to date, and the editor, Mr. William Cole, urges his fellow-members of the Essex Field Club to assist him in issuing the monthly parts punctually and regularly. Last year's volume contains, among others, valuable contributions on the cryptogamic and flowering plants of the county, on certain groups of lepidopterous insects, on the mollusca of the Thames estuary, on the Essex boulder clay, and on the undulations of the Chalk. The new volume opens with an interesting account of the existing flowering plants of Epping Forest as compared with the earlier records, by Mr. J. T. Powell; and Mr. William Cole follows with a discussion of the boundaries of the Forest. Mr. J. French describes evidence of supposed old fish-reservoirs, perhaps dating back to a period anterior to the Roman occupation of Britain. Mr. W. Whitaker contributes statistics of Essex well-sections; and the Rev. Hilderic Friend has a series of notes on British earthworms, with special reference to those of Essex. The *Report of the Marlborough College Natural History Society* for 1891 lately issued contains many small notes of local interest, and there are some observations by Mr. A. S. Eve on "coombs" and "lynchets." Coombs are dry valleys in the Chalk, and lynchets are horizontal ridges or banks on the slopes of the Chalk hills. Mr. Eve thinks that the latter were not made by man.

The May number of the *Quarterly Journal of the Geological Society* (vol. xlviii., pt. 2) contains many noteworthy papers, of which those by Professor Prestwich and Mr. Clement Reid on Pleistocene Britain are noticed elsewhere. In his Presidential Address, Sir Archibald Geikie treats of volcanic action in Britain from the Devonian age onwards. Among the papers the older rocks attract much attention; and there is a valuable memoir by Messrs. Jukes-Browne and Harrison on the Oceanic Deposits of the Barbados. The latter paper, supplemented by a note by Mr. J. W. Gregory,

has an especially important bearing on the question of great recent changes of level in the land of the Caribbean region. In the May number of the *Proceedings of the Geologists' Association* (vol. xii., pt. 7) two great problems are discussed. Professor J. F. Blake treats of the evolution of the Cephalopoda, and alludes to some general principles they seem to illustrate; Mr. H. B. Woodward discusses the subject of life-zones in sedimentary rocks, with special reference to his researches in the Jurassic Formation. The most generally interesting paper in the new issue of the *Transactions of the Manchester Geological Society* (vol. xxi., pts. 16, 17) is Professor Williamson's personal account of his early geological researches (1835 and onwards) in the neighbourhood of Manchester. Another geological paper appears in the last part of the *Mem. Manchester Lit. and Phil. Soc.* (ser. 4, vol. iv.), in which Mr. W. Brockbank attempts to prove the Permian age of the St. Bees Sandstone. There is also a brief valuable contribution to geology in the May number of the *Proceedings of the Royal Geographical Society* (vol. xiv., No. 5), in which Mr. R. M. W. Swan supplements Mr. Theodore Bent's paper on Mashonaland with some geological observations. Central Mashonaland, it appears, consists of elevated granite plateaux, varying from 3,000 to 5,000 feet in height. Groups of isolated little granite hills rise above the plateaux; and the only other rocks represented are quartzite and schists, with some crystalline limestones.

THE Royal Agricultural Society of England has issued eight diagrams illustrating the growth of the Wheat Plant, with explanatory notes by Mr. Carruthers, its consulting botanist. They are published by Messrs. W. & A. K. Johnston, at 10s. the set, and form a most instructive and beautiful series. The diagrams are reproductions of original drawings by Francis Bauer, now in the Botanical Department of the British Museum, and as Mr. Carruthers states, and as all who have seen any of the artist's drawings will fully credit, no more careful study or faithful representations of wheat have ever been made. "The diagrams and notes are prepared for the use of farmers, principally in view of the present interest in technical education, and of the senior scholars in country schools, whose studies are being directed to subjects that will engage their attention through life." The first sheet deals with the structure of the grain, the second with its germination, and the third with the young plant. In the next three the growth and development of the flower and ear are fully illustrated; the seventh shows the ripening of the grain, and the last the structure of the "straw" or stem of the plant. The figures are very clear, and each is carefully described in the notes, where the physiological functions as well as the anatomy of the parts are explained in simple scientific language, while the author has been careful to avoid errors and keep the information up to date.

THE Royal Academy of Sciences and Letters of Denmark offers a prize of 600 kronor for an original treatise on the Natural History of certain marine fishes of the northern European seas. The work should comprise the complete life-history of the fish dealt with, may be written in Danish, Swedish, English, German, French, or Latin, and must be sent in by October, 1894. Further particulars can be obtained from the Secretary, Dr. H. G. Zeuthen, University of Copenhagen.

MR. G. W. LAMPLUGH has been appointed an Assistant-Geologist on the Geological Survey of England and Wales. Mr. Lamplugh is well known through his researches on the Geology of East Yorkshire, and more especially on the Glacial Drifts in the neighbourhood of Bridlington and Flamborough Head, and on the Speeton Clay. He has also studied Glacial phenomena in Vancouver Island. The appointment of so experienced a geologist will add greatly to the strength of the Geological Survey, and we understand that he will carry on the work of mapping in the Isle of Man.

OBSERVATIONS AND CORRESPONDENCE.

The Limpet's Adhesive Power.

Having previously ascertained by a series of experiments, made at Folkestone in the summer of 1889, the force necessary to overcome the great powers of adherence of the limpet, I determined to make another series, in order, if possible, to find out the source of these remarkable adhesive properties.

To do this, I placed several limpets on the side of an empty glass tank; I then drilled two holes through different rings in the top of each shell, and passed through them a stout copper wire. The ends of this wire were twisted together, and then attached to a spring balance. By pulling on the latter, the force necessary to detach the limpet could be readily observed. Owing to the glass plate it was easily ascertained that no air space existed beneath the foot, which in every part was in close contact with the glass.

On exercising slight traction on the limpet, the foot and mantle became still more closely applied to the surface of the glass. There was, therefore, nothing to suggest a sucker-like action.

On injecting the limpet with corrosive sublimate, more than sufficient to immediately destroy all vitality, the shell became quite loose, but still the foot remained adherent. The force necessary to detach the dead limpet was, however, very much less than in the case of the living, for while a force of 35 lbs. was required to remove the living limpet, 25 lbs. sufficed to displace a limpet immediately after poisoning. Twenty-four hours after death a force of 9½ lbs. detached the limpet.

When the limpet was dislodged, a thin gelatinous coat remained on the glass wall; this substance appeared only slightly soluble in sea-water.

We may, perhaps, be justified in concluding from the above experiments that while some portion of the adhesive power may be due to atmospheric pressure, a very considerable amount, if not the major part, is probably dependent upon the throwing out of a very tenacious substance. In favour of the latter view is the definite evidence of the absence of a vacuum beneath the foot, and the fact that the adhesion continues after the death of the animal, also the positive proof of a tenacious secretion.

With regard to the secretion of the limpet, it may be mentioned that many species gradually eat their way into the hard strata to which they may be attached—Limestone, Old Red Sandstone, &c.

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TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

REPRODUCTION OF THE FORAMINIFERA—(*J. Eliot Jenkinson, Edinburgh*).—No very definite information has as yet been obtained on this subject, but important details have been accumulating since 1854, when Max Schultze, in his "Organismus der Polythalamien," figured a *Miliola* and a *Rotalia*, in both of which numerous young shells are shown. Among the Foraminifera it is likely that both fission and gemmation obtain, and there is evidence of viviparous reproduction following segmentation. Strethill Wright has noticed the young in *Spirillina* (*Ann. Mag. Nat. Hist.* [3], vii., 1861, p. 360); Carpenter treated exhaustively of the subject up to his time in his "Introduction," 1862; and Schacko (*Archiv. Naturg.* xlix., 1883, p. 428. and *Sitz. Ges. Nat. Freunde Berlin*, 1882, p. 130) has figured and described *Peneroplis proteus* showing the last fourteen chambers of the shell crowded with embryos, while, to be more exact, he figures another specimen, to prove that the shape of the embryo corresponds precisely with the original chamber of the parent shell. In 1884, Brady ("Challenger" Report) figured a specimen of *Cristellaria crepidula* in which young shells are seen to occupy some of the chambers; and in a more recent paper "On the Reproductive Condition of *Orbitolites complanata*, var. *laciniata*" (*Journ. R. Micros. Soc.*, 1888, p. 693), this condition is beautifully shown. Carpenter had already noticed this in 1862, and Semper, in 1863, had been very near to Brady's discovery (*Zeitschr. Wiss. Zool.*, xiii., 1863, p. 558). (*See also* Schlumberger, *Bull. Soc. Zool. France*, xiii., 1889, p. 222.) As there is no orifice in the three genera last mentioned sufficiently large to allow of the passage of the young to the exterior, it seems highly probable that the birth of the young means the destruction of the parent, unless (which appears to be the case) an actual segmentation of the parent takes place to produce the young. We see the same thing when *Amœba* divides into two; the parent ends independent life and becomes two immature individuals. In *Cymbalopora*, the last or youngest chamber of the mature shell is much inflated, and young shells are found therein; and Brady noticed the presence of the rotaline portion of the mature shell, showing traces of the fractured balloon-shaped chamber in a gathering of mature and young individuals ("Challenger" Report). It is possible, therefore, that in *Cymbalopora* also, the birth of the young means the destruction of the parent. The most recent figure bearing on the subject is that of a *Lagena*, containing a young shell, which is given in a paper by Chaster in the *Ann. Rep. Southport Nat. Hist. Soc.*, referred to on p. 159 of our issue for April.

With regard to the question of sex, De la Harpe, when studying the Nummulites (*Mém. Soc. Pal. Suisse*, vii., 1881, p. 127; *Bull. Soc. Géol. France* [3], ix., 1881, p. 171), observed that there are two recognisable forms of each species, the individuals being divisible by size; and, further, that the smaller shells invariably begin with a large chamber, while the larger shells either have no recognisable central chamber, or one of very small size. Parker and Jones had previously noted the same thing. De la Harpe considered these variations as specific. Munier-Chalmas and Schlumberger took up the question, and came to the conclusion that the difference was one of development only, and that the "pairs" were two forms of the same species (*Bull. Soc. Géol. France* [3], viii., 1880, p. 300; *ibid.*, x., 1882, p. 470; *ibid.*, xiii., 1885, p. 273; *Comptes Rendus*, xcvi., 1883). Schlumberger has found this dimorphism in all the Milioline Group, and all his recent papers (in the publications of the Geological and Zoological Society of France) may be consulted on the subject. De la Harpe, in one of the papers quoted above, suggests that, had it been a fact that sex existed among the Foraminifera, he would have been inclined to regard the size of the central chamber as of a sexual nature, and Patrick Geddes seems to agree with this suggestion (*Proc. Roy. Soc. Edinb.*, 1885–6 [1886], p. 931). The subject has been referred to in many other papers; it is impossible to give a complete list here, but they will all be found in Sherborn's "Bibliography," published in 1888.

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NOTES AND COMMENTS.

Provincial Museums.

PROBABLY every English naturalist who has had occasion to visit the Natural Science museums of the Continent, and notably of Germany and Austria, has returned with feelings of the superiority of the provincial museums to those of our own country. Scattered in the English provinces we have museums which contain important historical collections, such as those at the Universities and York; extensive series of local specimens, as at Bath; important types, such as the Labyrinthodonts at Warwick, and the Austin Crinoids, at Liverpool; or valuable general collections, such as the Gurney Birds at Norwich, the Wallace Collection at Ipswich, Mr. Willett's Chalk Fossils at Brighton, or the Marquis of Northampton's Collection at Northampton. Nevertheless, in spite of the valuable contents of many museums, and the extensive local series obtained by the energy of the curators and their volunteer assistants, we cannot but feel that the provincial museums are not what they ought to be, and, in consequence, have failed to stimulate local research, and to popularise scientific education as much as they might have done.

The reasons for this are no doubt partly inherent in the particular conditions of our country. In the first place, Britain is so small, and internal communication so cheap and easy, that a greater centralisation is naturally to be expected than in a Continental State, which is larger in size, has fewer and slower railways, and really consists of a federation of different races. Munich, Dresden, Breslau, and Stuttgart occupy a very different position to Berlin than do our great provincial towns to London. Most of the famous Continental

museums are in cities that are the capitals of distinct nations, and the local fields they have to illustrate are wider and less explored, while race patriotism is a stronger stimulus than county feeling. Municipal rivalry in this country may occasionally be keen, but the stimulating jealousy of the Italian cities cannot be rivalled elsewhere on this side of the Atlantic.

But allowing most generously for all this, there yet remains much that is not due to any such general cause. If we compare the amount of research turned out of the smaller German museums, with that executed by our own curators, many of whom have better libraries and material, we cannot but feel that a certain amount of blame rests with them. No doubt the provincial curators are burdened with many trivial duties, and so long as the few good museum appointments, such as those on the staff of officers of the Natural History Museum, are filled by promotion within that building, there are few chances of promotion, and little encouragement to originality or research. The provincial museums too often have no permanent scientific curator, and are entrusted either to volunteers or to men who have to look after them in the intervals of teaching or library work. The result is that the museum is not well kept, tablets get dusty, the labelling falls into arrears, and, as an aspect of untidiness pervades the place, it loses whatever attractions and educational value it may once have had. And this does not apply only to the smaller provincial towns: it was the Oxford Museum that an examination candidate once quoted as the authority for the statement that no insect had more than five legs, while the vast majority had only three.

The moment we attempt to consider what remedy can be found for this state of affairs, we are met by the initial difficulty as to what is the real ideal of a museum. At present opinions are certainly not settled as to whether museums are to be regarded as store-houses of material or as illustrated text-books. The former was for long allowed to hold the field, but during recent years the latter theory, stimulated by the Index Collections at the Natural History Museum, has steadily been gaining ground. The former is still held in Germany, while the Americans have in some cases carried the latter to the extremest limits, and the comparison of a German and American museum clearly illustrates the difference between the two.

The typical German provincial museum generally has an entrance through a back courtyard, the exhibition rooms are crowded with cases of different shapes and patterns, in which the greater proportion of the collection is exposed to view, and so the visitor easily finds the specimens that interest him. After sundry experiments the right keys are found, and as soon as twenty years' dust has been removed from the specimens he is able to set to work. But in the museums of the new style everything is different; the building is a handsome modern structure surrounded by a grass plot that would do credit to

the best rolled Oxford quad; the fittings are luxurious, the cases are beautifully French-polished and warranted dust-proof, and the rooms are scrupulously clean, thanks to an impressive proclamation in the entrance-hall, which decrees that neither "smoking, spitting, nor chewing are allowed on the premises." The mounting of the specimens is faultless, but the poor zoologist, who has hoped to see an extensive series of specimens of the same species or fauna, has to content himself with the examination of a few faultlessly-mounted specimens, including a crocodile, a lion, a giraffe, an elephant, a few other such curiosities, and a room of dissections of examination types. The museum has been converted into an elementary text-book illustrated by actual specimens or dissections, and in the absence of the responsible member of the staff, who, of course, takes his vacation at the time that the visitor has to take his, the latter leaves the museum no wiser than he entered it. In cases such as these, the museum has been run on lines that must ruin its value to systematic zoologists. No doubt the show collections are intended for general instruction, but to turn a large museum simply into a tool for teaching the terms and definitions of elementary biology is a reckless extravagance: this is the work of the school museum, whereas that of the large State Museum is to stimulate observation and general interest, and to aid the collector to identify his specimens by the display of adequate series of species and variations.

But in order to do this strict limitations are necessary on the collections housed in the museum, and the great fault of the English provincial museums is, as a rule, their miscellaneous nature; and this can only be prevented by the exercise of considerable tact and courage on the part of the curator. Professor Duncan used to tell a story of a gentleman who called on him one day, when he was rearranging the Colchester Museum, and expressed such interest in the collection that he would be delighted to present to it a specimen of an 18 ft. crocodile. "Oh! no doubt you would, if it is 18 ft. long," was the opening sentence of the reply, in which the offer was gratefully declined. Unfortunately, this view of the aims of a provincial museum has not been always acted upon, with the result that in many cases the collection better illustrates the travels of the local gentry than the natural history of the neighbourhood. But the necessity for emphasising this view of the case is no longer necessary; the idea that a museum is intended as a mere "raree show" has long since been past. The educational value of museums is rapidly becoming recognised, and with the spread of technical teaching the necessity for an increase in their number and efficiency will, doubtless, follow, just as the free libraries have followed the establishment of School Boards. At the same time, they will cease to be regarded as mere branches of the library, and under independent control will be free to improve as greatly as the Natural History Departments of the British Museum have done since they escaped from the thraldom

of subserviency to the National Library. This change, it must not be forgotten, was mainly due to the continued agitation of Sir R. Owen, and a general alteration in the museum system of the country cannot be expected until the provincial curators strive for it. Hitherto there has been little united action among museum authorities; the foundation of the Museums Association, however, is a step in the right direction, and if this but establish a stronger *esprit de corps* among curators and stimulate them to use present opportunities more efficiently, it cannot but lead to the museums being placed on a sounder basis in the future.

The Deep-Sea Deposits of the West Indies.

Ever since the return of the "Challenger" Expedition first gave us a clear idea of the nature and distribution of the deep-sea deposits, no argument seems to have told so strongly for the theory of the permanence of oceans and continents than the asserted entire absence of deep-sea deposits among the marine sediments that have been raised above the sea. Various objections have been at times raised to this argument, but a first adequate reply to it has now been made by the publication of detailed descriptions of beds in Barbados which are claimed as of truly deep-sea origin by Haeckel, Jukes-Browne, and Harrison. Haeckel, in his "Challenger" Report on the Radiolaria, remarked that the Radiolaria of these deposits resembled those of the deepest areas of the Pacific. Jukes-Browne and Harrison, from a geological examination of the beds, maintained in letters to *Nature* that they were identical with deep-sea oozes, while the same conclusions were urged by Gregory on the evidence of an Echinoid having abyssal affinities obtained from the radiolarian deposits. The last number of the *Quart. Journ. Geol. Soc.* now contains two more papers bearing on the same subject. The principal of these is a detailed description by Messrs. Jukes-Browne and Harrison of the beds, giving an elaborate account of their structure, chemical, microscopical, and lithological; it is claimed in the paper that the extensive series of rocks comprised in the "Oceanic Deposits" of that island afford a complete parallel to the oozes of the present ocean floors: they include representatives of ordinary chalky earths, of radiolarian and diatomaceous oozes, and red clays. The lithological evidence shows that these are identical with modern deep-sea deposits, while the evidence of all the fossils known, including radiolaria as affirmed by Haeckel, of the Foraminifera as affirmed by Brady, and of the one echinoid as interpreted by Gregory, is in full agreement with this view. As these deep-sea beds are there interbedded between shallow water sandstones, and coral rocks, it affords evidence of a complete interchange of continental and oceanic conditions in late Tertiary times. Moreover, these beds do not only occur at Barbados; a pteropod marl indicates a fairly deep-water condition

at approximately the same period in Jamaica, while in the heart of the West Indian area radiolarian beds identical in character with those of Barbados have been found. Similarly, at the last meeting of the Geological Society, a paper by R. J. L. Guppy was read, describing the stratigraphical relations of a similar set of beds in Trinidad, and an appendix by Gregory describing the microscopic structure of the rocks shows that there again we have true oceanic deposits. This conclusion was strongly supported by Dr. Hinde, who had made a careful examination of the rocks. Geologists may, therefore, now fairly claim to have answered the challenge to produce deep-sea oozes as part of the raised land masses.

Another Continental Oceanic Deposit.

The last number of the zoological series of the Linnean Society's Journal is devoted to a description of the sponge remains from the Oamaru deposits in New Zealand, by Dr. G. J. Hinde and Mr. W. M. Holmes. The diatoms of this locality have long been known to microscopists, and have been described in a series of papers by Messrs. Grove and Sturt, published by the Quekett Club. The interest of this deposit is due largely to the fact that it appears to be a truly deep-sea pelagic deposit, and thus forms an additional illustration of the alternation of pelagic and continental conditions. The authors remark (p. 179): "The absence of coarse arenaceous materials is the same in the Oamaru as in the recent deep-sea ooze. We may therefore conclude that this Oamaru rock was a deep-sea deposit, formed at some considerable distance from land, and that it may rightly be compared with the Diatom ooze which now forms a belt of varying width surrounding the South Polar Regions, and extending from the Antarctic Circle to about lat. 40° S." The average depth of this diatom ooze is 1,477 fathoms, but the authors think the Oamaru deposit was formerly in deeper water than this, as radiolaria are so abundant in it; they think that its closest analogue is some ooze dredged by H.M.S. "Egeria," between 2,479 and 3,000 fathoms, off the south-west coast of Australia. The sponges described belong to 110 species and 43 genera, and this, of course, excludes those of which the flesh spicules have not been found. The species are nearly all extinct, and the deposit is probably to be regarded as of Upper Eocene or Oligocene age. Species of the group Monactinellida preponderate, whereas in the Mesozoic sponge beds this is almost unrepresented: this the authors regard as another instance of the imperfection of the geological record, the smaller monactinellid spicules having been rendered unrecognisable during the processes of fossilisation. In the course of such a careful examination of so large a series of specimens and species as that in the present memoir, numerous anomalies of distribution have been noticed: thus, genera known only living in the Central Atlantic

occur in the deposit, while genera which inhabit abyssal depths are associated with others now restricted to shallow seas. The authors of this careful and detailed monograph are to be congratulated on the combination of accurate systematic diagnosis, with a full consideration of the affinities of the fauna. The plates are of exceptional merit.

Epping Flora.

In the pamphlet on the "Organisation of Science," which we noticed last month, the author empties the vials of his contempt upon the compilers of county floras, which he calls "a kind of drivel over which life, and time, and print, are wasted." The author seems to forget that if naturalists are born, not made, it is collecting that has a good deal to do with the bearing: most men begin as collectors, and if this work is to be discouraged as waste of time, the next generation of naturalists is likely to be much smaller than the present. Except with those who agree with the writer of that pamphlet, we think most naturalists would strongly disapprove of the planting of wild plants from one district in the few small tracts of virgin land we have. We notice in the last number of the *Essex Naturalist* that *Damesonia* has recently been found in a very well known pond in Epping Forest.

"The thing is neither so very fine nor rare;
But the question how the devil it got there"

is a problem that is seriously puzzling the botanists of the Essex Field Club. There can be no doubt that such a plant as *Damesonia* (better known by its later name of *Actinocarpus*) could not possibly have escaped notice, while it is difficult to see how its introduction can have been effected by natural agencies. On the whole, one is bound to conclude that a too enthusiastic member of some acclimatisation society must have planted it there. If this sort of thing were done extensively it would hopelessly destroy the interest in the distribution of our native plants, which is the principal incentive to study among a large class of local workers. If this be the true explanation of this case, if the Epping botanists could only find out the man who has helped to muddle up the Forest record, that pond would probably receive an addition to its fauna as well as its flora.

The Dearth of Specialists.

In March and May we referred to Mr. F. Du Cane Godman's remarks on the increasing difficulty of obtaining specialists to undertake the work of describing many groups of insects. Mr. Whymper's "Travels amongst the Great Andes of the Equator," of which a second edition is in the press, affords an illustration of this difficulty, for though the expedition was made twelve years ago, the supple-

mentary volume in which the zoological collections are described has been issued with several large groups of insects omitted, as no one has been found able to describe them.

British Association.

The 62nd Annual Meeting of the British Association is to be held at Edinburgh, commencing August 3.

Geology on this occasion receives especial honour in the person of the President, and rightly so. The Science of the Association is now divided into eight sections, and during the 61 meetings Geology has been represented seven times; by Buckland, Sedgwick, Murchison, Lyell, Phillips, Ramsay, and Dawson. Now the Presidential chair is to be occupied by Sir Archibald Geikie.

The cities and towns where meetings have been held include 22 in England, four in Scotland, three in Ireland, two in Wales, and one in Canada. Birmingham has on four occasions received the Association, in this respect standing alone. There are, however, many large English towns that at present remain unvisited, partly, it may be, on account of a lack of local scientific enterprise, and partly on account of the difficulty in finding accommodation, or in raising the funds necessary to offer a fitting reception.

Among these towns we may mention the following, all of which have a population above that of Exeter, one of the cities to which a very pleasant visit has been made. We give these in order, beginning with the lesser populations:—Darlington, Lincoln, Worcester, Burton-on-Trent, Hastings, Newport, Northampton, Reading, Halifax, Burnley, Carlisle, Huddersfield, Derby, Preston, Bolton, Blackburn, Oldham, Sunderland, Leicester, and Portsmouth. Other towns of importance might be mentioned, but some of these, like Wolverhampton, are situated very near to larger towns that have accorded a welcome to the British Association.

The following Presidents of the several Sections for the Edinburgh meeting have been elected:—A. Mathematical and Physical Science, Professor Arthur Schuster; B. Chemistry and Mineralogy, Professor Herbert McLeod; C. Geology, Professor Charles Lapworth; D. Biology, Professor W. Rutherford; E. Geography, Professor James Geikie; F. Economic Science and Statistics, The Hon. Sir C. W. Fremantle, K.C.B.; G. Mechanical Science, Professor W. C. Unwin; and H. Anthropology, Professor Alexander Macalister.

The Seychelles, their Land Tortoises and Turtles.

Probably the most interesting of the Annual Colonial Reports issued this year is No. 40 [C. 6563, price 3*d.*], Mauritius (Seychelles and Rodrigues), which contains a Report on the Islands of Aldabra,

one of the Seychelles group. The report was written at the request of the Governor, by Thomas James Spurs, the lessee, who takes considerable interest in the structure of the island and in the preservation of its fauna and flora. Mr. Spurs remarks on the more abundant vegetation of Aldabra, compared with Astore, Cosmoledo, or Assumption. The island is covered with trees and stunted shrubs, is very green, and contains mangroves as much as 55 feet in height. As, however, the bush is inhabited by myriads of mosquitoes, few venture therein. Aldabra is flat, 20 miles long, trending north-west to south-east; its shape is crescentic, and a line of three islands between the horns encloses a lagoon, access to which is only obtained at low water by one of the four channels. The narrowness of the channel and the strength of the currents render it practicable only for steamers. In some valuable notes on the structure of Aldabra, Mr. Spurs says at first sight it seems extraordinary that an island essentially madreporic, and so vast, should not be interspersed with sandy plains; when, however, it is known how strong the currents and surf are, it is easily explained. The sea, bursting with great violence on the reef, has swept off all the calcareous detritus, and imparted to the growing coral an extraordinary strength. Mr. Spurs notes that the predominating coral in Aldabra is a great "porite"; he has not found it in the other islands, and has been unable to find it in a living state. The decomposition of the coral rock yields a yellow mould of great fertility, but, unfortunately, there is very little of it. The writer laments the destruction of the larger trees, which he considers partly due to the hurricane of 1889, and mainly to the mephitic exhalations from the soil. He quotes a note of Commander Needham's, written in 1890, as to the "rising of jets of vapour of large quantity from the water, which appeared around it to be depressed as in a whirlpool," and goes on to state that "the inference is that Aldabra is still liable to volcanic action." After some interesting meteorological notes, Mr. Spurs remarks that thirty species of trees and shrubs exist, but all are of small size, as they hardly find sufficient nourishment. Of the fauna, the large tortoises were very numerous, but owing to the reckless destruction by the harpooners, they have diminished in numbers. Referring to Picard Island, the writer has taken means to restock the place and to destroy the rats, which are the greatest enemies of young tortoises. He remarks on the activity and swiftness of these great chelonia, and says that they retire to the bush from May to September, and come out in October with the first rain. It is then that they couple, and the female lays from six to eight eggs in November, in January, and in February.

The sea turtles are numerous, but the males alone inhabit the bay. As soon as a female has attained the age of producing (20 to 25 years) she disappears—whither no one knows. When they return to the coast to lay eggs, they are covered with barnacles of two or three weeks' growth. The females are more valuable than

the males, and more easily caught, so much so that the proportion is ten males to one female, though ten females are hatched for every male. The males fight desperately in the breeding season, to the great advantage of the sharks, which as they are too small to eat a big turtle, eat only the fins, head, and tail, though one specimen caught by Mr. Spurs had a turtle inside of the weight of 30 lbs. The female turtle lays three times in each season, each time about 125 eggs. When hatched, the young fall a prey to land crabs, grallics, and frigate birds, and when they reach the sea, they allow themselves to be carried out by the currents into deep water, for there they are better able to escape the grasp of the fish. Mr. Spurs complains of the recklessness of the harpooners, who throw at great and small indiscriminately, and states that the protective laws were made by those ignorant of the habits of the turtles. He predicts that if not amended and enforced, turtles will be extinct in the Seychelles in ten years.

In a short supplementary letter from T. Risley Griffiths, the administrator, it is pointed out that the natural difficulties of the country and the seclusive habits of the land tortoises render their extinction unlikely, and while not exactly agreeing with the possible extinction of the turtles, shows that the enforcement of laws against fishermen in the open sea or uninhabited land is almost impossible. This letter also points out that the seas around the Aldabra group contain large quantities of beche-de-mer (the trepang of the Malays), and that Mr. Spurs has taken some Chinese, who are experts at preparing this article of diet, to Aldabra, and hopes to open up a new branch of trade with the Chinese markets. Mr. Spurs told Mr. Griffiths that he also expected to secure some 12,000 green turtles a year, the meat of which he proposes to dry and ship.

The Revival of Dried Seedlings.

In the *Revue Générale de Botanique*, M. Gaston Bonnier gives an account of some interesting experiments on the revival of dried seedlings of wheat, bean, haricot bean, pea, and maize.

Eight lots of wheat of the last year's harvest, each containing fifty grains, and as comparable as possible, were soaked in water for a day. These lots were designated A, A^1, B, B^1, C, C^1, and D, D^1 respectively, and subjected to different treatment. A and A^1 were dried for two days, A at 35° C., A^1 at 85°; at the end of the time they had not lost weight. The rest were set to germinate on sawdust under a bell-glass. After two days B, B^1 were taken up and dried for two days, B at 35° C., B^1 at 85° C. The radicle of the seedlings was showing.

At the end of the third day, when three roots were visible, C and C^1 were removed, and dried for two days at 35° C. and 85° C. respectively.

The remaining lots, D, D^1, were similarly treated at the end of the fourth day, when the roots were well developed, and the plumule had attained a length of 5 mm.

After drying, ten specimens were removed from each lot, put aside in a dry place, and set to germinate five months afterwards (in the following March). The results were quite analogous to those obtained with the other seedlings. M. Bonnier found that all the seedlings dried at 85° had quite lost their germinating power, except twelve out of the forty comprising the lot A^1, and these produced very poor plants, of which eight ultimately perished.

Of the seedlings dried at 35°, thirty-four out of forty of B, two recovered, giving plants as vigorous as those produced normally from seed. Of C, twenty-eight out of the forty recovered, but the resulting plants were less vigorous than those from seed, and five ultimately perished; while of D, only twenty-one out of forty recovered, yielding poor plants, twelve of which ultimately perished. No recovery took place under the same conditions with plants further advanced.

We see, therefore, that the recovery after drying depends on the stage of development of the seedling, and also on the temperature at which it is dried. It is very interesting to note the length of time—several months—during which the state of suspended animation can be retained.

Beans germinated for eight to fifteen days at 14° C., and then dried for one day at 35°, were able to resume active life; but if dried longer at the same temperature until there was no more loss of weight, they did not recover.

In the case of the haricot, younger seedlings must be taken than with the bean to ensure recovery. On these M. Bonnier made some histological observations. Seeds were germinated for two days and then dried completely at 20°, 35°, 55°, and 85° respectively. On examination of sections of the cotyledons in glycerine, the aspect of the cells was very different in the different cases. As we should expect, the dessiccation had a much greater effect on the protoplasm than on the cell-membrane, or the starch or aleurone grains. In fact, in the seedling dried at 85° C., there was no sensible change in the thickness of the last three; but the grains were much more crowded against each other and the cell-wall by the shrinking of the protoplasm. Iodine has a similar effect on the starch grains in the seedlings dried at 20° C. or 85° C., while Zinc Chlor. Iodine or aniline Violet colours the membranes almost in the same way. The protoplasm dried at 85° is very strongly stained by aniline-violet, but scarcely at all at 20° C.; Carmine in water stains it immediately at 85° C., but not at all at 20° C. These simple tests, M. Bonnier suggests, might be used to examine the power of germination by means of reagents. It is evident that the water combined with or contained in the protoplasm plays the most important part in the passage from active life to the dormant stage, and *vice versâ*.

Seedling peas could be dried without death resulting in a more advanced stage than haricots. Thus, after germinating for ten days at 13° C., when the root and hypocotyl together measured 6 or 7 cm., four seedlings were dried for twenty-four hours at 35° C. They were then soaked in water and put in the ground, when all four revived, producing plants which flowered abundantly.

Some maize was germinated for twelve days at 15° C., when the root and hypocotyl on an average measured 2 cm. After drying for twenty-four hours at 35° C., two-fifths of the specimens recovered, but when dried at 85° C. there was no recovery.

M. Bonnier also studied the relations between the gases taken up and given off, and also the evolution of heat. He gives no account of the experiments, simply stating that the phenomena in the revived seedlings are analogous to those obtaining in the germination of seeds, except that the first period (during which the proportion between the gases is nearly equal to unity, and the heat disengaged is less than subsequently), is relatively abridged, especially in the more advanced stage of development at which the seedling was dried.

Finally, M. Bonnier made a cultural experiment. Peas were sown in a plot of land, and then allowed to dry in the sun. On burying them a little in the ground and watering them they all revived.

The Protection of Birds.

The following circular letter has been issued by Mr. W. A. Nicholson :—

"Sir,—The committee of the Norfolk and Norwich Naturalists' Society are very desirous of bringing under the notice of landowners and agriculturists the great desirability of affording more efficient protection to useful birds, particularly those which, as destroyers of vermin and injurious insects, render immense service to the farmer and the community at large. Frequent comments and letters have recently appeared in the public journals as to the disastrous effects resulting from the indiscriminate slaughter of many useful species, not only in this country, but also on the Continent, and it is hoped that the publicity given, and the attention drawn to the subject will lead to a more judicious course of action. The importance of this matter, in view of the great devastation caused by the plague of field voles (mice) in some parts of Scotland, and past experiences in Lincolnshire, cannot be overlooked, and the opinions of the Scotch farmers in the districts affected, quoted from the reports to the Board of Agriculture, point to the folly of destroying owls, hawks, and weasels. The barn owl, a true farmers' friend, is much persecuted, but a more useful bird, as a destroyer of vermin, does not exist. It has been computed, by competent observers, that when it has young it will bring a mouse to its nest every 12 or 15 minutes, and as many as 20 good-sized rats, perfectly fresh, have been counted in a single nest. A recent communication to the daily papers states that a nest containing five young ones, being taken and placed under a hen coop about a mile distant, no less than 24 rats, large and small, brought there by the parent birds,

were found lying outside the coop the following morning. The owlets were at once returned to the place from whence they were taken. The kestrel hawk, a great killer of mice, is another bird which merits protection, and it is much to be desired that game preservers would give their keepers stringent orders not to molest it. To such an extent is the destruction of our native birds carried on, that it is not improbable further legislation in the matter will be called for, and it is to be hoped the Board of Agriculture will continue to prosecute their inquiries into the pecuniary loss accruing from such destruction."

The *Revue Scientifique* records that the enthusiastic protector of birds in France, M. Xavier Raspail, has just issued a letter to the Minister of the Interior, pointing out how the laws against bird destruction are openly violated by farmers. He states that of 15,544 birds destroyed, some 13,000 of them are known to feed on insects. He points out especially that the cultivation of wheat is becoming less and less remunerative, and one of the causes is traceable to the destruction of larks, whose food largely consists of the larva of a beetle, *Agriotes striatus*, whose ravages to the roots of wheat are well known. Vine growers complain of the scarcity of bird life in the vineyards, and perhaps the very best method of compelling the obstinate to understand is to pit them against their more sensible brethren. The *Zoologist* for May reprints portions of Messrs. Dudgeon and Davidson's reports on the plague of field mice in Scotland. They are stated to be "swarming in millions," and the cause of the pest is definitely said "to be the destruction of the natural enemies of the voles—hawks, owls, weasels, &c., and it has been remarked that, where plantations are present, affording roosting for birds of prey, the districts in their vicinity are less seriously affected." And again, during a similar outbreak in 1875, large accumulations of the casts of owls, consisting of the fur and half digested skins of mice, were seen on every hand. The report is very convincing, and should be circulated broadcast throughout the country by those responsible for general agricultural prosperity. The subject is attracting attention in the proper quarter already, for it is stated that the Royal Agricultural Society are contemplating the appointment of an economic ornithologist.

THE large number of 260 elephants have recently been captured in the Garo Hills by a party of hunters headed by Mr. Savi, superintendent of Keddahs at Dacca.

As an instance of the sudden destruction of animal life, it is interesting to note that, on the occasion of some heavy rains in Texas recently, so much sediment was carried to the rivers and streams that the water became so thick as to kill the fish, which floated by thousands down the flood.

A curious instance of the decalcification of bones by the action of water from a peat bog is recorded by Messrs. E. R. Waite and P. F. Kendall in the June number of *The Naturalist*. The case is that of a Fallow Deer discovered last summer in the peat of Goole Moor, Yorkshire. The bones are quite pliable and elastic, and of a dark brown colour; and the teeth also are so light that they float in water. Remains of the hair were also found with the skeleton.

Dr. Kükenthal has been led to undertake the researches just referred to by his detailed investigation of the Embryology and Comparative Anatomy of the Cetacea, on which he will shortly issue an elaborate work. He considers that the Whalebone Whales have been evolved from a group of land-quadrupeds quite distinct from that which gave rise to the Toothed Whales, Porpoises, &c. Moreover, he is of opinion that the great number of the teeth in these aquatic mammals is due to the splitting of the fewer many-cusped teeth that existed in their ancestors.

The pineal eye of vertebrates still continues to attract attention, and Dr. Carrington Purvis attempts to criticise some current views in a brief paper on the pineal body of the Porbeagle Shark (*Lamna cornubica*) in the newly-issued part of the *Proceedings of the Royal Physical Society of Edinburgh* (Session 1890–91, pp. 62–67, pl. ii.). Dr. Purvis suspects, from his observations, that the conclusions of previous observers as to the pineal eye being comparable with a highly-developed invertebrate eye, will prove unfounded. He is inclined to think that it may be "a vertebrate type of eye, arrested, however, at a very early stage (primary optic vesicle stage) in its development."

The last number (30) of the *Bulletin of the Botanical Department of Jamaica* (April, 1892) contains the economics of the moth *Diatræa saccharalis*, Fabr., the sugar-cane borer, by T. D. A. Cockerell. After a brief historical note, which shows that the larva was probably that one referred to by Sir Hans Sloane as early as 1725, and that the imago was identified by Westwood in 1856 as the *Phaleena saccharalis* of Fabricius, 1793, the author notes the life-history of the insect from the egg to the moth, which has been figured by Comstock and Howard. The eggs are laid on the leaves of the young cane near the axils, and the larva penetrates the stalk at or near the joint, tunnelling through the soft pith usually in an upward direction. The whole life of the insect, from egg to moth, may be taken at six weeks. Little is known of the natural enemies of the sugar-cane borer, but ants have been observed to keep it in check. *Chanliognathus pennsylvanicus*, in its larval stage, has been seen feeding on larvæ of

Diatræa. The notes on geographical distribution seem to point to a wide range; West Indies, United States, Sandwich Isles, India, and Queensland, are mentioned. The injury done by the larva is very serious; in one case a grower writes, "we had estimated for 140 tons, but will not make more than 115 tons of sugar," a reduction probably resulting in loss instead of profit. The larva of *Diatræa* has been found to attack maize, sorghum and gama grass, and as Mr. Cockerell points out, "the occurrence of the borer in a wild grass may be of considerable practical importance, as it would be almost useless to kill the borers in the canes if they were breeding abundantly in an adjoining grass-patch." Of the remedies, the most effective seems to be that of burning the waste upon the plantation itself, though Paris green, carbolic acid, sulphate of ammonia, kerosene, have all been serviceable. A fertiliser called "Kainit" seems to be protective, and has the additional advantage of improving the soil for future crops. Some remarks on species allied to the sugar-cane borer, and on some other sugar-cane pests, complete a careful paper, which, from the fact that the author has collected together the writings of his predecessors on the subject, and added to them many personal observations, should prove of much value to growers of this plant.

THE imports of raw caoutchouc and raw gutta percha into the United Kingdom for the past seven years, occupies the *Supplement to the India Rubber Journal* for 9th May. The article gives the weight imported per month, its value, and the localities from which it is obtained. We notice that, as regards caoutchouc, the import steadily increases, while the import of gutta percha varies considerably from year to year. The price of the latter has risen rapidly the last few years.

ENGLISH grown tobacco was, we believe, not a success, and English grown tea will probably never be more than a curiosity. However, Mr. W. Iceton has recently reared a number of the latter plants in his palm nurseries at Putney, and Mr. John Roger, formerly a Ceylon tea planter, has prepared tea from them. The plants and the economic product were exhibited at Exeter Hall at a recent meeting, and the Princess Louise and the Marquis of Lorne, who were present, were regaled with the beverage. Mr. Roger believes this to be the first preparation of tea from the home-grown leaf.

IN the June number of *Grevillea* Dr. M. C. Cooke takes leave of the subscribers to the journal, which he has directed and edited from its commencement twenty years ago. Increasing years and fickle health are the reasons for transferring the duties and responsibilities to other and younger hands; but Dr. Cooke does not intend to "sever

his interest" or "withhold the use of his pen." Mr. George Massee undertakes the responsibility of carrying on the work, and will endeavour "to accomplish the desire expressed by the editor in the first volume; that of describing or recording new discoveries—especially British—in every section of Cryptogamic Botany, also, by abstracts and notices, to indicate the results of work done in other countries, biological, morphological, and systematic." Assistance has been promised by well-known specialists, and Mr. E. A. L. Batters will take entire charge of the section devoted to Algæ. We are glad to notice that each number is to contain "one or two plates, plain or coloured, as occasion may require."

In a preliminary note on the Foraminifera dredged by the Prince of Monaco off the Azores, contributed to the *Mém. Soc. Zool. France* (vol. v., 1892, p. 193), M. Schlumberger describes and figures for the first time the young free stage of *Polytrema miniaceum* (Linn.). A median section shows an assemblage of three spherical chambers having thin walls pierced with few perforations, and arranged as the embryonic chambers of a *Globigerina*, except that there are no special openings from one chamber to the other. Around these three chambers are numerous others, irregular in size, and presenting the adult exterior characters of *Polytrema*. M. Schlumberger adds that the embryonic form may be found in adult specimens, a little above the surface of attachment.

M. E. L. Bouvier has published some observations on the power of Hermit Crabs to adapt themselves when young indiscriminately to left-handed and right-handed Gasteropod shells (*Bull. Soc. Philom. Paris*, ser. 8, vol. iv., pp. 5–9, 1892). He concludes that the majority become adapted to the right-handed twist, because shells of this character are by far the most numerous. When immature, the crabs seem to be indifferent as to the direction of the twist, though, as already remarked by A. Agassiz, there is a tendency in the abdomen to curvé to the right even in very young individuals that have not yet dwelt in a shell.

The mode of formation of the well-known chitinous envelope of the larvæ of Dragon-flies (*Libellula*) has recently been investigated anew by M. Joannes Chatin (*Comptes Rendus*, vol. cxiv., pp. 1135–1138). Contrary to the prevailing belief that this covering arises as a secretion, M. Chatin concludes that it originates by the direct modification of the cells of the epidermis itself. The process as described is peculiar, and the results of the investigation, if confirmed, will necessitate some modification in current views as to the true nature of the skin in insects.

Two noteworthy contributions to knowledge of the Hydrozoan polypes have lately appeared. In studying the common *Hydra*, Dr. R. Zoja (*Rendiconti R. Istit. Lombardo* [2], vol. xxv., pp. 700–712, pl. iii.) has been led to interpret as evidence of a comparatively complex nervous system, certain groups of granules and radiating filaments, which he has been able to observe with great distinctness. In an elaborate memoir on *Ceratella* (*Trans. Roy. Soc. Victoria*, vol. ii., pt. ii., pp. 8–24, pls. ii.–iiiA.), Professor Baldwin Spencer supplements and completes the work of previous observers in describing the anatomy of this remarkable polype. Originally determined to be a sponge by Gray in 1868, *Ceratella* was first recognised as a Hydrozoan by H. J. Carter in 1873, and Professor Spencer now confirms the belief that it represents a family distinct from that of the Hydractiniidæ.

THE supposed salivary glands of the "sea-mouse" (*Aphrodite*) have recently been examined by M. A. E. Malard, who points out that they are not in the least glandular in character, and ought to be regarded as labial palpi (*Bull. Soc. Philom. Paris*, ser. 8, vol. iv., pp. 15, 16).

IN describing a new Staphylinid Beetle (*Trygaeus javanicus*) from Western Java (*Notes Leyden Mus.*, vol. xiv., 1892, p. 61), Dr. D. Sharp expresses his dissent from the current belief that the insect fauna of Japan is more similar to that of the Palæarctic region than to that of the Oriental region. He is of opinion that the belief is due solely to the imperfection of the evidence, and that when as much is known about the insects of the East as is already known about those of Europe, the fauna of Japan will be found to agree most closely with that of the Oriental region.

THE Pharmaceutical Society have just published a Catalogue of the Hanbury Herbarium contained in their Museum at Bloomsbury Square. It forms a small octavo volume of about 150 pages, and is the work of Mr. E. M. Holmes, the Curator of the Museum. Mr. Daniel Hanbury's Pharmaceutical Herbarium and collection of Materia Medica were presented to the society by his executors, on the condition that they should be kept apart and carefully protected from injury or loss. As the specimens are, as a rule, excellently preserved, and complete in detail, they will doubtless, with the help of the catalogue, be of great service to students of pharmacy, who seem to have been in want of something of the kind.

The classification adopted is that of Bentham and Hooker's "Genera Plantarum," and the plants are arranged alphabetically under each natural order. Separate sheets of specimens are indicated by italic letters, and the specimens on each sheet by ordinary

numerals. The value of the Catalogue is much enhanced by the dates, localities, and references to published works included under each specimen, with frequently interesting notes on the history or properties of the plant in question.

An important memoir on the Petroleum and Ozokerite of Galicia, by Mr. Boverton Redwood, appears in the *Journ. Soc. Chemical Industry* (vol. xi., No. 2). Petroleum occurs in the "Carpathian Sandstones," the upper member of which is Eocene, the lower being Neocomian; in the upper Eocene or Oligocene; and in the Miocene beds. Ozokerite is found in small quantities in the older strata, but the main deposits are in the Miocene of Boryslaw. This material is extracted by shafts; while the petroleum is pumped from bore-holes. The strata are much folded, and are sometimes slightly inverted. The more productive areas for petroleum are frequently along anticlinal folds. Mr. Redwood's memoir is rendered much more valuable by the addition of a general map of Europe, on which all the principal petroleum fields are marked.

Something will happen to the President of the Royal Society, for has not Dr. Irving, in the June number of the *Geological Magazine*, remarked "there can be no necessity for pointing out the importance of this *dictum* from the pen of Lord Kelvin; it supports my own contention." It is not often that mortal man receives such a compliment as this.

In our May number (p. 169) we announced that a Catalogue of the Gasteropoda of the Inferior Oolite would shortly be published by Mr. Hudleston; we should have stated that the work is the joint production of Mr. Hudleston and Mr. E. Wilson, and that it is a Catalogue of the British Jurassic Gasteropoda.

The last part of the *Transactions of the Zoological Society* (vol. xiii., pp. 165–175, pl. xix., 1892) is devoted to a description by Mr. E. T. Newton of a finely-preserved skull of the gigantic extinct Beaver-like animal (*Trogontherium*) from the Forest-bed of the Norfolk Coast. As our readers are probably aware, the genus *Trogontherium* was originally described upon the evidence of a skull from the superficial deposits of Siberia described as long ago as the year 1809; and, although most of those entitled to speak with authority on the subject have been convinced of their identity, the remains of the giant beaver hitherto obtained from the Norfolk Forest-bed have been too imperfect to render it absolutely certain that they belonged to the same species as the Siberian animal. The skull now so ably described by Mr. Newton

is thus an opportune "find." As the result of an exhaustive examination, the author is convinced that not only is the Forest-bed animal specifically identical with the Trogonthere, but likewise with the creature found in the Upper Tertiary of St. Prest, in France, and described under the name of *Conodontes*. Mr. Newton is also careful to indicate the points in which the skull of the Trogonthere differs from that of the true Beaver (*Castor*); and the points of distinction are so great as to leave no manner of doubt as to the right of the two forms to be at least generically separated. The reason why a widely-distributed animal like the Trogonthere, apparently as well fitted to survive as its contemporaries the Beaver and the Desman, has become extinct, is not yet apparent.

THE scope for the operation of the principle of Natural Selection in a group of organisms so lowly as the Bacteria, may at first sight appear somewhat limited; but in an article just published by Professor G. Canestrini in the *Bull. Soc. Veneto-Trent. Sci. Nat.* (vol. v., pp. 85–100), the author points out that the range of possible variation is not so restricted as might be supposed. In the first place, the diminution in size of the organisms may count as an advantage; and the possible modes and manners of locomotion are sufficiently multifarious. The varying development of protective membranes and capsules may, in the Professor's opinion, afford much scope for the effective agency of Natural Selection—and so also may the numerous variations in the form, size, and capability of resistance of the spores. On the whole, it seems probable that Bacteria originated primitively as organisms living in the air; and, if that be the case, there are thus endless possibilities of modification (at least physiologically) as they become adapted for life respectively in animal tissues or fermenting and putrefying liquids. In any case, whatever be the *modus operandi* of the evolution, remarkable new forms are continually being discovered, and attention may be particularly directed to the newly-established genus *Nevskia*, found by Dr. A. Famintzine in the aquarium of the Botanical Laboratory of the Imperial Academy at St. Petersburg (*Bull. Acad. Imp. Sci. St.-Pétersb.*, n.s., vol. ii., No. 3). *Nevskia ramosa* is believed by its discoverer to be the first representative among the Schizomycetes of the colony-forming organisms with branched processes, of which the corresponding forms have long been known among Algæ and Infusoria.

THE "Société Scientifique du Chili," founded by a group of French residents in April, 1891, has issued the first fascicle of its "Actes." This is a work of 185 pages illustrated by 18 plates. The longest memoir is by M. F. Lataste, and is an "Etude sur la faune chilienne," and of it two parts have been issued dealing with the Lizards and Bats. The President of the Society, M. Obrecht,

contributes an interesting paper on the daily movements of the soil in Santiago, illustrated by 18 plates of diagrams. M. Philibert Germain describes a journey to Bolivia, in which he gives some interesting account of the Aymara Indians and the Lamas, and also gives a result of three months' work in the valley of Sipotuba, in the now revolted province of Matto Grosso. Some notes on the synonymy of some Chilian insects is the only paper published in Spanish. M. A. F. Noguès has a memoir on the "Genesis of Gold," than which no more appropriate subject for study could be found in a South American republic. The publications of the *Deutsche Verein im Santiago* are well known to all naturalists.

I.

The Story of Olenellus.

THE trilobite *Olenellus*, after forty years of comparative seclusion, has of late attracted the attention of geologists to a degree almost unprecedented among invertebrate forms of life. It has given its name to a "zone" which, accepted though it is by Messrs. J. E. Marr and H. B. Woodward, seems none the less of vast proportions when compared with our ordinary conceptions of fossil zones; it takes the first rank in a fauna that includes 150 species of other genera; and it now absorbs our thoughts when we behold miles of apparently unfossiliferous strata, or when we examine, for the hundredth time, obscure organic markings treasured in our own collections. At the outset this unassuming creature bore other names than *Olenellus*; it was regarded as a family-dependent rather than as an ancestor; but in due time field-observation has raised it to the most honourable position.

When Dr. Ebenezer Emmons, in 1844 (1, p. 21), figured a flattened and imperfect trilobite, and named it *Elliptocephala asaphoides*, from the elliptical marking formed by the eyes and a part of the glabella, he could not have foreseen the wealth of literature to which his discovery would give rise. In fact, *Elliptocephala* and *Atops* were the first two forms described from the now famous Olenellus-fauna, and the historic site of their discovery has been fixed with precision as near Reynold's Inn, west of North Greenwich, Washington County, New York State (19, p. 638). *Atops* as a genus has disappeared, and *Elliptocephala* was speedily absorbed into *Olenus* (Hall, 2, p. 256); but its lustre was only temporarily dimmed. After describing *Olenus thompsoni* and *Olenus vermontana*, from the shales of the Hudson River series at Georgia, Vermont (4, p. 60), James Hall published a revised figure of the former, accompanied by a restoration, and redescribed the species as the type of a new genus, *Barrandia* (5, p. 115). The species *vermontana* became also included in this genus; but its full beauty was unknown till later, earth-stresses having harshly dealt with the original, curtailing it of 19 segments. *Barrandia* was thus defined as possessing only 13 or 14 thoracic segments; but the great development of the third segment was rightly made a character distinguishing it from *Olenus*. Curiously enough, this feature is shown in Emmons's original figure of *Elliptocephala*

asaphoides, though it is not referred to by him; and this species seems to have remained as an *Olenus* until Ford investigated it in 1871 (7, p. 33, and 10, p. 257). Hall, in establishing *Barrandia*, insisted upon the form of the glabella, of nearly equal width throughout, and upon the narrow elongated pygidium, and commented on the difficulty of determining the facial sutures. Two years later (6) he found that the name *Barrandia* was pre-occupied, and reverted to what had been, indeed, his first conception, *Olenellus*.

Here, then, we have, in 1862, two well described species referred to the genus *Olenellus;* what was known as to their horizon? It was generally believed that the beds of Georgia in Vermont, with presumably corresponding strata on the Belle Isle Straits in Newfoundland, were above the horizons containing *Paradoxides*, and were referable alike to some part of the Potsdam series. The Georgia beds thus remained as Upper or Middle Cambrian until 1888, and the palæontological history of *Olenellus* is in consequence full of an unconscious humour. Hall (5, p. 115) prophetically remarked that "these forms"—*O. thompsoni* and *vermontana*—"will be found to mark an important horizon in our geological series," but this was on account of their association "with other forms that indicate the last appearance and final dying out of the types of that ancient crustacean fauna which marked, so far as we now know, the dawn of life upon our planet."

Mr. S. W. Ford (7) gave *Olenellus* additional interest by his researches among the rocks of New York State, and Brögger (8) observed of the Scandinavian *Paradoxides kjerulfi* that it seemed "significantly related to the American Cambrian species *P. thompsoni*"—the typical *Olenellus* of Hall. This apparently simple remark was the foundation, seventeen years ago, of the present high cult of *Olenellus;* it was taken up by Linnarsson (9), who emphasised the difficulty of seeing facial sutures in *P. kjerulfi;* and finally by Holm (13), who removed this species to *Olenellus*.

Meanwhile, Ford and Walcott in America fell into a trap that nature had provided for them, or rather remained in it unsuspectingly. The former (10, p. 256), treating of the old *Olenellus asaphoides* of Emmons, observed that it "may be safely regarded as higher in grade than any known form of *Paradoxides* whatsoever," and he preferred to keep it generically distinct from *Paradoxides kjerulfi*, which he looked on as its probable ancestor. His paper is eminently unassuming and fair-minded in tone; it leads from the small to the great, from the concrete to the abstract, from embryo-trilobites to the principles of descent; and it might be urged that here indeed we have zoological considerations confirming the work of the field-surveyor. Those who would absorb palæontology into zoology, instead of regarding it as a mutually helpful meeting-ground, might feel that William Smith's methods, based on laborious outdoor observation, were destined to find a formidable rival in the established principles of embryology.

Walcott (11, p. 166) assented to Ford's argument, remarking that *Olenellus* "expresses, in one of its species at least, the decadence" of the *Paradoxides* type. But he frankly admitted that *Olenellus gilberti* showed a permanent retention and great development of what were regarded as embryonic characters, and he accordingly recognised in this species a "retrogression." In the same paper an admirable amount of work is published on the Olenellus-fauna, regarded as Middle Cambrian; and a new genus, *Mesonacis* (11, p. 158), is put forward for *Olenellus vermontana* (Fig. 2), which now rejoices in its full 26 thoracic segments and a broad, not styliform, pygidium.

Into this scene of zoological and geological contentment Dr. Brögger suddenly discharged a bomb-shell. In a remarkable paper (12) on the Olenellus-zone in N. America, he pointed out that in Europe *Olenellus* preceded *Paradoxides*, and urged that, from a comparison of the faunas, made possible by the extensive work of Walcott, there was "no reasonable doubt" that the Olenellus-zone in America was also on the lowest level. Walcott, finding the New York area still difficult of interpretation, at once pushed his researches to Newfoundland, and gave us the complete section, in unbroken and undisturbed strata (14), at Manuel's Brook, Conception Bay. The American sequence straightened itself out; the Lower Cambrian of the Hudson River was shown to have been thrust by a profound fault over on to the Ordovician rocks (19, p. 526); and we have now the beds of Georgia, Vermont, with *Olenellus*, as Lower Cambrian; the St. John and Avalon beds, with *Paradoxides*, as Middle Cambrian; and the Potsdam and Belle Isle beds, with *Dikellocephalus* and *Olenus*, as Upper Cambrian. The basement-bed at Manuel's Brook is a coarse conglomerate, and rests directly upon "Algonkian" gneiss. Thanks to the intercommunication made possible by scientific journals, the work of Brögger and Holm in Scandinavia, and of Schmidt in Russia, thus rapidly bore fruit in North America; and *Olenellus* assumed its place as marking the earliest known fauna of the globe. The next fauna that may be found below it—and hints of this are already darkly scattered (22)—will be of true Precambrian age.

The triumph was to William Smith, a man grossly ignorant of phylogeny;[1] it only remained to re-explain the relationship of *Olenellus* and *Paradoxides* (16, p. 39). The child had proved to be the father; and the arguments in the contrary direction have been readjusted to everyone's satisfaction. It is a case of

"By the Lord, I knew ye, as well as he that made ye."

After this, we can only admire the courage of Messrs. Peach and Horne (24, p. 240) in suggesting that *Olenellus* is the central point in which the more modern trilobites, the Limuloids, and the Merostomata, converge.

[1] See his "Stratigraphical System of Organised Fossils," 1817, p. vii.

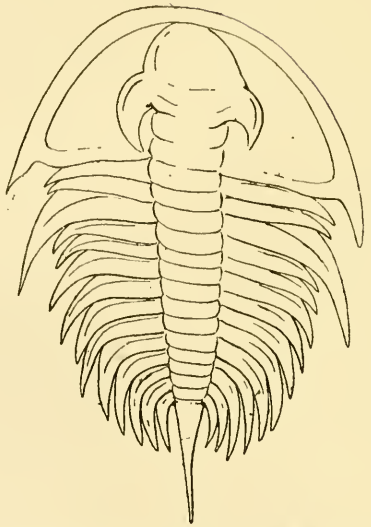

FIG. 1.—Outline of *Olenellus thompsoni*, Hall Parker's Quarry, Georgia township, Vermont. (After Walcott; size not stated.)

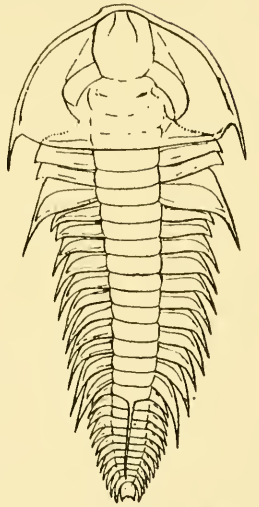

FIG. 2.—Outline of *Olenellus (Mesonacis) vermontana*, Hall Same locality. (After Walcott; size not stated.)

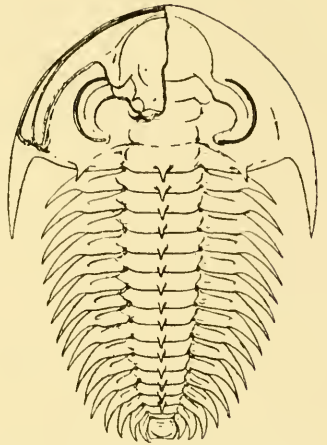

FIG. 3.—Outline of *Olenellus (Holmia) kjerulfi*, Linnarsson. Tomten, parish of Ringsaker, Norway. (Restoration, after Holm; × 1⅓. The hypostome is shown by removal of part of the head-shield; the glabella in *Holmia callavei* is distinctly narrowed in front.

Walcott, in his last great work on the Olenellus-fauna (19), abstains, somewhat generously, from breaking up this important genus; and most workers will approve his action in the present state of knowledge. Messrs. Peach and Horne (24, p. 236) also give a clear grouping of the eleven species already known. Whether *Olenellus* proper, *Mesonacis*, or *Holmia* (18), we find in all a semi-circular head-shield, prolonged into long or short posterior spines; a glabella of nearly equal width throughout, but often narrowed in front; large eyes; and no true facial sutures. The number of thoracic segments becomes of little value, ranging from 13 to 26, and it is interesting to find the characteristic head-shield in conjunction with a styliform pygidium in *Olenellus* proper, and a Paradoxidian pygidium in *Mesonacis* and *Holmia*. The great development and frequent prolongation of the third thoracic segment is not a constant character in the same species, though its occurrence in any specimen suggests that we are dealing with *Olenellus*. The surface of the whole test is often seen to be ornamented by a network of fine lines enclosing polygonal spaces, so that even fragments may thus be recognised.

The above details would be beyond our present purpose but that the search for *Olenellus* has already assumed vigorous proportions. Professor Lapworth (15) in 1888 announced the first discovery of the genus in the British Isles, near the base of the Comley or Hollybush Sandstone, at the foot of Little Caradoc in Shropshire. The species, *O.* (*Holmia*) *callavei*, has been figured and restored (20), so as to be worthy of its Scandinavian rivals, which its large size, six inches by four inches, practically equals. Professor Lapworth unerringly predicted that the Durness series of Scotland might also reach down to the base of the Cambrians; and in 1891 Sir A. Geikie (22) announced the discovery of *Olenellus* in the North-West Highlands. Messrs. Peach and Horne have now (24) given us a full account of the occurrence of a new species, *O. lapworthi*, in dark shales at the top of the "Serpulite Grits," and also in the underlying "Fucoid Beds" (so named from fucoidal casts of worm-burrows) on the Loch an Nid river in the south of Dundonnell Forest, Ross-shire. The tourist was formerly denied access to this rugged deer-forest, and Professor Heddle's kindly meant description of it was sternly excised from the visitors' book at the neighbouring hotel; but perhaps now the hunters of an invertebrate fauna may occasionally gain admittance. The Geological Survey is fortunately free in this respect; and we now know that the "Serpulite Grit," the "Fucoid Beds," and the quartzite forming the base of the Durness series, some 680 feet in all, are of Lower Cambrian age, and that "the Precambrian age of the Torridon Sandstone necessarily follows, the quartzites being unconformable upon it."

It is noteworthy that when Professor Jas. Nicol (3) made his famous statement that the lower gneiss of the Scottish Highlands might have been forced up to form the apparently overlying eastern

gneiss, he still regarded the Torridon sandstones as Devonian; but Murchison soon after referred these enormous masses, the bulwark of the west highland coast, to the Cambrian System. Professor Judd has, we believe, in his teaching uniformly regarded them as Pre-cambrian, an opinion now completely justified.

The Longmynd Series also gains in antiquity by the discovery of *Olenellus callavei* at a higher level; and it has been unhesitatingly placed as Precambrian by Professor Blake. Dr. Hicks (23) has claimed as *Olenellus*, from their characteristic surface-ornamentation, certain trilobitic remains from rocks some 3000 feet below the Menevian at St. David's; and he gives us in consequence a classification that may be stated thus:—

Upper Cambrian	{	Tremadoc Series.
		Lingula Flag ,,
Middle Cambrian	{	Menevian ,,
		Solva ,,
Lower Cambrian		Caerfai ,,

The triple palæontological division of the Cambrian into the *Olenellus*, *Paradoxides*, and *Olenus* zones is thus arranged for and accepted in Great Britain; Professor Lapworth (20, p. 533) would call the three divisions Taconian (the Olenellus-zone), Menevian (or Paradoxian) and Olenidian; and doubtless there will be some discussion and rectification of the limits of each division. Mr. Walcott (21, p. 376) already asks us to place half the Tremadoc Series in the Ordovician; and the story of *Olenellus*, so far as it has gone, is not calculated to give us confidence in a resting stage in classification. However grateful we may be for the literature showered on us, there is one severe critic (25) who would have preferred results to dissertations. The open country has been appealed to, the first and final arbiter in stratigraphical determination; discovery must precede discussion, and we are on the eve of notable discoveries in the Olenellus-beds of many lands. It is, after all, the Olenellus-fauna that should attract us, rather than any single genus; and its richness will be seen by all who examine Mr. Walcott's comprehensive memoir (19). We may in any case be sure that the roots of any genealogical trees that we may casually construct will be found to reach far down into yet more fascinating Precambrian zones.

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GRENVILLE A. J. COLE.

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HOVETON GREAT BROAD AND THE BURE.

The view is taken from the slope of the hills of Bure Valley Beds, capped by Glacial Drift, which form the southern margin of the valley of the Bure; the same beds are seen in the distance on the north side. In the immediate foreground is one arm of Salhouse Broad; the Bure flows between this and Hoveton Broad, from both of which it is separated by strips of "rand." The flat plain of alluvium marks the extent of the original Broad.

II.

The Physical Features of the Norfolk Broads.

THE rivers that drain the eastern slope of England have, on the whole, a somewhat similar course; they rise on the eastern side of the series of hills that forms the main watershed of the country, they flow with a gradually decreasing gradient till they expand into a triangular estuary widely open to the sea. Such is the case with the Tees, Tyne, and Thames, and the rivers that unite to form the Humber and the Orwell, or that flow into the Wash. In the case, however, of the Yare, and its tributaries the Waveney and the Bure, which drain the region between the Fenlands and the Orwell, the conditions are very different: instead of the large, broad-mouthed estuaries, the entrance is very narrow, and the river expands behind into an extensive sheet of water. The shoaling, moreover, has not resulted in a series of long banks parallel to the main flow of the water, as in the Thames, or in broad flats of marsh either on the sides as in the Humber, or at the head as in the Wash; the rivers flow through tracts of land formed, at least in the upper part of the course, more by the growth of water-plants than by the actual deposition of mud or sand; and, finally, the rivers remain comparatively insignificant streams, while the main water area is formed by the isolated sheets now so well known as the Norfolk Broads.

That the area over which these are distributed was once an estuary, similar to those of the Thames, Wash, and Humber, there can be no doubt, from reasons that will appear later. And, as the rivers that formed this estuary resemble in their source and general course the others of the east of England, it does not at first appear easy to see why, in this case alone, the estuary should have entirely silted up, or why, in so doing, it should have given rise to a type of scenery that is quite unique. In searching for an explanation, one naturally tries to obtain a first clue from the Broads themselves, by asking what is known of their origin. Etymology is at once ready with the answer that the name is derived from the Saxon *Braedan*, "to broaden," signifying that they are simply the broadenings of rivers, much as Windermere has gained its title of "the winding river lake." But a very brief visit to the area is sufficient to demonstrate the error of this view of the case. Having heard of the fame of Broad sunsets, the writer, on the evening of his arrival

in the district, started to row down the Bure, hoping to reach Wroxham Broad in time to enjoy, in the quiet of its reeds and rushes, a sunset of which the clouded glory was worthy of Constable's country. The Broad was less than a mile distant by land, but for far more than that length the river kept at its uniform canal-like width, without any sign of its intention of broadening out to satisfy the definition: it was obvious that something was wrong, and the quest was abandoned. Next day, from the deck of a wherry, the matter was explained. Instead of the river passing through the Broads, it kept sullenly aloof from them; as we sailed down the river there was Broad to right of us, Broad to left of us, Broad in front of us, but by a series of ingenious twists and turns it managed to wind through the whole lot of them, either eluding any direct contact with them, or communicating only by a few narrow and often overgrown passages (*see* Fig. 2). With some exceptions it is the same elsewhere: the Broads are usually completely isolated from the rivers, which may flow round three sides of them, as, *e.g.*, at Surlingham (Fig. 1).

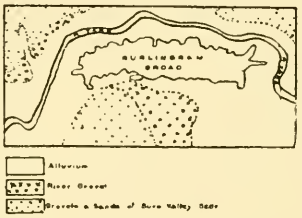

FIG. 1.—Surlingham Broad and its relations to the Yare. (1 inch to the mile.)

Etymology, in fact, was, in these cases, as misleading as usual, and was responsible for the very one explanation that was manifestly hopeless. The orthodox definition having thus collapsed, one was led to look around for some more reliable theory.

Lakes are usually classified[1] into six groups: (1) those occurring in rock basins, eroded by glaciers or formed by other agencies; (2) those due to the elevation of parts of the sea floor; (3) those lying along the depressions formed by the folding of rocks; (4) those due to the damming up of a valley by moraines or landslips; (5) those in extinct volcanic craters; and (6) those occupying mere hollows in clay. The Norfolk Broads are but slightly above, or are even below, the level of the sea, and are not separated from it by any rock rim, so they do not belong to either of the first two classes[2]: the valleys in which they lie are due to erosion through fairly horizontal strata, so

[1] *See, e.g.*, H. G. Seeley. Phillips' "Manual of Geology," ed. 2, 1885, pp. 134–7.

[2] A glacial origin has, however, been applied to the Broads (J. E. Taylor, the Norfolk Broads and Meres, Geologically Considered. *Trans. Norf. and Norwich Nat. Soc.*, vol. i., 1872, pp. 30–40), but the little evidence advanced in support of this conclusion is easily explained in other ways, and the direct evidence on the contrary is overwhelming.

that the rock foldings in the district will not help in the matter. The Broads do not bear any relation to the general glaciation, nor do they regularly shallow towards their lower ends in the manner characteristic of glacially eroded basins; there is not a trace of volcanic activity in the district, while the regular distribution of the Broads along the river valleys separate them from the irregularly scattered lakes of the last class, of which there are representatives in the area. The Broads, taken as a whole, therefore, do not fall under any of these six categories.

Though it is one article in the true geological faith that wherever there is land there is material for geological study, I had entered the district expecting to find much of interest in its flora and fauna but little in its geology; as far as previous experience went, flat tracts of alluvium, whether peat bogs as of Ireland or Sedgemoor, or deltas as of the Rhine, or river plains as of the Po, were, in spite of one's creed, of but little interest compared with the hills around them. Nor had a perusal of the literature of the area done anything to suggest that it would afford any very exciting problems; it is true that there had been a fierce and wordy war some 70 years ago, but the warmth of this was due to the fact that while one of the combatants was a geologist who scoffed at the historical "evidence" of his opponent, the other was an antiquarian who could not understand the scientific arguments of the former. In latter times an interesting lecture by Mr. H. B. Woodward, on "The Scenery of Norfolk," and the remarks in Mr. J. H. Blake's Survey Memoir, are the most valuable contributions, but in neither is the general question of the Broads discussed in any detail. It is to these authors, and to Mr. Clement Reid, that we owe the main part of our knowledge of the geology of the district.

The Broads themselves occur in a somewhat triangular area, with the coast line from Lowestoft to Horsey as the base and Norwich as the apex. A substratum of chalk occurs over the whole area, covered by the clays, &c., of the Lower Eocene along the eastern margin. Neither of these, however, are here seen at the surface, which is mainly occupied by gravels, glacial deposits, the alluvium of the old estuary, and the sand dunes that line the coast. Three rivers—the Bure, Yare, and Waveney—traverse the district. At first they flow through fairly steep valleys, which gradually widen out and unite into a broad level plain separated from the sea by a series of sand dunes or "meals," as they are locally called. Seen from the railway or the road the scenery is tame and uninteresting, the hills are low, the soil is heavy, and the valleys appear bare and flat. Viewed from the rivers or the Broads the whole aspect of the country changes; the hills, in contrast to the marsh that borders the waterways, now appear to stand out in bold relief, their slopes are well wooded, while clumps of fine oaks and elms mark out patches of gravel. The groves of lofty bulrushes that margin the

watercourses, the acres of flat-leaved pond weeds and water lilies that cover the Broads, and the dense tussocks of the sedge (*Carex paniculata*) that stand up like buoys to warn the sailor of the shoals, together form to the ordinary townsman a revelation of the charm and beauty of our water flora.

Reluctant though one may be to leave the water, it is necessary to do so if we desire a closer acquaintance with the geology of the district. Landing on the flat tract of meadow that borders the rivers, one gets an idea of its formation: the soil is everywhere damp and studgy, the surface is broken up by holes between the roots of the rushes or marsh plants, by the growth and decay of which these "rands" have been mainly formed. The hill-slopes, on the other hand, are composed of gravels and pebble beds, with occasional beds of fossiliferous ironstone, belonging to the Bure Valley beds, a series now of more than local interest since Professor Prestwich correlates it with the isolated patches of hill-gravel that stretch away to the south-west of England. Whatever may be the verdict upon this conclusion, there can be no doubt that the beds are here marine in origin, and that they were deposited in shallow water, probably in an estuary. Above these Bure Valley beds rests the series of sands, gravels, clays, and loams belonging to the Glacial Period: the chief of these is the Chalky Boulder Clay formed by the great ice-sheet that once covered East Anglia. The only bed later than these is the Alluvium, which forms the plains at the foot of the hill-slopes: but the glacial beds occur on the summits of the hills on either side of the valleys and rarely at lower levels within these; hence it is obvious that they once spread as a sheet over the whole district before the existence of the present valleys, only occasionally cutting down to lower levels. Now these valleys are of great depth; thus a boring at Yarmouth passed through 170 feet of drift[3] before it struck the London Clay, thus proving the existence there of a buried river channel 160 feet below the sea level, and therefore indicating a subsidence of the area to this extent. This might have been inferred, without any direct proof, from the fact that the valleys could not have been cut out by streams flowing with their present sluggish course. The whole fall of the Bure from Wroxham Bridge to Yarmouth is only about 2½ inches to the mile. An elevation of 160 feet would have turned the Bure into a torrent, with ample power for the erosion of its channel to this depth. As the excavation of the valleys commenced almost immediately after the close of the Glacial Period, it is quite probable that the elevation thus indicated was part of the great elevation which seems always to have accompanied, if not directly caused, the glaciation of temperate regions. The valleys were, however, certainly formed before the subsidence, so that when

[3] S. V. Wood has suggested that part of this may be really Crag (*Geol. Mag.*, iv., 1867, p. 560).

the land had reached its present level the area was occupied by a great estuary, the arms of which ran far up the courses of the three rivers. Probably before the subsidence had reached its maximum, the estuary had commenced to shoal, and in this case the silting was controlled by a factor which caused it to proceed on very different lines to those upon which the process has been carried out in other English estuaries. The nature of this factor we will now consider.

If one stands upon the sand dunes along the Norfolk coast as, *e.g.*, at Horsey, and looks out seawards, one notices lines of breakers parallel to the shore. These are due to shoals, and if we examine them, the coarsest material is found at their north-west end, indicating that the movement of the drift and beach material is here from north to south. The explanation of this movement must be sought in the peculiar circulation of the tide in the southern part of the North Sea: the main tide sweeps round the north of Scotland, and being concentrated by the contraction of the sea, it strikes with full force against the projecting coast of Norfolk, and drives along it to the south. When the northern tide begins to lose power, the southern tide that has been piled up in the English Channel forces its way into the North Sea, and as its passage through the Straits of Dover has given it a north-easterly set, it consequently follows the Continental coast. The circulation in the south part of the North Sea thus forms a great eddy, the tide sweeping south along the East Anglian coast, and returning along the opposite shores where the main projections, such as that of North Holland continued by Texel and other islands, point to the north. The fact that the tide practically always flows southward along this part of the English coast, has the effect of piling up the sediment on the north sides of the mouths of the rivers; and the gradual continuation of this action results in pushing them further and further to the south. Thus the Yare now opens at Gorleston, two miles to the south of Yarmouth, instead of its original mouth at Caistor, three miles to the north. Yarmouth itself was crossed during the passage, and the site of the present watering-place was a submerged shoal so late as 1000 A.D. Similarly the Alde, which opened at Aldeborough, on the north side of Orfordness, has cut a new course behind the London Clay hill at that point, and now opens to the south of it. The mouth of the Waveney was no doubt once at Lowestoft, but as this was barred, and as it was prevented travelling south by high land, it turned north and joined the Yare. The Hundred Stream is reported by tradition to have opened to the sea at Horsey, and such is by no means improbable; if so, its mouth was blocked, and it had to reverse its flow and become a mere tributary of the Bure.[4]

[4] Cases of the reversal of flow of rivers are rare, but well-authenticated ones are known. *See, e.g.*, the classical case of the Rhine, Sir A. C. Ramsey. *Quart. Journ. Geol. Soc.*, vol. xxx., 1874, p. 81.

The same southerly drift has affected other parts of the eastern coast: thus to it is due the great projection of Spurn Head; but in the case of the Humber, the river is sufficiently large to prevent its mouth from being seriously interfered with, and thus it has been but slightly deflected to the south. In the case of the Thames, the land to the north could not yield such masses of glacial boulders and other *débris* as the sea had to work with in Norfolk.

The influence of this barring of the rivers would be to at once check the velocity of the current at the mouth: the water striking the bar would immediately drop its load of *débris*, and thus strengthen the barrier by forming a shoal on the inner side. This would be built up further during floods, and by the growth of plants which would bind the loose sediment into a coherent mass, and by their decay gradually raise it above the water line. Thus, what would happen would be simply the silting up of the estuary at its mouth, instead of at its head as in the Wash, or along lines of still or "dead" water as in the Thames. This tract of silted land would, by the continuation of these operations, work its way gradually backward up the estuary, leaving a great sheet of water separated from the sea by a band of alluvium; of this Breydon Water may be the diminished representative. But as the land worked further backward, it would cross the entrance of branches of the estuary; the sediment would be carried along the central channel, upon the sides of which it would be deposited: it could thus cut off the branches either entirely, as in the case of Fritton Lake, or connected by a channel just sufficient for the escape of the surplus rainfall, as does the memorable Muck Fleet[5] for the three great sheets of Rollesby, Ormesby, and Filby Broads.

In such a way may be explained the origin of those Broads which lie in valleys off the main courses of the three rivers, but this will not suffice for those which lie along the rivers, and wholly surrounded by the alluvium. Let us take the most complex case. At the head of the alluvial plain of the Bure, at its junction with the narrow valley above Wroxham, there is a series of seven Broads, separated from one another and from the gravel hills on either side by tracts of ordinary "rand." The origin of the marginal rand is very simple: under the influence of the wind, rain, and frost, the pebbles constantly tend to roll down the hill sides into the Broad; upon the flat submerged bank thus formed, bulrushes and other plants would take root and help to raise it into a low terrace running at a uniform height along the foot of the hills.

The Bure must at this time have swept down the valley above Wroxham with considerable power, carrying with it much mud and sand. When the river struck the great Broad, which covered the

[5] "Fleet," it may be remarked, means ditch, and has no reference to the rate at which this Muck may be ascended.

site of the whole seven (*see* Fig. 2) its velocity and carrying power would be simultaneously reduced; it would, therefore, drop all its sediment as a delta or fan-shaped heap around its mouth. At times, when the river was low, this might tend to form a bar across its entrance, but during floods the river would unceremoniously re-cut its channel through this pile of *débris*, and spread the fan further outwards. As the fan grew, the whole face would not receive equal amounts of detritus; the middle would grow faster than the sides, and thus project into the Broads; rushes and plants growing on the shallow sides of the fan would help to complete the embankment, and confine the river to one narrow channel. Even during floods, when the river swept over the embankments, the stems of the dense rush grove would act as a sieve, and catch all the mud, and thus not only raise the main walls, but prevent the silting of the Broads on either side. All fresh supplies of sediment would, therefore, instead of being spread evenly over the floor, be piled up on either side of the mouth of the channel. In this way the embankment would in time

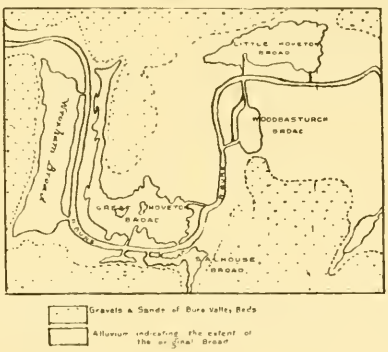
LITTLE HOVETON BROAD
HOCKERSTURGH BROAD
GREAT HOVETON BROAD
SALHOUSE BROAD
Gravels & Sands of Bure Valley Beds
Alluvium indicating the extent of the original Broad

FIG. 2.—The Hoveton Group of Broads; occurring in a tract of alluvium (unshaded) the whole of which was once occupied by the original Broad. (1 inch to the mile.)

run right across the large original Broad, dividing it into two smaller ones, separated by the river and its embankments. The actual line of the embankments would be deflected by the influx of lateral streams, or follow the main current when it diverged from a straight line to avoid higher land. Tributaries, moreover, would in the same way cut up the two Broads into yet smaller ones, and the size of the whole would be gradually reduced by the slow inward growth of the rands around the margins.

Various local modifications occur owing to the different directions in which the rivers entered the Broads: thus at Ranworth the stream struck the middle of an elongated Broad, and has cut it into two nearly equal lobes. At Surlingham (Fig. 1) the Yare was deflected sharply to the north by the hill to the south of the Broad, which marks the site of a sheet of "dead" water left in this angle.

If this theory be the true one, then the Broads are gradually decreasing in size, as is well-known to be the case, and those which

still communicate with the rivers tend to become completely isolated. In fact, the size of the Broads may be some measure of their age: as the silting commenced at the seaward end, the largest Broads would be expected to occur at the upper parts of the valleys. Such, indeed, is the case; thus the great Hoveton and Wroxham Broads on the Bure, Barton Broad on the Ant, Hickling Broad on the Hundred Stream, Surlingham Broad on the Yare, and Filby and its associated Broads on the Muck Fleet, are all in the upper part of the valleys. A gradual decrease can be traced as the Broads are followed towards the sea, of course excluding those off the main courses of the rivers: they become smaller, more regular in outline, completely isolated, and further removed from the rivers. Thus on the great flat plain through which the Ant flows south from Barton there are two small isolated Broads, one a mile to the west of the river, and the other half a mile to the east. Along the lower courses of the rivers there are numerous such remaining to bear witness to the great sheets of waters of which they are the diminished representatives, much as the small oceanic atolls remain to attest the presence of the extensive fringing reefs from which they have arisen.

The peculiar type of scenery of the Broad area is thus the outcome of exceptional physical conditions; but we are tempted, in conclusion, to inquire whether there is any geological evidence as to the occurrence of similar conditions in the past. In considering this point it may be advisable to attempt to form some idea of the evidence that would be preserved in this area upon which a future geologist would have to rely in trying to picture its present physiography. To do this let us return to Horsey, and note how the north-easterly winds are driving the sand dunes over the adjoining country. A section here would expose a basement bed of clay containing estuarine and freshwater fossils, with occasional bands of marine shells, and also the roots of aquatic plants or of trees that love a damp soil. Above the clay would be a seam of vegetable matter formed of the stems of similar plants, with occasional clumps of tree stems: the whole of this seam would be remarkably free from mechanically derived sediment, thanks to the sieve of bulrush stems that grew around the margin; this bed would be in turn covered by a mass of sand or sandstone. But this is simply the description of a coal seam with its root-bearing underclay and its sandstone roof. Further, the occasional submergence of the coal-field, as indicated by the layers of estuarine or marine shells, the general flatness of the area (upon which Croll has laid such stress), and the purity of the coal secured by a marginal filter of plant stems, like the mangrove swamps of the Mississippi (to quote Lyell's comparison), are all points of striking resemblance between the Broads and the conditions under which our coal-fields were deposited.

Hence, though the Broads may not tempt the geologist with any large series of instructive diagrammatic sections, nor yield to

the collector a wealth of fossils or minerals, yet it is not without interest of its own; it enables us to follow in detail the history of the infilling of a great estuary, and it presents us with, perhaps, the closest analogy to the conditions of the formation of our coal-fields. The Norfolk Broads, therefore, are not so much of value as affording an opportunity for the study of one part of the geological record, as in the lessons they teach as to how some parts of that record were written.

REFERENCES.

The following references may be some assistance to any who wish to follow the subject further. The most valuable contribution to the Robberds-Taylor controversy is the following: "On the Geology of East Norfolk; with Remarks upon the Hypothesis of Mr. J. W. Robberds respecting the Former Level of the German Ocean," by R. C. Taylor; published by J. Cochran, London, 1827 8vo. 68 pp., 14 pls (*See also Phil. Mag.*, vol. i., 1827, pp. 277–290, 346–353, and 426–432). The opposite side of the case will be found in the "Geological and Historical Observations on the Eastern Valleys of Norfolk," by J. W Robberds, Jun. 8vo. Norwich, 1826. The controversy was continued in a "Reply to Mr. R. C. Taylor's Remarks on my Hypothesis on the Former Level of the German Ocean." *Phil. Mag.*, vol. ii., 1827, pp. 192–206 and pp. 271–285 To this R. C. Taylor replied in two papers: "On the Natural Embankments formed against the German Ocean on the Norfolk and Suffolk coast, and the silting up of some of its Estuaries." *Phil. Mag.*, vol. ii., 1827, pp. 295–304; and "On the Geological Features of the Eastern Coast of England, and Concluding Remarks on Mr. Robberds' Hypothesis." *Ibid.*, pp. 327–331.

A list of the most important Broads and their dimensions are given by R. B. Grantham in "A Description of the Broads of E Norfolk," of which an abstract was published in the *Quart. Journ. Geol. Soc.*, vol. xxv., pp. 258–9. Further topographical details and clear proofs of the estuarine nature of the Broad area are given in "The Geology of the Country near Yarmouth and Lowestoft," by J. H. Blake. *Mem. Geol. Surv.*, sh. 67, London, 1890, pp. 3–5, 74–9; in "The Geology of the Country around Norwich," by H. B. Woodward, *Mem. Geol. Surv*, London, 1881, pp. 3–4, 143–4; in "The Scenery of Norfolk," by the same author, *Trans. Norfolk and Norwich Nat. Soc.*, vol. iii, p. 439; and again in his "Geology of England and Wales," ed. 2, 1887, pp. 594, 602–3. A general review of the glacial deposits of the area will be found in "The Glacial Drifts of Norfolk," also by H. B. Woodward, *Proc. Geol. Assoc.*, vol. ix, 1885, pp. 111–128. A summary of the literature on the Bure Valley beds will be found in Professor Prestwich's paper on the Westleton Beds *Quart. Journ. Geol. Soc.*, vol. xlvi., 1890, pp. 86–92.

J. W. GREGORY.

III.

The Evolution of Flat-Fish.

IN the May number of NATURAL SCIENCE, Mr. J. T. Cunningham criticised a preliminary note of mine in the *Société de Biologie* (16 Jan., 1892), dealing with a curious monstrosity in a turbot found at Wimereux. In this note, which my critic appears to have imperfectly read, I have carefully distinguished those *monstrous* Pleuronectids which show arrested development (without stoppage in growth) from the double flat-fishes whose existence Cunningham says is recorded in almost every ichthyological work of any importance.

The former are sufficiently rare to have been recorded carefully by the authors. *Pleuronectes cyclops* of Donovan is probably the first example cited. Couch speaks of a similar case among the turbots (*Rhombus maximus*). MacIntosh and, more recently, Filhol have also met with examples, while the case noted by Yarrell relates to a Brill (*Rhombus lævis*). The same monstrosity, according to Couch, is not very rare among *Zeugopterus punctatus*, and we know that *Zeugopterus* differs but little from *Rhombus*. It has also been observed among the flounders, whose development is as slow as that of the turbot. Higgins records, under the name of *Pleuronectes melanogaster*, a flounder notched above the eyes, and of a very dark brown colour, almost black, on both sides. Newman, Thompson, and Ritzema Bos have noticed the same malformation in individuals of this species. These monstrous specimens differ from the normal not only by the colouring of the blind side, but also by the notch in the dorsal fin, by the much greater thickness of the muscles on this side, which changes the form of the vertical section of the fish, by the existence in the turbot of tubercles like those on the upper side, and by other anatomical characters to which I shall allude later, in a special paper.

I have stated that these monstrosities, by arrest of development, are more frequently to be found among *Rhombus* than among the other Pleuronectids. I ought rather to have said among *Rhombus* and the flounders, or in a more general way, as I have indeed indicated, among the Pleuronectidæ of gradual metamorphosis (palingenetic development). The accuracy of this assertion is not only verified by the authors quoted, but also by the fact that since I drew the attention of the

members of the *Société de Biologie* to this question four months ago, two large monstrous turbots have been noted to me, one at St. Vaast la Hougue, the other at Le Croisic, by colleagues who only occasionally visit the sea.

Double specimens constitute an anomaly entirely distinct from the preceding, much less profound, and much more frequent, since they are to be found among all kinds of flat-fishes. It is almost impossible to visit the fish markets in the French seaports without finding one or several specimens. Among the turbot, double specimens, or at least more or less spotted examples, must have been so abundant at a certain time, or in certain localities, that, as I have formerly remarked, our earlier ichthyologists (Daubenton, Bonnaterre, Lacépède) considered the piebald coloration of the under side as a specific character of *Rhombus maximus*.

Among the flounders at Wimereux the average of individuals completely double is about three per cent., but it would be much higher if we included in our statistics the individuals merely piebald. Malm has already recorded this frequency of specimens coloured on the under side, and he attributed it to the fact that, as the flounders lived near the mouth of rivers, in water continually agitated, their young were unable to hold themselves constantly on one side or the other.

After having definitely established the distinction between the two anomalies (1st, the arrest of development; 2nd, the colouring of the blind side), I found it insufficient to limit myself to the statement, as my friend Pouchet has done recently, *that it is very frequent to find several anomalies on the same specimen.*[1] Indeed, if it is ordinarily thus, it is because a first deviation in the development often entails in consequence other modifications. I have, therefore, tried logically to make the second anomaly subordinate to the first, and it is in consequence of this that I have been led to speak of observations well known and easily to be duplicated, at least in many cases, in the metamorphosis of the Pleuronectids.

I said that the turbot of which I speak "must have swum (*devait nager*) in a vertical position, and must have rested rarely on its right side." I am not in the habit of affirming what I have not seen for myself, and, what is more, I had the testimony of two such experienced ichthyologists as Day and MacIntosh in support of my opinion. Cunningham merely denies this evidence with the unnecessary remark: "this is simply an error of observation." Now, on what facts does Cunningham base this contradiction of his predecessors? On the observation of a double plaice (not monstrous), and even *imperfectly* double, since the posterior three-fourths of the lower side are coloured in the same way as the upper, the anterior fourth being

[1] G. Pouchet, Remarque sur deux turbots à face nadirale pigmentée. *Société de Biologie*, 5 mars 1892, p. 200.

white.[2] But these specimens are very numerous; all the fishermen know them, and it is needless repetition to tell us that they lie perfectly horizontally on the ground. In spite of this, Cunningham remarks: "Giard assumes that double fishes swim vertically." What I said was, that not the *double* but the *monstrous* specimen (which is very different) swims vertically; besides which, I do not affirm that monstrous flat-fishes swim all their life vertically, but only that they remain longer in that position than the others, and, in any case, long enough to allow the influence of the light to act efficaciously on the side ordinarily colourless.

Cunningham tells us that he has made a series of careful experiments to prove the direct action of the light on the skin of the under side of the flounder. Obviously, I can only speak of those experiments results of which he has published (*Zoologischer Anzeiger*, 1891), and the method adopted in these does not seem to be satisfactory, for as long as the young flounders were not covered by a net, the experiment was not conclusive, and when they were held down by a net (the meshes of which must have been extremely fine, since they prevented the renewal of the water), the fishes died rapidly. This experiment forcibly reminds one of that of the man in the fable, who, wishing to habituate his ass to eat nothing, saw the animal die of hunger at the very moment that his experiment seemed likely to succeed. Cunningham tells us to-day what he had neglected to mention in his previous paper, that "at Plymouth reversed flounders are exceedingly common, almost as abundant as normal specimens," and that he cannot see what disturbing influence this fact can have on his own experiments. "It is obvious," says he, "that if inheritance in the flounders acts as Giard supposes, there would be none but double flounders, since right-sided and left-sided individuals are always breeding with one another." This is an astonishing misconception of the laws which govern the products of cross-breeding among animals. As all biologists know, the progeny obtained by the union of two races cannot be intermediate between the two progenitors, but resemble closely one or the other. For instance, in a union between a female white mouse and a wild gray male, one obtains constantly young, and some of whom are completely gray others entirely white. Also, we know that a certain anatomical character has a better chance of reappearing in the descendants when it has disappeared more recently, and it is evident that a right-sided flounder which can count a certain number of left-sided individuals among its ancestors, will have a greater tendency to acquire the colouring on its left side under the influence of the causes which have determined in the race the possession of this colouring.

[2] The position of the eye and the terminating comb of the dorsal fin are not of much signification in the plaice, where the eye is normally almost on the edge of the head, and the dorsal fin terminates almost behind the eye.

I consider necessary the introduction of assumptions indicating to biologists in what sense they should direct their observations or their experiments, and which may suggest these experiments. I have long since expressed my opinion on those naturalists who have not sufficient imagination to create for themselves assumptions, or enough courage to give vent to them. We must run the risk of making mistakes sometimes, and be always ready to abandon a theory which is proved incorrect.

But before discussing with Mr. Cunningham the explanation that I have given of the asymmetry of the Pleuronectids, and which has been accepted by so competent a zoologist as Professor A. Agassiz, I shall wait till my contradictor has brought me conclusive experiments, or new observations, and not merely sterile and destructive criticism.

A. GIARD.

IV.

Is Stigmaria a Root or a Rhizome?

IT is matter for congratulation that two such eminent authorities as Sir J. W. Dawson and Professor Williamson should have been moved simultaneously to a publication of their views on *Sigillaria* and *Stigmaria* (1). That this should have happened is of itself sufficient to show that the subject is an important one, and that its discussion at the present time is not inopportune. In again reverting to it, I do not propose to take up all the numerous questions it involves, and which, unfortunately, are not always clearly distinguished, but rather to deal briefly with one of them, viz., the question as to the morphological nature of *Stigmaria ficoides*, apart from its physiological functions. In doing so, no attempt will be made to formulate a final and decisive opinion, but simply to indicate the considerations that must be taken into account before the question can be regarded as settled. It may be that to many geologists and palæontologists a purely morphological question will possess but little interest, but to others, and especially to botanists, there will be no need to apologise for its discussion.

External Characters.—It has been pointed out again and again that the external characters of *Stigmaria ficoides* were considered by the older palæobotanists as inconsistent with the view that it is a root. So far as I know, no instance has yet been adduced in which a root bears rootlets arranged in a quincuncial order, and in which the rootlets become so regularly and so generally detached, leaving behind such well-defined scars as those met with in *Stigmaria ficoides*. Nor has any root hitherto been instanced in which the rootlets approach so closely to the growing point of the axis as they do in a specimen of *Stigmaria* described by Count Solms (2), where the appendages grow smaller and shorter towards the apex, the distances between them diminishing, while they become curved forward, and close up like a bud over the growing tip. The same authority points out that it is only as the appendages develop that they take up a position at right angles to the axis, so that they show the phenomena of epinasty and hyponasty like ordinary foliage leaves, phenomena which up to now have been unheard of in the case of rootlets.

In his communication to this Journal (3), Sir J. W. Dawson states that *Stigmariæ* "grew in the underclays or fossil soils, and that

their rootlets radiated in these soils in all directions." This may be accepted as a correct description of the state of affairs, but, as such, shows how much the so-called rootlets differ from those commonly met with in recent plants. In these, their positive geotropism induces them to grow vertically or obliquely downwards, no matter in what position they arise on the parent axis. In the case of horizontal rhizomes which give off rootlets from all sides—that of the common *Iris* for example—even the rootlets which spring from the upper surface soon curve over and grow downwards, and do not radiate "in all directions" as do the appendages of *Stigmaria*.

Finally, *Stigmaria ficoides* has *two* modes of branching, viz., by dichotomy and by the formation of lateral appendages at short intervals and on all sides. This is so unusual a phenomenon in the the case of roots that it is doubtful whether another instance can be cited where it occurs.

On the whole, then, it appears that the external characters of *Stigmaria ficoides* differ in several very important particulars from anything we are acquainted with in existing roots, and these differences do not seem to have been satisfactorily explained by those who advocate the view that it is a root. On the other hand, they are all in harmony with the hypothesis that it is a rhizome, and, indeed, so far as they go, give unqualified support to it.

Internal Anatomy.—Though not as completely known as could be wished, the internal anatomy of *Stigmaria ficoides* has been fairly well worked out by several investigators, especially by Williamson in his Monograph, published by the Palæontographical Society (4). As there described it possesses a parenchymatous pith, which at an early period becomes fistular, and this is surrounded by a zone of wood, made up of wedge-shaped masses of scalariform tissue, which are separated by medullary rays and were somewhat truncate at the apex. In the youngest axes, according to Williamson, the pith is surrounded by "a thin ring of very small vascular bundles," and the secondary increase in thickness is said to have been effected by means of a "meristem ring equivalent to a cambium zone" (5). It is important to observe, however, that the secondary increase proceeds outwards from the primary bundles, so that although the latter form the apices of the wood wedges, there is no line of demarcation between the primary and the secondary elements. Outside the zone of wood there are traces of a bast layer, which, however, is seldom preserved, and outside this again a cortex consisting of an inner and an outer portion, the former of which usually disappears with the bast to which it is contiguous.

Now, in this and other accounts given of the anatomy of the axes of *Stigmaria ficoides*, nothing is more striking to a botanist than the total absence of all those structural details which are everywhere recognised as morphologically characteristic of roots. There is no central cylinder with its pericycle and endodermis. There are no

centripetally-developed strands of xylem, nor any alternating strands of phloem. There is secondary growth in thickness, but it does not originate in the manner which is so characteristic of roots, but rather, so far as can be seen, in the way which prevails in stems. Hence it follows that anatomically there is scarcely a single character in the axes of *Stigmaria ficoides*, as found in this country, which, to a morphologist, is suggestive of a root.

Anatomy of the Appendages.—Some of those who have most confidently maintained the view that *Stigmaria ficoides* is a root, have naturally attached much importance to the anatomy of the appendages. I say naturally, because it cannot be denied that in some respects that anatomy bears some resemblance to what we find in some rootlets. Williamson has especially drawn attention (6) repeatedly to the fact that the single vascular bundle of the appendages has a likeness to the monarch bundles found in *Ophioglossum*, *Lycopodium*, *Selaginella*, and *Isoëtes*. Now, if it could be shown that this bundle is really homologous with a true monarch bundle, it would be a strong point in favour of the root hypothesis, *so far as it affects the appendages*. But there are difficulties in the way of doing this. In fact it *may* be a collateral bundle, and this seems to be the opinion of Count Solms (7). Secondly, its mode of origin differs widely from that of existing monarch bundles, which, according to Van Tieghem, arise in one of three ways (8). In most species of *Lycopodium*, by repeated dichotomy of the root trunk, the central vascular cylinder becomes reduced to the diarch type. At the next dichotomy each branch carries off half the xylem and half of each of the phloem strands, and these latter uniting, an arc of phloem is formed on one side of the xylem, and the monarch bundle is constituted. In *Lycopodium inundatum*, *L. selago*, some species of *Ophioglossum* and *Isoëtes*, the monarch bundle appears to arise from *a diarch bundle* by suppression of the phloem on one side of the xylem, while in *Selaginella* it sometimes arises by the suppression of one of the two xylem strands and the union of the two strands of phloem. Similar views, it may be added, appear to be held by Russow (9). Thus, however abnormal these roots may be in some respects, *they are perfectly normal in the penultimate stage, presenting the diarch structure* so common in Ferns, Equisetum, and other vascular Cryptogams. Now in *Stigmaria ficoides* the so-called monarch bundle of the appendages does not arise in any of these ways, nor does it originate from a diarch bundle. As described by Williamson (10), each bundle arises at the inner apex of a vascular wedge of the axis, and is presumably a derivative of one of the primary vascular bundles. In some cases, if not in all, the bundles share in the secondary increase of the axial bundles from which they arise, a peculiarity by no means of common occurrence. Apart from this peculiarity, the mode of origin of these bundles approaches much more closely to that met with in the bundles of leaves than to that found in rootlets which arise from a root. From

all this it seems reasonable to conclude that if not altogether nullified, the evidence afforded by these bundles in favour of the view that the appendages are rootlets is considerably weakened. Still, for the present, it may be well not to insist upon this and to leave the morphological nature of the appendages, so far as it is to be interpreted by structure, an open question. It hardly needs to be said that even if they should eventually turn out to be rootlets, that of itself would not be decisive of the nature of the axis.

Summarising what has been advanced, it may be said then that so far as external characters and internal anatomy are indications of the morphological nature of the members of a plant, the facts seem to point to the hypothesis that *Stigmaria ficoides* is a rhizome as the true one. No one at this day disputes the fact referred to by Sir J. W. Dawson and Professor Williamson, that *Sigillariæ* and other Carboniferous plants were continued at the base of the stem into *Stigmariæ*. But this throws no light on the morphological nature of the latter, although it may be conclusive enough as regards their physiological functions. As the matter presents itself to a botanist, it is for those who maintain the root hypothesis to explain how it happens that all the characteristic root structures are absent, while so many that are characteristic of stems are present. It may be urged that, in Carboniferous times, the laws of plant morphology were different from what they are to-day, and that we cannot interpret palæophytic structures in terms that are applicable to existing plants. To this it may be replied that a generalisation so far-reaching as this ought to be established on independent and indisputable evidence before it is applied deductively to the solution of a controverted question. As a matter of fact, palæobotanists do not act upon any such principle in dealing with the great majority of Carboniferous plants, but, on the contrary, continually base their inferences as to structure, affinities, modes of development, &c., on the assumption that the same general laws are applicable to the whole Vegetable Kingdom and to fossil as well as to recent forms. Another contention that may be advanced is that the various members of plants were not so sharply differentiated in Carboniferous times as they are to-day. This, at first sight, is a plausible contention, and deserves candid and impartial consideration. But care must be taken to avoid confusion in this matter between the plants of an earlier geological epoch and plants of a less complex organisation. Among recent plants, the distinction of root and shoot is well-known to be absent from the lower forms, and to be restricted to vascular Cryptogams and Spermaphytes. Hence there is no impropriety in saying that in the lower forms the members of the plant body are not so sharply differentiated as in higher types. But to apply this generally and without qualification to Carboniferous plants is a much more questionable proceeding. Much, indeed, if not everything, depends upon the type of plants that are under consideration. So far as I am aware,

no palæobotanist maintains that in the Ferns and Coniferæ of the Coal-measures we ought not to expect the usual morphological distinctions between the roots and the stems, and until positive proof is adduced to the contrary, it may be fairly assumed that in the fossil Lycopodiaceæ such distinctions are not altogether obliterated. If this reasoning be sound, then, whatever truth it may have elsewhere, the contention under discussion can scarcely be applied to explain the absence of all the characteristic root structures from the axis of *Stigmaria ficoides*.

It may be submitted, then, that the plant morphologist is within his rights in appealing to the structure of *Stigmaria ficoides* for assistance in the determination of its morphological nature, and in endeavouring to interpret that structure in accordance with the general laws of plant anatomy. When this is done, it appears, as I have endeavoured to show, that its organisation approaches closely to that of a stem, and has nothing in common with that of a root. Moreover, as we know from observation that it was not aërial, the conclusion is indicated that it is a rhizome. As was intimated at the outset, however, the formal and definitive assertion of this conclusion as an established truth is beyond my present purpose. Hitherto, the upholders of the root hypothesis have not dealt in detail with the facts to which attention has been called, and it remains to be seen, therefore, how far they can reconcile the one with the other.

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THOMAS HICK.

II.

A REPLY TO MR. HICK.

I fully agree with Mr. Hick when he intimates that morphoogical considerations have an equal, or even a greater value, than physiological, when endeavouring to ascertain the true nature of *Stigmaria ficoides*. But while admitting this, I am not prepared to follow him in appealing too strongly to conditions now found among living plants, and pushing them too far when endeavouring to explain the very different combinations that so abundantly present themselves among the Palæozoic forms. At that period no Angiosperms existed on the earth, and even the Gymnosperms were very far from reaching their modern development. Under these circumstances, the Cryptogams chiefly became the giant forest-trees of that remote age. To become such, they required an organisation very different in some respects from that of their degraded living representatives. Hence, we must not appeal to these degenerate types for illustrations and explanations of structures no longer existing. Still less must we turn to what we find in the Angiosperms, that wholly distinct race which has taken the place of the primæval Cryptogams in our woods.

The primæval giants of the swampy forests had doubtless a morphology assigned to them adapted to the physical conditions by which they were surrounded, but if even their dwarfed and otherwise modified descendants fail to throw light upon morphological details once so common, still less must we expect to obtain that light from the living and wholly different flowering plants. In this respect the *via media* is the safest. That these modern forms are in some line or other descended from the primæval types is accepted by every Darwinian. Hence, while we accept from the former any light that they can throw upon peculiarities in the latter, we must not deny that those peculiarities were real because they are no longer to be found among living plants.

The question before us seems, at the first glance, to be a very simple one, but it is less so than it really appears to be. Are our British forms of *Stigmaria roots* or *rhizomes?* We are soon involved in difficulties unless we disputants are agreed upon the sense in which we employ these two terms. To be certain on this point, I have selected the definitions of a rhizome given by Sachs, by Maout and Decaisne, and by Henfrey.

Sachs says: "It is of very common occurrence with Cryptogams and Angiosperms for a persistent primary axis or branch-system to continue to grow underground, and to send up only at intervals long

foliage-leaves or shoots, which subsequently disappear in their turn, and are replaced by others. When such axes or branch-systems lie horizontally or obliquely in the ground, *and produce lateral roots*, they are called rhizomes."—*Text-book of Botany*. 2nd Edition. English translation, p. 216.

Le Maout and Decaisne say: "The rhizome or root-stock is a stem which extends obliquely or horizontally below or on the surface of the ground, the advancing portion emitting fibrous roots, leaves, and shoots, the posterior gradually dying."—*Descriptive and Analytical Botany*. Hooker's translation, p. 11.

Henfrey says: "The *leaf-scaled stem*, found especially among herbaceous perennial plants, is seldom continuous with an axial root. On the other hand, it is very prone to produce adventitious roots, as is natural to its usually subterraneous or creeping mode of growth." "If the main axis persists, producing a few branches each year, and dying away behind but slowly, as it advances forwards, a more or less root-like structure is produced, termed a root-stock or rhizome."—*Elementary Course of Botany*, p. 25.

We have here three definitions from works and authors of the highest authority—one German, two Frenchmen, and one Englishman. I ask my opponents on what one point does our British *Stigmaria* correspond to any of these three?

Let us examine Mr. Hick's arguments on this matter.

External Character.—Even if no root is now to be found on the earth on which the rootlets are arranged in quincuncial order, it does not follow that such an arrangement could not formerly have occurred. We have now no aërial Reptiles with wings twenty feet across, nor archaic birds with their mouths full of true teeth; but no one dreams of advancing this as an argument against the former existence of the Pterodactyle and the *Hesperornis*. I will therefore dismiss this argument as valueless.

Mr. Hick's next two arguments seem to me to neutralise one another. In the first, the supposed rootlets cannot be rootlets because in a specimen seen by Graf Solms, as these rootlets approached the tip of the root, they bent towards its top, thus approximating towards the condition of the leaves of a bud. In the second he seizes upon Sir William Dawson's statement that the rootlets radiate in all directions, hence they differ from those "*commonly* met with in recent plants." What is or is not commonly met with cannot be of much importance to an argument relating to the *Stigmarian* question. I again leave these two opposite objections to neutralise one another.

Mr. Hick tells us that *Stigmaria* has two modes of branching, one by dichotomy, and the other by the development of the lateral appendages or rootlets in quincuncial order. I object to these latter organs being designated branches. Morphologically, it is not impossible that they may rather be regarded as highly developed and

symmetrical emergences; *functionally* they obviously performed the work of root-hairs; anyhow, they were not branches in the ordinary acceptation of the word, and in the sense in which the dichotomy of the large roots is a true ramification.

The external characters of *Stigmaria* "are all in harmony with the hypothesis that it is a rhizome, and, indeed, so far as they go, give unqualified support to it." I must confess that this statement of my old friend almost takes my breath away. It is difficult in a short controversial communication like this to go into the details of the subject, but I say unhesitatingly that, in my mind, and I think I know as well as most people what the detailed features of *Stigmaria* are, that no dogmatic statement could be further from the truth. I ask my readers to go over the three definitions of a rhizome quoted at the beginning of this communication. First and foremost we have always root, leaves, and, at intervals, growing buds, one of the latter especially being present at the apex of the rhizome, and that one usually being the largest and the strongest, while the retral end of the rhizome is usually either already dead and decayed, or in process of becoming so. Now I ask anyone who is unable to visit the museum of Owens College and thus see the magnificent specimen preserved there, to consult my Monograph in the publications of the Palæontographical Society. Though a poor substitute for the grandeur of the original as it is now preserved, let him refer to the photograph on plate xv., or to the diagram drawn to scale on page 46, and tell me where to find any one of the features of a rhizome. Instead of the retral death and decay of such structures, we have the four primary uncompressed roots, ancient as the central stem itself, and having a mean diameter of 32 inches, but the various divisions of which ultimately tapered to a very small diameter. On page 49 of the same Monograph the reader will learn how I traced on the spot one root of a similar but yet larger tree, along its subdivided and tapering length for 37 feet 4 inches, reduced at its apex to a mere point. What is there here corresponding to the swollen bud which occupies the distal lengthening extremity of a true rhizome? I further ask, Where are the lateral branches, each terminating also in a growing bud, seen in every rhizome?

In addition to this, what have we in the shape of appendages corresponding to the admixture of leafy growths forcing their way upwards, and of the roots bending geotropically downwards, to bury themselves in the soil? Not one of these features, characteristic of a rhizome, exists, and yet we are asked to believe that not only are these structures "all in harmony with the hypothesis that it is a rhizome, and, indeed, so far as they go, give unqualified support to it." Well might such a statement take my breath away! But I have not yet done with it. The question still to be considered, seeing that every other kind of appendage is absolutely absent from the Stigmarian root except what I affirm to be rootlets, is, What are the

facts relating to this part of the subject? Seeing, then, that *Stigmaria ficoides* is absolutely devoid of any and every kind of appendage excepting these organs, can anything but rootlets be made of them? Each one has the characteristic structure described in my Monograph. They scarcely present the variations that might be expected in a large number of individuals of the same organ. I have certainly studied hundreds of thousands of these objects, and I fearlessly affirm that, apart from the changes due to growth through long periods of time, the organisation of one is that of them all. Now, such being the case, are we to call these objects root-appendages or leaves, because they cannot be both? To begin with, their symmetrical quincuncial arrangement forbids this. If, then, such cannot be, if we determine to call them leaves, where are the roots of the rhizome? and, if they are rootlets, where are the leaves?

M. Renault boldly took the bull by the horns, and declared that some of them were the one and some the other; but this is sheer nonsense. Assuming, then, as I do, that they are rootlets, what is to be said for that argument? Mr. Hick refers to M. Van Tieghem on the subject; now it was from M. Van Tieghem's paper on Symmetry in Plants chiefly, I first learnt what is fundamental of my knowledge on the subject of monarch, diarch, and other similar modifications of this symmetry as it presents itself in roots and rootlets. At an early period of my work I satisfied myself that the rootlets of *Stigmaria* were examples of the monarch type of root. I at once sent him specimens of these Stigmarian rootlets. He was not long in replying "They are the monarch roots of a *Lycopodiaceous* plant;" a decisive reply of our highest authority on the subject. Mr. Hick thinks he has settled the question by expressing the suspicion that these rootlets may be collateral. It may strengthen his conviction when I tell him that they are collateral, so far as the bundle consists of a xylem and a phloem element arranged side by side, as he will see when he consults fig. 52 of my Monograph, but their development from a monarch tracheid, side by side with a phloem element, was followed by a subsequent series of processes that has no parallel in the living Lycopodiaceæ. The reason for this is obvious: the living forms have no true exogenous development of a vascular zone. No one worth naming now opposes the truth promulgated by me nearly twenty years ago. In most of, if not all, the Carboniferous Lycopodiaceæ, such a growth was developed on an extensive scale. The tree, originally an embryo produced from a minute macrospore, often lived to be more than a hundred feet in height, and its stem attained to more than three or four feet in diameter. Whatever may be the function of the vascular tissues of its rootlets in connection with the absorption of nutriment from the soil, they must have some function to perform. In a small seedling this work required but little machinery for its performance, but when the embryo became an arborescent structure even the rootlets were supplied with additional vessels, to enable

each one to take an increased share in the work. Little by little, as each new exogenous vascular zone was added to the entire exterior of the secondary wood, *which wood alone extended into the subterranean structures*, the upper margin of each pre-existing rootlet bundle received an additional vessel or two, developed in a continuous line from the periphery of the xylem zone, through the cortex, and terminated on the rootlet bundle, between the pre-existing tracheids and the phloem in contact with them. This continuous growth in size, and in the number of the tracheids constituting this rootlet bundle, is another of the many features of common occurrence among these Carboniferous plants which are never met with in any recent form. But these processes in no way make the rootlet bundle other than a monarch; and Mr. Hick himself admits that "If it could be shown that this bundle is really homologous with a true monarch bundle, it would be a strong point in favour of the root hypothesis." In this I agree with him. The root-bundles of the ancient and the modern types are as homologous as the stem structures of the two, *i.e.*, so far as homologues can exist between plants which, though belonging to the same great family, are, nevertheless, in some respects so differently organised.

I have dealt thus far with Mr. Hick's arguments because they are but the superficial echoes of the ideas of M. Renault, the high priest of the doctrines which I reject; who, in calling *Stigmaria* a rhizome, has constantly in view the angiospermous structures so named, with which it has no kind of relationship. M. Renault declares that *Stigmaria*, like the mint or the water-lily, develops leaves and shoot-producing buds, as well as rootlets. I deny as emphatically as ever that our British *Stigmariæ* do anything like this; hence it neither is, nor can be, a rhizome in the sense in which these writers use that term. But I have long had in mind another much more complex aspect which these *Stigmariæ* present, and which has not yet received the attention that it demands. The combined peculiarities presented by this remarkable organism seem to me to demand a special and different nomenclature. In my next communication on the subject I hope to present these hitherto undiscussed aspects in a definite form, whatever may be their true value.

W. C. Williamson.

III.

A REJOINDER.

There are only two or three points in Professor Williamson's reply which seem to call for immediate comment, and this, of necessity, must be extremely limited.

(1.) The specimen in the Manchester Museum at Owens College does not seem to me inconsistent with the statement that "the external characters of *Stigmaria* are all in harmony with the hypothesis that it is a rhizome." Much depends upon the interpretation put upon its various features, and it is here that the divergence of opinion really begins. To show, however, that I am not alone in my estimate of the significance of these characters, a further reference may be made to Count Solms' "Fossil Botany" (p. 288), where substantially the same view is put forward.

(2.) My references to Van Tieghem are to his latest views on the origin of the vascular bundles which plant anatomists regard as being *truly monarch*. *Apart from the mode of origin*, these bundles are scarcely, if at all, distinguishable from collateral bundles. I do not gather, from Professor Williamson's statement, whether Van Tieghem was acquainted with the development of the bundles of the appendages of *Stigmaria* when he pronounced them to be monarch. But it may be mentioned that Count Solms, who is familiar both with their structure and development, writes of them as follows ("Fossil Botany," p. 277):—

> "That the bundles in this, as in the preceding case, are *collateral* and capable of secondary growth, is evident at once from the inspection of a transverse section figured by Williamson." (Monograph of *Stigmaria ficoides*, pl. xi., fig. 61.) [Italics mine.]

(3.) As the authority for the more important statements made in my article is given in every case, it seems both unnecessary and undesirable to introduce the name of M. Renault, who is not referred to in any way, and who is certainly not to be made responsible for what I have written. Not less desirable is it to avoid mixing up the question of the morphological nature of *Stigmaria* with a discussion of the systematic position and affinities of the plants to which it belonged.

THOMAS HICK.

V.

Agricultural Museums.

THE good old days, when it was thought sufficient if a man were able to do his work in a rough and unintelligent way, are passing by. The mere acquaintance with routine and practical details, which served the fathers, will no longer do for the children, and the head must be brought into play as well as the hands. The apprenticeship, which used to be everything, is only a part of the training for the various branches of industry. The advance of Science, and the wider application of scientific methods, together with foreign competition, are responsible for much of this. Be the cause what it may, we are entering on a new era; and the change is little short of a revolution. Mechanics' and technical institutes are being opened in all the large centres, where the youth of the place may go in the evening and, under skilled instructors, learn something about the tools they work with and the forces they use. And the movement has excited considerable interest, and is likely to be increasingly taken advantage of.

This is in the right direction, competition or no competition. It will not only produce better results, but also give the workman more pleasure in his work. Probably the time is not far distant when it will be insisted upon as a necessary part of a man's training, and when the apprentice will be released for part of the day in order that he may acquire it. This will come about in quite a natural way when it is found out that the man who knows what he is about has an advantage over the rest. Such institutions are only possible where the population is concentrated, and in such places they will doubtless soon be put on the safe basis of permanent endowment or assured annual income from some public source.

The County Councils have taken up the idea, and are applying it as far as it can be carried out in outlying districts. These bodies are wisely seeking to bring within reach of the ploughman, and even of the farmer himself, a more intimate and scientific acquaintance with field and farmyard. Perhaps nothing has advanced more rapidly in recent years than the Science of Agriculture, or rather the application of the sciences to agriculture. We are already in the habit of talking of the farmer of the old school as a being very much behind the times. The treatment of the soil and the raising of

the stock are capable of being conducted in a much more efficient way, if but the requisite knowledge be there. The question is, how to provide this for the dwellers in our thinly scattered farms and cottages. The present system of itinerant lecturers visiting in succession the country villages, and gathering an audience from the surrounding district into some school-room or other available place, is on its trial. In a few places it has succeeded in attracting and even interesting those it was meant for. In the majority of instances, perhaps, it has failed, often miserably failed, to rouse the bucolic mind from its lethargy, or draw it aside from its usual method of spending its evenings. It may be that country people are not quite awake to what this invasion of their solitude means, and have not yet come to any conclusion as to what sort of creature this new kind of packman is. Some seem to be more amused than edified, and are accused of the levity of a passing joke at the man of science. The scheme is certainly open to the objection that they are scarcely prepared for it, especially when the lecturer insists on airing his superior information; and also to the suspicion in the most successful cases of leaving little permanent influence behind. Those who have tried have sadly to confess how little they can teach to any purpose even in a session of continuous work, and are disposed to be sceptical of the fruit of ten or twelve lectures hastily cast on such rough soil.

It is easier, however, to criticise than to suggest anything which will serve the purpose better. The want is a difficult one to meet and itinerancy seems the only solution in the meantime. When our educational machinery is completed, perhaps technical as well as secondary instruction will be provided in country schools.

As an auxiliary to this or any other scheme, would it not be possible to have a museum in which the facts could be presented to the eye? In addition to illustrating the lecture, it would remain a permanent element when the lecturer had departed. There are few country districts where a room might not be had which could be opened every evening, after six or seven o'clock, for those who cared to visit it and spend an hour there. Not a general museum, which is only a sort of show place—general things seldom do very much good, but a strictly agricultural museum, containing only what is interesting and valuable to country people, arranged in an easily intelligible way. The rent of such a place would be trifling, and the trouble of getting the things together and grouping them would not be great.

This paper is intended to show not how such a museum might be laid out, but how it is actually being laid out in the neighbourhood of Dundee. At least a start has been made, whatever the end may be.

Plainly, the thing to begin with is the soil. That is the basis of the whole matter. To the ordinary farmer, much more to the ordinary ploughman, soils are soils. They need ploughing, manuring,

and reaping at the proper seasons, and that is all. But, possibly, a little more intimate acquaintance with them would be found to repay the trouble. Now this museum of ours brings clearly out the fundamental fact that soils are the outcome of the disintegration of rocks, and it does this in the simplest manner possible, *i.e.*, by showing every stage in the process from the rock to the resultant soil. The parent rocks may be distant, as in the case of alluvial soils, or the glacial subsoils with which the lowlands of this district are so largely covered, but, near or distant, the relationship exists. The scheme reaches even further back than the rock. Some rocks are of comparatively simple structure—for instance, Sandstone; others are comparatively complex, as Gneiss, and Granite. In the first row of boxes are placed the minerals which enter into the composition of the common rocks, as rock crystal, calcite, felspar, hornblende, mica, augite, olivine. In the second row follow a few of the typical rocks, built up out of one or more of these minerals, those common to the district, as Old Red Sandstone, quartz, limestone, gneiss, granite, dolerite, being preferred. A few of these typical rocks have been selected to illustrate the process of decomposition. In succeeding rows of boxes the gradual formation of the more familiar soils is illustrated, and it is shown whether this happens with or without chemical change. Sandstone, dolerite, and granite already occupy the fourth, fifth, and sixth rows, and the other rocks of the county remain to be added.

The composition of the minerals which make up the rocks is clearly stated both in words and in chemical symbols, and every stage in the progress of decay is made plain in the same way. It. thus appears that soils are largely made up of alumina and silica According to the preponderance of one or other of these, the soil is known as sandy or clayey. When neither is in excess the result is a loam. In addition, there are often minute varying proportions of iron, lime, manganese, sodium, potassium, sulphur, and in some cases phosphorus. In the last of these boxes, illustrating the process of decomposition, the surface layer, the soil proper—darkened by the decay of animal and vegetable organisms (humus)—is placed. A rough outline of one of these cases which will show the arrangement followed is given on the next page:—

Minerals.		Typical Rocks.	Decomposition and formation of soils.		
Rock Crystal—oxide of silicon, SiO_2.	Augite, &c.	Old Red Sandstone, SiO_2.	Dolerite—original rock.	Granite, &c.	Sandstone, &c.
Calcite—carbonate of lime, $CaCo_3$	Olivine, &c.	Limestone, $CaCo_3$.	Passing into diabase. Lime in felspar and augite has united with Co_2 to form $CaCo_3$, &c.	&c.	&c.
Orthoclase—potash, felspar, silicate of alumina and potash, $Al_2O_3, 3\,SiO_2, +K_2O, 3\,SiO_2$.	&c.	Mica schist, mica, and quartz.	More advanced stage, augite almost completely decomposed.	&c.	&c.
Mica—silicate of alumina, potash, and peroxide of iron, $3\,Al_2O_3, SiO_2, +K_2O, 3\,SiO_2$.	&c	Gneiss, mica, felspar, quartz. —— Granite, mica, felspar, quartz.	Yellowish brown grit or earth formed by chemical decomposition.	&c.	&c.
Hornblende, &c.	&c.	Dolerite, felspar, augite, olivine, &c.	Soil from near the surface, darkened by decayed vegetable matter.	&c.	&c.

In a second case an attempt will be made to bring out in as clear a way as possible the relation of the soil to the plant and animal life of the farm. Obviously organisms live and grow because they get out of the earth and air something which nourishes them. The most satisfactory way perhaps of finding out what that something is is by analysis.

Some such arrangement as the following will be adopted as a kind of educational experiment. The first row of boxes will contain the inorganic elements, metals, and non-metals found in the soils and also in the substance of plants and animals, such, for instance, as the calcium and potassium of felspar, the iron of augite and olivine, and the phosphorus of wheat and animal tissues. These will be arranged according to their greater or less abundance. The soils will follow in the next row, the order indicating the variety of their constituents and their consequent fertilising power. Heading the others will be placed an alluvial soil, which, having been brought from a distance, is probably the waste of many different kinds of rocks, whose ingredients have been finely sifted and mingled by water. Then the dolerite, which, when thoroughly decomposed and allowed to accumulate, is sufficiently complex, and therefore fertile, down to the comparatively homogeneous chalk or barren sand.

In a third row of boxes the soils will be arranged with a view of showing what they are specially rich or deficient in. Then will follow the analyses of the different crops raised, and over against each will be placed the special manure, or manures, needed to mee their special wants. The scheme is rudely indicated in the accompanying plan :—

Inorganic Substances, Metal and Non-Metal, in Plants and Animals.	Soils.		Analyses of	
	Comparative fertility.	Composition.		
Sodium Na	Alluvial, very fertile	Clay rich in potash ·76 phosphoric acid H_3PO_4 ·16	Wheat.	Special Manure
Phosphorus P	Volcanic, fertile	Sand silica 95% K 07 H_3PO_4 ·10 Lime 1·8	Oats	Do.
Potassium K	&c.	&c.	Barley	Do.
Sulphur S	Calcareous, comparatively poor	Chalk, rich in lime 30%	Turnips	Do.
Calcium Ca Iron Fe., &c.	Sandy, barren	Peat, organic matter in excess	Clover	Do.

It remains to make the acquaintance of the live stock. One can scarcely imagine any intelligent farmer or ploughman who does not wish to learn all he can about his chief property. The farm animals are not very difficult to understand; and it ought not to be an impossible task to represent the main features of their structure and relations to the eye. The facts that the hoof is an enlarged nail-covering over the first joint of the finger or toe, and that the so-called "knee" is really the ankle or wrist, and the lower part of the leg beneath "the knee" represents the bone or bones in the substance of the hand or foot metacarpal or metatarsal, and that the animal therefore walks on the tip of the finger or toe, ought all to be clearly expressed. The common ancestor of all the ungulates, with its five toes or digits on each foot, and their gradual reduction to four in the Rhinoceros, ought also to be indicated.

The bearing of the character of the toes on classification should also be clearly shown, with examples of the odd-toed group, the perissodactyla, and the even-toed group, or artiodactyla. The other characters by which this is generally accompanied might also be illustrated.

For instance, in the odd-toed ungulates the stomach is simple, while in the even-toed it is more or less complex; in the former the dentition is usually complete, in the latter it is usually incomplete·

The museum will present all the peculiarities of these animals by a very simple process of grouping. The odd-toed horse will come first with its simple stomach and its four kinds of teeth. The even-toed sheep (ruminant) will be last with its highly complex stomach and very incomplete dentition; and between them will be placed the omnivorous pig, with its very slightly-divided stomach and its almost complete dentition.

This is only the nucleus of a teaching museum, and manifestly a great many things remain to be added before any approach to completeness is made. A small case may profitably be used to make those interested better acquainted with the different grasses grown in the fields and their relative value for grazing purposes. In the present state of information it would be a risky experiment to ask the ordinary agriculturist to point out the perennial rye grass (*Lolium perenne*) and to indicate its exact position in the order. In this district there is no commoner grass in our hay-fields than Yorkshire fog (*Holcus lunatus*), which only ranks as a weed grass, and whose presence indicates poor land or bad tillage.

Another case may be made to indicate the natural use of food-stuffs with a view of producing certain definite results; and still another devoted to fruit culture as a subsidiary branch of farming, and one which is likely to rise in importance if small holdings become general. Even the arrangement suggested is by no means final. Details might be added or subtracted, and a fresh grouping followed as a better way suggested itself. It is not necessary that one should be a repetition of the other; indeed, it would be a weakness and a hindrance if it should be so. The local conditions of farming differ, and will suggest that greater prominence should be given to one thing in one district and to another elsewhere. All that is meant here is to point out the use such an institution might serve in the enlightenment of farmers and ploughmen, in the economy of material by the more accurate adaptation of means to ends, and in the consequent spread of scientific agriculture.

J. H. CRAWFORD.

VI.

Amber and Fossil Plants.

OF the various media of fossilisation there are few, if any, which afford us such vivid glimpses of the period in which the organisms preserved as fossils formed part of the existing faunas and floras, as Amber and its included organic remains. To most people the fact of insects being found in Amber is well known, but few, possibly, are aware of the botanical treasures which have been found embedded and almost perfectly preserved in this substance. Plant fragments sealed up in fossil resin, with the minutest detail of structure intact, reveal many facts of no little interest from a botanical and geological point of view: they afford us, moreover, a faithful record of the conditions which obtained in those forests whose trees supplied the preserving material.

To briefly refer to a few of the plants and plant fragments found in Amber, and note what conclusions may be drawn from their examination, may not be without interest.

Our knowledge of the Amber plants is due mainly to the labours of Goeppert, Menge, and Berendt; and in more recent years Conwentz has contributed exceedingly interesting and valuable memoirs on the same subject.

The substance Amber, first mentioned by Homer, began to attract attention in 600 B.C., when Thales, of Miletus, one of the Seven Sages, discovered its remarkable properties from which sprang the Science of Electricity, so called from the Greek term for Amber, ἤλεκτρον. Without attempting to trace the history of Amber and of the various theories as to its true nature, we may note that, even in the time of Aristotle and Pliny, guesses were made as to its source of origin which came very near the truth. Under the general term *Amber* are included a number of fossil resins whose chemical and physical properties need not be considered in detail. The following varieties are mentioned by Conwentz (2):—*Gedanite*, *Glessite*, *Stantienit*, *Beckerite*, and *Succinite*. *Succinite*, the commonest and best known of these, is generally described as a resin exuded from an extinct species of Pine, *Pinus succinifera* (Goepp.). Doubtless this species of Conifer was only one of several which yielded this particular kind of resin. The chemical composition of Succinite is given by

Helm[1] as follows:—Carbon, 78·63 %; hydrogen, 10·48 %; oxygen, 10·47 %; and sulphur, 0·42 %. Its specific gravity is from 1·05 to 1·09, and hardness from 2 to 3; slightly brittle, and with marked conchoidal fracture.

From the days of Phœnician traders up to the present time, the Baltic coasts have supplied by far the largest quantities of Amber; the present annual yield in Prussia alone being estimated at a value of three million marks (6, p. 828).

The Baltic Amber occurs in a rolled condition in lower Oligocene strata. Credner, in his "Elemente der Geologie" (1, p. 700), quotes a typical section from Runge, giving the following succession, in descending order—(1) Surface soil, (2) "Diluvium," (3) Brown-coal, (4) Glauconitic Amber-bearing beds. The strata rich in Amber have an average thickness of 1·3 to 1·7 metres, and are for the most part below sea-level. Along with the rolled pieces of Amber in the glauconitic sand and clays occur various marine mollusca whose remains became embedded in the sediment, mixed with the hardened resins and vegetable *débris* carried down by rivers which flowed through the Pine Forests of Scandinavia and Finland. Washed out of the cliffs by marine denudation, the Amber is picked up at ebb tide or collected in nets, and by other means at high tide; in some localities it is obtained by mining.

From the abundant occurrence of this fossil resin in the drift of Jutland, Denmark, Sweden, Holland, and other districts, it is considered probable that the amberiferous strata formerly extended over a wide area in Northern Europe.

Although the Königsberg district is much the richest, other localities and other geological horizons have yielded Amber in greater or less abundance. From the Greensand of North America, from Miocene beds in Sicily, from Tertiary strata of France (Loire Department and Paris Clay), from Basle, Galicia, Roumania, Greenland, the shores of Spain, and other regions, Amber has been obtained in varying quantities.

In England this fossil resin has been found in the Cambridge Greensand, the London Clay in the neighbourhood of Kensington, on the shores of Sussex and Essex, and in the Pliocene beds of Norfolk, no doubt, as Mr. Clement Reid has suggested in the case of the Cromer "Forest bed" (10, p. 451), washed out of older strata.

The various fossil resins were orginally poured out from the tissues or sealed up in the cells and resin ducts of coniferous trees which covered a wide area of Northern Europe during the Eocene period.

Of this "Amber vegetation" we find an abundant supply of samples, both in the form of incrustations and petrifactions. In

[1] References to papers referring to Amber may be found in the works of Goeppert, Menge, and Conwentz, mentioned at the end of this paper.

many cases, where the resin has acted as an agent of petrifaction, we have plant-tissues preserved in wonderful perfection, sometimes rendering visible even nuclei in delicate parenchymatous cells, and giving us an insight into the minute anatomical structure of these Tertiary fossil plants, even more complete than that presented by the silicified Palæozoic plants of Grand Croix and Chemnitz, or the calcified remains in our English Coal-measures. In the form of an incrustation, Amber has no less distinctly preserved fragments of twigs, leaves, buds, and flowers; waifs and strays of Tertiary floras caught up in the slowly flowing resin or blown by wind and carried by animals on to the surface of the sticky exudations of resinous tissues.

The Amber which has rendered possible these microscopical examinations of fragments of a former vegetation, was present in the form of resin in ducts and receptacles scattered through the cortical and woody tissues of Conifers. In living trees, such as Pinus and many other Conifers, resinous secretions occur in lysigenous and schizogenous spaces; spaces, that is, due either to the absorption of groups of cells or to the gradual separation of uninjured cells. Resin is present, both as a normal product of the vital activity of healthy plants, and, in still greater abundance, as the result of wounds or injuries of various kinds which stimulate the secreting cells to abnormal activity and an increased production of resin. This abnormal occurrence of resin in living trees has been called by Frank *Resinosis*; for a similar phenomenon in the "Amber trees" Conwentz (2) proposes the term *Succinosis*.

Not only has the Amber been furnished by resins in horizontal and vertical ducts and spaces scattered through cortex, wood, medullary rays, and pith, but occasionally the well-preserved sections of Amber trees have revealed the existence of special groups of parenchymatous cells in the middle of woody tissues, and which have been set apart for resinous secretions. Occasionally, too, succinosis is seen in old tracheids of the wood whose membranes and cavities have become filled with resin, or in an unusually large number of schizogenous spaces in various parts of stems; the result of injurious atmospheric or organic influences which have made extra demands upon the resin supplies. If we picture to ourselves the thick Eocene forests in which animal and plant life was luxuriant and Nature worked with a free hand, it is not surprising to find that resinous material was produced on a larger scale than in the Pine woods of modern Europe where the forester's art and the altered climate have lessened the severity of the struggle for existence.

Dr. Conwentz, of Dantzig, whose recent monograph gives us full descriptions of the coniferous trees, the minute anatomical structure of which has been so clearly preserved by the resinous impregnations, prepared himself for the task of reading the Amber records by a careful study of the conditions of forest-life to-day, especially in

such cases where Pine woods spread unhindered over wide tracts of country. We may note some of the results obtained by an application of experience gained in recent forests, to the interpretation of the fragmentary relics of Tertiary forests.

In modern forests a variety of causes produce resinous exudations; resin ducts and receptacles are tapped by branches falling from the lower parts of stems where they are unable to secure their proper share of light and nourishment, and eventually decay and fall. A falling tree crashes through the branches of its neighbours; wind, storms, and lightning tear off branches or splinter stems. From all such causes resin receptacles are opened up and exudation results. The scars left by broken branches invite the attacks of animals and parasitic plants, which, in their turn, injure the living tissues and increase the flow of resin, which, softened by the sun's heat, may spread over the surface of branches and stems, enclosing in its course splinters and loose fragments of wood, and sealing them up, to be eventually entombed in hard, clear Amber. Insects, flowers, leaves, twigs, and light fragments, are carried by wind or other agency to the soft resin and embedded in it.

In addition to the Amber formed in this way, it has frequently happened that resin has been hardened and preserved in the ducts or cells from which it had never exuded. To compare the included insects and plants with living species is fairly simple, and such a comparison gives us a good idea as to the life in the Amber woods.

Leaving out of consideration the fossil insects, and avoiding detailed lists of plants, we may briefly refer to some of the better known or more interesting genera. Abundant evidence is afforded by plants of various families that the Amber flora flourished in a subtropical climate.

Beginning with Dicotyledons, we note the presence of such familiar genera as Fagus, Acer, Castanea, and Quercus, represented by leaves, flowers, fruits, &c.

In addition to these genera there are many others belonging to the same class, of which we may note some of the more familiar and interesting families.

CUPULIFERÆ.—Specimens of *Quercus* are common in the Baltic Amber; for the determinations of the various species we are chiefly indebted to Caspary and Conwentz. The pendant male inflorescence of *Quercus piligera* (Conw.) figured by Conwentz (3, vol. ii.) is a striking example of a fossil whose surface characters are seen through the clear Amber scarcely less distinctly than if a fresh flower were before us. In the same Family we have four species of *Castanea*, also leaves and floral organs of *Fagus*.

DILLENIACEÆ.—This tropical and Australian Family has a representative in *Hibbertia*, a genus native in extra-tropical Australia, whose leathery leaves with grooved and hairy lower surface are well shown in Conwentz's figures.

MAGNOLIACEÆ.—Two genera, *Magnolilepis* (Conw.) and *Magnoliphyllum* (Conw.), are referred to this tropical Family. In the LINACEÆ we have fragments of one species *Linum oligocenicum* (Conw.).

GERANIACEÆ.—*Geranium* and *Erodium* are both included by Conwentz in his list of Amber Angiosperms. Schenk has expressed a doubt as to Conwentz's determination of these genera; if they are rightly determined they are the first fossil examples of this temperate and subtropical Family.

ACERINEÆ.—Five species of *Acer* are mentioned by Conwentz.

SAXIFRAGACEÆ.—Two genera, *Stephanostemon* (Casp.) and *Adenanthemum* (Conw.), have been instituted to include Amber fossils referred to this Family. The recent genus *Deutzia*, native in China, Japan, and the Himalayas, is detected by the occurrence of flowers with characteristic stamens.

MYRICACEÆ.—A species of *Myrica* and a new genus *Myricophyllum* (Conw.) represent the two contributions of Amber to this Family.

AQUIFOLIACEÆ.—Three species of *Ilex* have been determined from well-preserved flowers.

LAURACEÆ.—The tropical and subtropical genus *Cinnamomum* has left remarkably distinct traces of its existence in the Amber flora. Specimens of exceptional beauty are figured by Conwentz (3, vol. ii.), in which are clearly shown the small valves or lids characteristic of the anthers of this Family.

EUPHORBIACEÆ.—The recent genus *Antidesma*, characteristic of warm latitudes, occurs for the first time in a fossil state as a well-preserved male flower in Oligocene Amber. Previous to this determination, the existence of Euphorbiaceæ in Tertiary times had been conjectured from leaf fragments which afforded evidence of little value.

PROTEACEÆ.—Species of *Persoonia*, *Lomatites*, and *Dryandra* are quoted by Conwentz as examples of this Australian and South African Family.

ROSACEÆ.—A flower included in Amber has been referred to this Family as a new genus *Mengea* (Conw.).

ERICACEÆ.—Leaf-bearing axes and fruits of *Andromeda*, a North American and North Asiatic plant, occur in the fossil resin; other genera also are referred to the same Family.

CAPRIFOLIACEÆ.—Two species of *Sambucus* are figured with floral organs in good preservation.

LORANTHACEÆ.—Representatives of the Mistletoe Family occur in *Patzea* (Casp.) and *Loranthacites* (Conw.). The former genus, originally described by Goeppert and Berendt as Ephedrites, and considered to be closely related to the recent genus *Ephedra*, has since been recognised by Conwentz, Caspary, and Schenk as one of the Loranthaceæ. Its characters are well illustrated in the figures of flowers and flowering branches given by Schenk in "Zittel's

Handbuch" (7, p. 713). This extinct form *Patzea* has its nearest living representative in the genus *Arceuthobium*, a parasitic Angiosperm of the Mediterranean, North American, and other regions. The oldest known examples of the Loranthaceæ are those mentioned by Saporta in the list of fossil plants from Lower Cretaceous strata of Portugal.

Among the Monocotyledons we may mention representatives of four families.

Liliaceæ.—*Smilax* has been detected from a well-preserved female flower.

Palmæ.—Four species of Palms are included in the Amber Monocotyledons, belonging to the genera *Phœnix*, *Sabalites* (Casp.), *Bembergia* (Casp.), and *Palmophyllum* (Conw.).

Araceæ and Gramineæ.—Three new genera have been instituted to include fragments referred to these two families.

We have not to trust, in the case of Amber flowering plants, merely to doubtful determinations based upon fragmentary leaves, but are dealing with flowers and fruits which the Amber has sealed up, and in a condition which allows of little or no doubt as to their true botanical affinities. To form some idea of the perfect state of these flowers, buds, leaves, and twigs, one has only to look at the excellent figures of Conwentz and previous writers, in which are seen sepals, petals, stamens, and pistils still in their places, and apparently as complete and distinct as if just gathered from the living plants. Another interesting example of flowers which have left similar relics of their delicate structures is that of the Sezanne tuffs of Eocene age (8, p. 6), in which perfect moulds have been left by the embedding and subsequent removal of the flowers in a calcareous matrix : wax introduced under an air-pump into the moulds, and after the removal by acid of the surrounding travertine, forms casts of the floral organs of the flowers. Specimens of these wax casts are doubtless familiar to those who have examined the collections of fossil plants in some of the Paris Museums.

Gymnosperms are especially abundant in the Baltic Amber, and the coniferous stems, roots, and branches furnish us with striking examples of Amber petrifactions. In the form of large pieces, or as small fragments, the wood and bark of Pines and other Coniferæ have been thoroughly impregnated with resin, which in its Amber condition allows us to examine microscopically the most minute structures in the tissues of these Amber trees. In addition to the vegetative parts of Conifers, male and female flowers, and even pollen-grains, are frequently found.

Many of the Amber Conifers belong to genera which have retreated since Tertiary times to more congenial extra-European latitudes.

Male flowers of Abietaceæ are mentioned by Conwentz (2) as common, and their frequent occurrence is explained by the fact that

they fall from the trees after their function has been fulfilled, at a time when resinous exudations would be plentiful, and in a condition suitable for the reception and embedding of foreign bodies.

The Californian genus *Sequoia*, the Japanese genera *Sciadopitys* and *Libocedrus*, the Swamp-Cypress (*Taxodium distichum*) and others afford examples of Conifers lost to Europe since the Tertiary period. In addition to those already alluded to, species occur of *Thuja*, *Abies*, *Juniperites*, *Picea*, *Cupressus* and other genera.

The great majority of the fragments of coniferous wood from the Baltic Amber have been referred to *Pinus succinifera*, a species whose various tissues have been fully described by Conwentz and previous writers; in certain anatomical details this species resembles varieties of the recent Conifer *Pinus laricio*, but in certain points stands alone as a characteristic and extremely common Pine of the Amber Flora.

From an examination of the forms and structural details of fragments or splinters of the wood of Pinus and other trees, it has been shown by Conwentz how it is possible to arrive at conclusions in many cases as to the conditions under which the splinters were formed. For such a work as this a close study of living trees is a necessary preparation. Pieces of wood torn off stems and branches by the impact of falling trees, or broken off by wind, have certain peculiarities in shape and microscopic structure which distinguish them from splinters struck off by lightning: the action of the latter is recognised by the torn membranes of the wood elements, and by the general form of the splinters, the action of wind and falling trees, &c., splits off fragments of wood whose component elements are for the most part whole.

Occasionally pieces of Amber show distinct signs of burning, suggesting by their charred appearance the burning of forest fires ignited by lightning flashes. In looking through microscopic preparations of English Coal-measure plants, one sometimes notices tissues in which the cell-walls are black and apparently charred, strikingly suggestive of the action of fire (9 *β*, Pl. 33, fig. 19).

Another phenomenon which is brought out by the microscopical examination of tissues in Amber is named by Conwentz (2) "*Vergrauung*." This consists in the loosening or partial separation of tracheids imparting to the surface of the wood a certain woolly or hairy texture. The same phenomenon may be seen on the exposed surface of Pine-wood shingles used in the roofs of country houses in the hilly districts of Germany and other countries; the fresh smooth surfaces after a time lose their freshness and become woolly as the result of this "Vergrauung" process.

Instances are recorded of double "annual" rings in stems and Amber trees. Among living trees, Kny (5) has described a case of double rings in *Tilia parvifolia*, whose leaves were eaten by caterpillars about the end of Juné, and this caused a premature development of the next year's buds. The branches on which these leaves had

been unfolded showed in the year's wood two rings instead of one. These double rings in the Amber woods may probably be referred to some cause similar to the one noticed by Kny. *Parasitic Fungi* are common in the tissues of the various trees ; their abundance points to a plentiful supply of moisture, and a temperature warm and congenial for such parasites. Among other Fungi, *Polyporus* and *Trametes* have left characteristic traces in the Baltic Conifers. Not only are their hyphæ found ramifying in tracheids and cells, but signs of their ravages are apparent in the broken-down elements of wood and cortex. Spores of Fungi distributed by wind and animals would readily effect an entrance into the tissues of their hosts, where fallen branches or the gnawing of animals exposed unprotected places on stems, roots, and branches.

Not only are there abundant records of parasitic Fungi in the Amber plants, but remains also of *Saprophytes* have been found in fragments of fallen and partially-decayed stems and branches.

Hyphæ of fossil Fungi have been noticed in fossil tissues of various geological ages ; in the Coal-measures one finds them occasionally in the cells and tracheids of Lepidodendra and other plants. The well-known *Peronosporites antiquarius*, figured and described by more than one writer, is a good example of a Palæozoic Fungus (9 *a*).

It is not the object of this paper to enumerate all the organisms of whose presence in the Eocene Forests Amber affords direct or indirect evidence. The existence of numerous spiders and insects, such as Butterfly larvæ and Beetles, is shown both by their remains sealed up in the fossil resin, and by their excreta and traces of their activity in borings or other markings in the woody and cortical tissues. In many microscopic slices of coal plants we may detect proofs of the existence of xylophagous animals in their excreta lying in cavities eaten out of the sound tissue (11). Higher animals have left equally distinct signs of their existence, sometimes in the form of hairs, or, in one or two cases, feathers embedded in Amber, sometimes in characteristic markings on bark and wood, and in the abnormal growth of tissues consequent upon wounds inflicted by animals in search of food.

Among other animals, the Squirrel, and a bird closely allied to the recent Woodpecker, may be mentioned as two examples of the Forest fauna which have been recognised in Baltic Amber.

From this general and incomplete sketch of Amber and some of its included fossils, we realise, to some extent, how important have been the investigations of the resinous petrifactions and incrustations as a means of focussing before us the life and conditions of Eocene forests whose remains have afforded such abundant supplies of Amber in the Oligocene beds of the German Samland.

In the ingenious methods employed by Conwentz in his microscopical examination of tissues fossilised in Amber, we have another instance of the value of Palæobotanical work. When supported by a

wide knowledge of recent vegetation, it affords not only a general view of ancient life, but enables us to sketch out the biological and physical conditions which made up the life of Tertiary forests whose scattered fragments were swept along by streams and rivers, to be eventually buried in marine sediment. Their subsequent gradual upraisal has brought them to light as relics of a past age, whose records, if read aright, carry us back to a geological period when Europe presented an aspect very different to that under which we are accustomed to regard it.

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A. C. SEWARD.

SOME NEW BOOKS.

A Naturalist in the Transvaal. By W. L. Distant. 8vo. Pp. xvi. and 277, with thirteen plates (four coloured) and other illustrations. London: R. H. Porter, 1892. Price 21s.

In this book, Mr. Distant gives us the results of his leisure during twelve months spent in the Transvaal on business. South Africa is a well-worked field, but Mr. Distant's observations have brought to light many new and interesting facts, while his collections have largely increased our knowledge of the insect-fauna of the district. The new species are described in an appendix to which many specialists have contributed. The only new vertebrate is a snake (*Glauconia distanti*, Blgr.), the greatest amount of attention having, naturally, been given by Mr. Distant to the insects, in several orders of which numerous forms found by him have been hitherto unknown.

The change which has come over the modern naturalist's view of nature is emphasised in the following passage:—"I could not help contrasting the different mental conceptions which dominated me when collecting in the Malay Peninsula twenty-two years previously, and those which now dominated my mind in a similar quest at Durban. Then, almost the sole aim was the discovery of new species; now, the constant wish was to make some small discovery to add to the ever-increasing knowledge of how animals derived their present shape and coloration in the struggle for existence." Accordingly, our author has given us various notes on protective resemblance and mimicry. The butterfly *Hamanumida dædalus* has been observed in West Africa to rest on the ground with closed wings the under surfaces of which vary in their markings to harmonise with different soils. In South Africa, however, Mr. Distant always found this insect resting with expanded wings, so that the upper surface was exposed, and assimilated to the colour of the greyish rocks and paths. The well-known *Danais chrysippus* was found in abundance accompanied by *Hypolimnas misippus*, whose female is so perfect a mimic of it. The distasteful qualities and warning coloration of the *Danais* afford it such protection that Mr. Distant enquires why it does not absolutely swarm, and suggests "some inherent weakness or danger which produces great mortality in the early stages." An enemy of this butterfly has been discovered by Mr. Distant in one of his new orthopterous insects, *Hemisega prædatoria*, which lurks among the tops of tall grasses to which it bears "aggressive" resemblance, and is thus enabled to seize the *Danais* when it settles on the flower. One specimen of *D. chrysippus* was obtained with wings mutilated as if by the bite of a bird, which was probably driven by hunger in the dry season, when insects are very scarce, to attempt a meal on the nauseous butterfly.

The appearance of insects as the wet season comes on is described with much interesting detail, and the contrast drawn between the

"veld" in drought and in rain is striking. The sudden rise of the watercourses ("spruits") is often a source of danger to travellers, though it must afford a grand object-lesson to geologists on the force of denuding agencies.

There are some notes of interest on birds. Mr. Distant tells us that the reputed fine of fifty pounds for killing the snake-eating Secretary-Bird (*Serpentarius secretarius*) is a myth, and that one was secured after three miles' chase on horseback, a fact which shows that the bird can run sometimes at least without breaking its legs. The heavy flight of the male Widow-Bird (*Chera progne*) in the breeding season is noted, its long tail being a great encumbrance, and, according to Mr. Distant, an example of the production of a disadvantageous structure by sexual selection.

On mammals, Mr. Distant has not much to say, except to deplore their extinction. The former magnificent antelope-fauna has been almost destroyed by the persistent attacks of the Boers, and the carnivora have necessarily much decreased in numbers. The prevalence of spiny acacias and hard-wooded trees is the only sign to-day that multitudes of herbivorous mammals once dwelt in the land.

Savage and civilised men alike receive attention. The Magwamba Kafirs are described in some detail, and Mr. Distant has formed a favourable opinion of the native character. A chapter is devoted to the Boer of whom our author gives what seems to be a fair and impartial criticism. It is not the Boers, however, but the English and recent Dutch and other European settlers who will make the future prosperity of the Transvaal; and, curiously, the modern Dutch emigrant is hated by the Boer. The gold and diamond mines have attracted some very undesirable speculators into the country, and some of the stories of gains and losses told by Mr. Distant are interesting and instructive. His sketches of colonial character are fresh and racy.

There is an evolution in human dress as well as in animal organs, and we are told that, in Pretoria, tall white hats distinguish lawyers and doctors, while the President is the only wearer of a tall black one!

The book is well got up, and the illustrations are very good. It is rather startling, however, to read (p. 107) about "*Galileos* who cared for none of these things"! G. H. C.

BREHM'S TIERLEBEN.—Third edition. Edited by Professor Dr. Pechuel-Loesche. —Die Kriechtiere und Lurche. By Dr. Alfred C. Brehm. New edition by Professor Dr. O. Boettger and Professor Dr. Pechuel-Loesche. 8vo. Pp. 825, with 167 engravings, 1 map, and 16 plates. Leipzig and Vienna: Bibliographisches Institut, 1892.

A THIRD edition of the celebrated work, "Brehm's Tierleben," is now in course of publication, under the able editorship of Professor Pechuel-Loesche, and we have just received the seventh volume, dealing with the Reptiles and Batrachians. For the preparation of this volume the editor has had the good fortune to secure the co-operation of Professor Oscar Boettger, one of the most distinguished of modern herpetologists. To Brehm's admirable descriptions of the habits of these animals, Professor Boettger has added much of his own observations, and brought the work thoroughly up to date as regards the systematic aspect, and thus raised it much above the usual standard of popular Natural Histories. In fact, as it now appears, Brehm's volume on the Reptiles gives not only an unequalled account of the

habits of this interesting group of vertebrates, but serves as a most useful book of reference to all interested in the classification and geographical distribution of recent Reptiles and Batrachians. So thoroughly has the revision of this volume been carried out, that even the results of observations recorded within the last few months have found their way into its pages. So often do we find books of a popular character inaccurate and behind date, that we cannot speak

FIG. 1.—*Psammodromus algirus*, from the Western Coasts of the Mediterranean. The specimen figured above represents the recently described *var. nollii*, from Algeria.

too highly of the value of a work such as the present one, which combines elegance of style with the strictest scientific accuracy.

We are pleased to find the classification in the volume before us conform to that adopted in the rearrangement of the collections in the British Museum. Thus, Snakes are no longer primarily divided into poisonous and harmless; the Slow-worm finds its place near the Sheltopusik and the Glass-snake of North America,

instead of being associated with the Scinks; and adaptations to the different modes of life are considered as subordinate in classifying the Chelonians and Batrachians. The characters of all families (with but a few exceptions) and higher groups are given; and the maps, showing the geographical distributions of the various groups, will prove most useful, drawn up as they are by so competent an authority as Dr. Boettger from the latest data available.

FIG. 2.—*Ceratobatrachus guentheri*, the horned frog of the Solomon Islands, discovered by Dr. H. B. Guppy.

A large number of new figures, mostly from the skilled pencil of Mützel (two of which we are able to reproduce through the courtesy of the publishers), have been added, some to replace less satisfactory ones in the previous edition, others to represent little-known or newly-discovered forms. Among the new figures, we notice excellent representations of *Anguis fragilis*, *Rana arvalis*, and other German species, all of which are now figured in the "Tierleben." A few of these

new engravings may, however, evoke some criticisms. It is doubtful, for instance, whether the representation of *Rana agilis* will afford much help in the recognition of this rare frog in Germany; and the new figure of *Heloderma*, the poisonous Lizard, which represents *H. suspectum* (not *H. horridum*, as stated in the letterpress), is inferior to the old one. The figure given on p. 146 as *Lacerta agilis* is surely taken, magnified, from *L. vivipara*, as correctly stated in the former editions; the snake on p. 467 is apparently *Vipera russellii*, not *Ancistrodon rhodostoma*; and the representation of *Ceratophrys boiei* on p. 690, stated to be of the natural size, is much enlarged, the frog, so far as we know, not exceeding a length of 3½ inches.

PROPERTY: ITS ORIGIN AND DEVELOPMENT. By Ch. Letourneau. (The Contemporary Science Series.) 8vo. Pp. xii., 401. London: Walter Scott, Ltd., 1892. Price 3s. 6d.

IF this work dealt with the origin and development of property simply from an historical point of view, it would merit no notice in this magazine. Professor Letourneau, however, is an anthropologist of the modern school, fully alive to the value of the ethnographic method, which, in the spirit of Natural Science, seeks to explain the evolution of social institutions by an appeal to the manners and customs of the lower races. What these races now are, such it may be assumed in broad terms our ancestors once were. Those who refuse adhesion to this fundamental tenet need not concern themselves with the study of the present volume; since, disagreeing radically with the author's method of enquiry, they will, of necessity, refuse to accept his conclusions. Those, on the other hand, who have faith in the simple method of seeking to interpret the unknown past of our civilisation by a consideration of the present condition of less civilised peoples, will find in Professor Letourneau's volume a storehouse of information and suggestion on some of the most interesting problems of sociology.

The desire of appropriation, whence springs the origin of property, seems referable to the imperious instinct of self-preservation, and is consequently common enough in the animal world. Man is proverbially an acquisitive animal, but other animals are largely acquisitive, and the naturalist will have no difficulty in multiplying the instances cited in the early part of this volume. When the shrike forms a collection of impaled victims, the creature provides, with instinctive foresight, for the wants of the morrow, much as a savage may hoard his food when fortune brings him more than is required to supply his daily need. House property is also common among birds and other animals, and even the possession of landed property is recognised in a certain sense. A strong animal, like a lion, may lay claim to a certain area as its hunting-ground, and be prepared to defend his right, in truly leonine fashion, against all intruders. Primitive man, on the contrary, would generally be too weak, single-handed, to defend his own area against the invader; hence he was led to associate with his fellows for the defence of common property. Isolation meant death; weakness led to union, and union to strength. Once in possession of conscious strength, a man's egoism asserts itself, and, gradually becoming forgetful of his obligations to his associates, he grows discontented with communal property and yearns for private possession. One man,

stronger than the rest, gains the ascendancy; individualism triumphs over altruism, and private ownership of land is established.

Private property was probably, in the first instance, of a strictly personal character. The weapons and ornaments which a man had made for himself were clearly his own belongings, and were consequently buried with him for service in the next world. When man passed from the condition of a hunter to that of a herdsman, his property took the convenient form of domesticated animals. Passing, in the course of ages, from this nomadic life to the settled life of an agriculturist, he acquired landed property which, as already explained, would be at first of a communal character. The solidarity developed in a village community led to a certain kind of altruism, and thus acted as a check upon egoism. But eventually an inverse process would be carried out; the soil became parcelled out into allotments, the cultivator enjoying merely the usufruct of his plot. Gradually, however, the chieftains would manage to transfer this right into property transmissible to their descendants, and such a practice would be imitated as far as possible by the more powerful of the community.

With the historic phases of the subject, such as the development of the feudal system, we have no concern in these pages; neither do we feel at liberty to refer to the author's views on the future of the right of property. For these, and many other topics, we must refer to M. Letourneau's volume—a volume which will be read with much interest by all who are busied with the fascinating problems of evolutionary sociology.

The Optical Indicatrix, and the Transmission of Light in Crystals. By L. Fletcher, M.A., F.R.S. London: Henry Frowde, 1892.

Every geologist who occupies himself with the study of rocks is familiar with the practical determination of minerals by their optical characters, and has found it necessary to acquire some knowledge of Fresnel's wave-surface; he is also probably aware that the method used by Fresnel in his marvellous memoir on double refraction has long been regarded as dynamically unsound, and that the nature of the luminiferous ether has lately been the subject of much investigation and discussion on the part of physicists.

Now, in his quest for instruction regarding the optical characters of crystals, the student is first confronted with the elastic properties of the luminiferous ether, and then with the deduction of the wave-surface by Fresnel's method; and yet if he has ever endeavoured to do so, he has probably failed to form any distinct conception of the plane waves on which that method is based.

A further difficulty will encumber the progress of any thoughtful student when he discovers that various elastic theories of the luminiferous ether have been devised, and yet that they all lead to the same wave-surface.

Mr. Fletcher's little book will not attract the attention of the general reader, because, being specially intended for the student, it is thrown mainly into a mathematical form; it is, therefore, all the more desirable to indicate the educational value of this extremely ingenious treatise.

The book is nothing less than a bold rejection of the recognised methods of teaching this subject, and the substitution of a new and original treatment of striking geometrical elegance which involves

no higher mathematical knowledge than appertains to the study of the ellipsoid.

In an interesting piece of historical criticism, Mr. Fletcher shows that Fresnel did not really deduce the wave-surface from his theory of the elastic ether, as is commonly supposed, but by a generalisation from the empirical results of Huggens; and he makes it clear that any theory which supposes light to travel in straight lines (rays) and to be due to a periodic change, whether a wave motion or other disturbance, at each point of the ray, must with the aid of certain natural assumptions lead to the same wave-surface.

Discarding, therefore, entirely any assumption regarding the physical constitution of the ether, he deduces the optical characters of crystals by an entirely new and extremely simple geometrical method from the Optical Indication, which is really nothing more than the ellipsoid (spheroid or sphere) familiar to physicists as the Ellipsoid of Polarisation of Cauchy. The simplicity of the method is greatly enhanced by the exclusive use of rays and not waves throughout the process.

We venture to predict that those students who are sufficiently courageous to master the first four chapters of Mr. Fletcher's treatise will not only find a lucid and logical explanation of the nature of crystalline double refraction, but will obtain an insight into the historical development of the subject which will be of vast assistance in the study of the writings of other authors.

ANNIVERSARY ADDRESSES TO THE GEOLOGICAL SOCIETY OF LONDON, 1891 and 1892. By Sir A. Geikie, F.R.S. (*Quarterly Journal of the Geol. Soc., Proceedings*, vol. xlvii., pp. 48–162, and vol. xlviii., pp. 38–179).

IN his presidential address to the Geological Society of London, in February, 1891, Sir Archibald Geikie set before himself the task of presenting "a general outline of the whole volcanic history" of the British Isles; and this outline has just been completed by the publication of his second address in May of the present year. As the author remarked, "placed on the edge of a continent and the margin of a great ocean-basin, the site of Britain has lain along that critic a border-zone where volcanic energy is most active and continuous;" and the records which he unfolds show a recurrence of volcanic phenomena in the west and north-west of the area, even after very long intervals of time. Thus, the Exeter eruptions being referred back to the Permian, the whole Mesozoic era passed without external manifestations; yet in early Eocene times the outbreaks again began on the continental border, giving us the plateaus and necks of Antrim and the Inner Hebrides.

Beginning in his first address with the Archæan masses, the author well states his position when he accepts "the term 'Archæan' as a general designation for the oldest gneisses and their associated rocks," and observes that he is unaware "of any reason why rocks undistinguishable in composition and structure from Archæan masses may not be found in much younger formations." The Lewisian gneiss is regarded as "originally a mass of various eruptive rocks," which may or may not have given rise to volcanic products at the surface. Certain pebbles of true lavas occurring in the Torridon Sandstone may actually be relics from volcanoes of Archæan age.

In discussing the "younger schists" a new term is introduced.

The schists of the central Scottish highlands, with included quartzites and limestones, and the lithologically similar rocks of Donegal, Mayo, and Galway, are named *Dalradian*, from Dalriada, a north Irish kingdom that sent out very successful colonists into Scotland. Like the American "Algonkian," this may come to be a very useful term, if confined, as its author would desire, to the localities in which it has originated. Sir A. Geikie, in cutting off this group from the Archæan, admits that it may contain rocks of various ages, from Archæan to Upper Cambrian; "sills" of igneous rock occur on a number of horizons in it, and in Co. Tyrone coarse volcanic agglomerates and lavas are recorded at its base.

Anglesey comes in for an interesting discussion; a core of Archæan gneiss is admitted "with some confidence"; and the schists are regarded as a metamorphosed clastic series, referred to the Dalradian, and including the quartzite of Holyhead Mountain. The important announcement is made that this latter rock is crowded with annelid-pipes; whether these are Precambrian or Lower Cambrian must be left an open question.

All through this portion of the address one sees a growing tendency to assign a Precambian age to the Torridon Sandstone, though this is not expressly stated; in fact, the officers of the Geological Survey only proved this point a few months later. While dealing with volcanic rocks, question after question of this kind has confronted the author; and the fear is that stratigraphical assertions or admissions of a most important character occurring in this address, may be overlooked by general workers, owing to their being interwoven with details of a more petrographic nature. Let us at once say that no one interested in the Precambrian controversy can afford to be without the address of 1891.

The candid acceptance of the views of critics of the older work of the Geological Survey is noticeable in the treatment of successive systems; but the acceptance or rejection of such criticism has been fairly based by the author on a review of the evidence in the field itself. Dr. Callaway's term "Uriconian" is thus adopted for the Wrekin volcanic series; and the great eruptions of the Cader Idris area are believed to have broken out in Tremadoc times, attaining their great development in the Arenig. The suggestion of a basal volcanic agglomerate in the quartzites and shales of Howth has already called forth a rejoinder from Professor Sollas, read recently before the Royal Dublin Society.

The second address, delivered in February, 1892, opens with an extensive account of the Old Red Sandstone volcanoes. Trachytes are recorded from near Jedburgh; and still more typical examples are described later, with analyses, from the Lower Carboniferous of the Garlton Hills, the rock of Traprain Law being regarded by Dr. Hatch as a phonolite. Interest attaches to the volcanic necks in the S.W. of Scotland which traverse the Coal-measures; others are correlated with these, and a Permian volcanic series is established. As already mentioned, the andesites in the red rocks of N.E. Devon are removed, somewhat conjecturally, from the Trias back into the Permian.

The Tertiary volcanoes are dealt with more summarily, owing to the author's important paper on this subject in the *Transactions of the Royal Society of Edinburgh* (1888). He maintains that the more acid rocks of Skye and Mull are intrusive both in the basalt plateaus and the gabbros, which have been usually regarded as the later series;

but the point is conceded that the rhyolites of Co. Antrim (still styled "trachytes") are at any rate older than "the later half of the basalts." On this, as on other matters, more may still be said; the two addresses contain so much that is new, interspersed amid summaries of older work, that criticism must be left to observers in special localities, while all readers will profit by the comprehensive view now given them. G. A. J. C.

WITHIN AN HOUR OF LONDON TOWN. By a Son of the Marshes. Edited by J. A. Owen. 8vo. Pp. 314. London and Edinburgh: William Blackwood and Sons, 1892.

"WITHIN an Hour of London Town," is not in any sense a scientific work, nor does it possess much originality. Its special charm lies in the wealth of language, rich and varied, but seldom forced or unnatural, which carries us rapidly away from the noise and rattle of City life, into the retired haunts of the "fern owl," the green woodpecker, and other interesting birds, whose habits will always repay the investigations of a quiet eye. There is just a flavour of censoriousness in the present volume, savouring a trifle too much of self-consciousness, but this is too well concealed to detract seriously from the value of its fascinating pages. Lovers of pets will revel in the description given of the habits of a Little Owl, though, remembering the numbers of this species imported into London every year, we cannot think that the little pet of "the Son" had really crossed the Channel by his own election. Chapters on "Autumn Lights and Shades," and "Winter Shifts" are strongly imbued with that healthy insight into the ways of wild creatures which is one of the growing signs of the times.

A large proportion of the book consists of the commonplace talk of country folk. This is often good of its kind, but it soon palls, and a little of it goes a very long way with most people. The author gives a long disquisition on the Hawfinch eating peas, but he has entirely forgotten to say that it consumes large quantities of injurious insects during the spring and summer. Its wholesale destruction, as described by the Son of the Marshes, is a direct defiance of the Wild Birds Act, and entirely opposed to the dictums of common sense. One female Hawfinch, shot near her nest in Kent, proved on dissection to contain—not green peas, but forty injurious caterpillars. Yet the author of this work seems almost to approve of its destruction.

We notice that the author occasionally becomes confused between allied species. For example, at p. 153 he identifies the Nearctic Buffel-headed Duck with the Goldeneye. At p. 163 he tells us that punt-gunners *do not shoot together*. Unfortunately, *many of them* shoot together, and share their luck together. The foot-note on p. 11 seems to imply that the term "gabble retchet" is applied to wild geese in Surrey. As a matter of fact, the expression only obtains (so far as we know) in Yorkshire, and the whole note is borrowed property. But we have no wish to find fault with the fare provided for us in this volume. It was a good idea to reprint the articles here gathered together from the *Times* and the monthly magazines. Such essays deserve to be encouraged, because they supply a pabulum of the daintiest and most airy kind for our play-hours.

ATLAS OF PHYSICAL GEOGRAPHY: illustrating in a series of original designs the elementary facts of Chartography, Geology, Topography, Hydrology, Meteorology, and Natural History. Edinburgh and London: W. & A. K. Johnston, 1892. Price 12s. 6d.

THIS new edition has been thoroughly revised under the supervision of Sir Archibald Geikie. It comprises 24 plates, with accompanying letterpress. Of these plates nine are entirely new. They include neat Geological Maps of the British Islands, Europe, and North America; a Map of the World, showing heights of the land and the depths of the Ocean-basins; Maps showing Isobaric Lines, and Prevailing Winds of the World for January and July, Climatological Charts of the World for January and July; and a Chart of the World, showing the forms and directions of the Ocean Currents.

THE STORY OF THE HILLS: A Popular Account of Mountains and How they were Made. By Rev. H. N. Hutchinson, B.A., F.G.S. Pp. 357. Illustrations. London: Seeley & Co., 1892.

FEW popular writers on Geology have surpassed Mr. Hutchinson in clearness of style combined with accuracy of expression, and we commend his new volume to the notice of all who desire a short general exposition of the subject of mountains. It is a work that will interest the most casual reader, and convey much solid information in a pleasant form. The first third of the book deals with mountains as they are; the second two-thirds are devoted to purely geological matters. Most of the illustrations are reproduced from fine photographs of scenery.

THE last number of the *Proceedings of the Zoological Society* (1892, pt. i., issued June 1st), contains a number of papers on systematic zoology, including papers on the Mammals, Reptiles, and Batrachians of Barbary, by Dr. Anderson; of the Myriapods and Arachnids of the same region, by Mr. Pocock. Mr. Thomas revises the Hyracoidea, and Mr. Sclater has two papers on African Mammals. Additions to the Insect fauna of Borneo and Africa are made by Messrs. Butler, Gorham, and Gahan. Worms occupy considerable attention, three of the most interesting papers in the number relating to this group, and are contributed by Mr. Beddard and Dr. Benham. The paper of most general interest is a very heterodox paper by Mr. W. Bateson, in which he maintains that the present idea of Homology is imperfect and "founded on a misconception of essential facts"; he points out that the current theory ignores variation, or assumes that any such variation respects the individuality of each member of a series of structures such as teeth: he shows by an extensive series of illustrations that variation is not confined within any such narrow limits. Mr. Lydekker describes a Sirenian jaw from the North Italian Aquitanian (Oligocene), the nature of the teeth of which afford strong evidence in favour of the order Sirenia having been evolved from some Artiodactyle Ungulate with selenodont teeth. By adoption of new and improved methods of treatment, Mr. M. F. Woodward has succeeded in settling the old question of the dentition of Hyrax; it is interesting to note that his work was mainly done on the specimens used by Professor Huxley in his well-known paper of 1863.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The removal of the Natural History collections at Prague to the new building of the Royal Bohemian Museum is now in progress. The cases, however, are not yet completed, and nearly all the specimens are thus packed in boxes. The difficulties are also much increased by the very limited staff at the disposal of the Director, Dr. Anton Fritsch. A special room 25m. in length by 12m. in breadth, to be termed the "Barrandeum," has been appropriated for the exhibition of the late Dr. J. Barrande's collection of fossils illustrating his "Systéme Silurienne de la Bohême;" and we are pleased to learn that the Barrande Fund, founded by Dr. Anton Fritsch for the further encouragement of the work that ceased at Barrande's death, has now reached the sum of 10,000 florins. The interest on this fund, which has been raised by voluntary contributions, will be available next year for the endowment of research in the Silurian Formation of Bohemia, and it is hoped that some palæontologist will be thereby induced to make important additions to our knowledge of the smaller fossil organisms of that Formation.

The Annual Report of the Vienna Museum of Natural History for 1891 appears in the new part of its *Annalen* (vol. vii., pts. 1, 2). In addition to the detailed lists of acquisitions and publications, and the general report of progress, there is an interesting brief account of the various scientific missions undertaken by members of the staff in foreign countries. These missions form one of the most noteworthy features in the work of the Vienna Museum, and during 1891 the costs of no less than eight journeys were defrayed from the fund at the disposal of the Director. Dr. Franz Steindachner's exploration of the depths of the Mediterranean in the Grecian Archipelago, under the auspices of the Vienna Academy of Sciences, is also an important feature in the year's work; and his subsequent co-operation with Dr. R. Sturany in the investigation of the fauna of some freshwater lakes in Macedonia resulted in the acquisition of a valuable collection from a previously unexplored region. Among future arrangements, Dr. A. Brezina is deputed to represent the Museum at the Scientific Congresses in connection with the Chicago Exhibition next year. Few changes were made in the staff of the Museum during 1891, and numerous congratulations were offered to the esteemed Director, Dr. Franz Ritter von Hauer, on the occasion of his seventieth birthday.

The Annual Report of the Brighton Museum for 1891, lately issued, records much progress in the arrangement of the Natural History collections, which are now very extensive. The most important addition during the year was the series of Brachiopoda, Bryozoa, and Tunicata selected and arranged by Miss Agnes Crane. In response to a request from the Secretary of the British Association Committee for the Registration of Type-Fossils, Mr. Edward Crane, Chairman of the Museum Committee, has prepared a list of the type and figured specimens of fossils, which is appended to the Report. The unique collection of fossils from the Sussex Chalk, presented to the Museum by Mr. Henry Willett, is especially rich in such specimens; and the Holmes Collection of Wealden Reptilia comprises several bones described by Sir Richard Owen.

The sixth International Geological Congress is to be held at Zurich in August, 1894. A preliminary circular, signed by Professors Renevier, Heim, and Golliez, has already been issued, together with a brief *Procès-verbal* of the meeting held on the 3rd

August, 1891, at Salzburg, which was concerned with certain European geological maps. Considering the nature of their country, the Swiss geologists are arranging two kinds of excursions, some across the Jura and the Alps, for those "accustomed to long walks," and others for the less sturdy, during which steamboats, railways, and other conveniences will be utilised. Full particulars will be furnished later. The meetings will be held at Zurich, but the excursionists will reassemble at Lugano for the final meeting of the session.

MR. BERNARD HOBSON, M.Sc., has been appointed Assistant Lecturer on Geology in the Owens College, Manchester. Mr. F. R. C. Reed succeeds the late Mr. Thomas Roberts as Assistant to the Woodwardian Professor of Geology at Cambridge, and Mr. H. Woods has been appointed Demonstrator in Palæobotany in the same university.

THE foundation-stone of an institution destined to include a Public Library, Museum, and School of Science and Art, was laid at Carlisle on May 26. The cost, amounting to £18,000 or £20,000, has been raised partly by public subscription, partly by a contribution from the funds at the disposal of the Town Council.

THE Annual Report of Mines for the Midland Counties for 1891 has recently appeared. The district inspector, Mr. A. H. Stokes, states that the amount of coal raised, 21,569,161 tons, is the largest quantity ever yielded by the Midland collieries in one year, and exceeds that of 1890 by over a million tons. Classes for instruction in mining have been formed, and appear to be appreciated in Nottinghamshire and Warwickshire.

DR. FORTNUM'S offer of the remainder of his collection of antiquities and works of art to Oxford University was accepted by Convocation on 14th June. The collection was offered, together with £10,000, on condition that the University should build a new Ashmolean Museum, close to the University galleries, at a cost of £11,000, an additional £4,000 for fittings being included in Dr. Fortnum's will. A further condition is that the University provide £300 a year, which, added to the interest on Dr. Fortnum's £10,000, will suffice to keep the whole in good order. The alterations will relieve the Bodleian Library, and improve the Taylorian Institute and the galleries, by the addition of the new museum.

THE second International Zoological Congress will open at Moscow on August 22. Arrangements are being made for excursions around Moscow, towards the Caucasus, the Urals, and Turkestan, but these depend on the number of names sent in. A reduction of 50 per cent. has been granted to the members of the Congress on all the Russian railways.

THE "Association française pour l'avancement des Sciences" will meet this year at Pau, from the 15th to the 22nd of September. The American Association meet at Rochester, N.Y.

AS a frontispiece to Professor Flower's book on "The Horse," noticed in our issue for March, a figure of the skeletons of a man and a horse are given. The skeletons themselves may now be seen in the mammalian alcove of the vestibule of the Natural History Museum, and will be found of much assistance to the student of comparative osteology. The bones in both are labelled, and around the half of each skeleton a mould of the skin is placed which enables the observer to understand the relations of the bones to the external form of the living animal.

MR. N. OKOSHI, Acting Consul-General for Japan, has been requested by his Government to pursue enquiries as to the organisation of the various museums in this country, with special reference to the artistic and scientific establishments. The

Tokio University having proved a successful and flourishing institution, the Japanese Government is turning its attention to the subject of national museums. In noticing this, the *Birmingham Post* comments on the increasing tendency to use the English language for scientific publications of international interest, and refers to the publications of the Tokio University and to certain serials of Sweden, Norway, and Holland as being in that language. The Tokio University publishes both in English and in the vernacular, and both series have been seen at the Natural History Museum. The wise plan of publishing abstracts of lengthy papers in some generally understood language, such as German or French, has for long been followed by the Russian and Hungarian scientific journals, while many papers published originally in Polish are later on printed again in German. Unless this were done, the task of reading the numerous memoirs on certain subjects would be a hopeless one, and much valuable information would be useless, except to a few, the knowledge of three to five Western European languages being a fair accomplishment for the average scientist.

THERE has lately been established "The Institution of Mining and Metallurgy," the temporary offices of which are situated at 18 Finch Lane, E C. Its object is the general advancement of Mining and Metallurgical Science, and it comprises four classes—Honorary Members, Members, Associates, and Students. With exception of the Honorary Members, all must be engaged in practical Mining or Metallurgical pursuits, or be about to enter the profession as students. The general plan of the new Institution corresponds with that of the Institute of Civil Engineers, and it is intended that it should take a corresponding position so far as concerns mining engineers. A number of the Associates of the old Royal School of Mines have joined this new body, one of them, Mr. George Seymour, M.Inst.C.E., has been chosen President, while Professor A. K. Huntington, Mr. Edward Riley (long associated with the late Dr. Percy), and others form the Council.

THE second Conversazione of the Royal Society was held on the 15th June. The exhibits included many illustrative of Natural Science. Perhaps the most interesting of these were the rats and rabbits of Professor Romanes, which showed the results of crossing various breeds. The experiments made "prove the error of those writers who assume that an act of fertilisation consists in the male and female elements intimately blending together, after the manner of a mere mechanical mixture, so that the offspring always presents characters more or less intermediate between those of its parents. In many cases this does happen, but in many other cases the admixture of hereditary elements is by no means intimate—those derived from the father and mother appearing to remain respectively grouped together," with the result that the offspring show a strong resemblance to one or other of the parents alone. Professor Seeley showed the skeleton of *Cynognathus crateronotus*, a new theriodont reptile from the Karroo Rocks of South Africa. The special nterest of the animal consists in the remarkable mammalian type of parts of its' osteology. Dr. Hicks exhibited the mammoth remains recently found in the north-west of London. Mr. Cunningham's specimens and apparatus used in the experiments on the artificial pigmentation of the lower side of flat-fishes (see NATURAL SCIENCE, p. 191), were shown by the Marine Biological Station. The remarkable non-venomous South African Snake (*Dasypeltis scabra*), whose food consists entirely of eggs, was exhibited by the Zoological Society. The egg is unharmed in the mouth of this snake; but by the muscular contraction of the gullet it is forced against and cut open by a saw-like row of gular teeth, which are formed by the inferior processes of the neck vertebræ. The hard shell is rejected by the mouth in the form of a pellet. The red pigment, Turacin, containing 7 per cent. of copper, which Professor Church has recently found in the wing feathers of the *Musophagidæ*, or plantain eaters, was shown. Insects were also represented by cases of Lepidoptera in illustration of mimicry, shown by Professor Stewart, and by Mr. Poulton's Photo-micrographs illustrative of the mode in which the scales have disappeared from the transparent wings of certain moths. Of the botanical

exhibits, the *Lodoicea seychellarum* or double cocoa-nut was sent from Kew; *Splachnidium rugosum*, Grev., the type of Misses Mitchell and Whitting's new order of algæ, was exhibited by Mr. Murray; while Professor Marshall Ward showed preparations of various botanical objects, chiefly fungi, which cause diseases and injuries in plants. Mr. Savile Kent sent some photographs of coral reefs, and the marine fauna of the Great Barrier Reef of Australia.

THE Annual Report of the Lausanne Natural History Museum for 1891 has just been published. The chief accessions recorded are a fine collection of Triassic fossils, formed by Professor Renevier at Hallstadt, the collection of European land and freshwater shells belonging to the late Gustave Maillard, a collection of algæ from Finisterre, from M. Chenevière, and some freshwater algæ from Department Manche from Mlle. Richard. The Report shows that this museum maintains its steady progress, mainly owing to its energetic keeper, Professor Renevier.

THE programme of the International Botanical Congress, to be held at Genoa next September, is as follows:—

Sunday, Sept. 4.—8 p.m. Reception of Foreign Botanists in the Municipal Hall.

Monday, Sept. 5.—9.30 a.m. Inauguration of the Congress in the Great Hall of the University. 2 p.m. First Scientific Session (Great Hall).

Tuesday, Sept. 6.—10 a.m. Inauguration of the Hanbury Institute (Botanical Garden). 2 p.m. Second Scientific Session (Great Hall).

Wednesday, Sept. 7.—9 a.m. Third Scientific Session (Great Hall). 2 p.m. Visit to the Exhibition and Town.

Thursday, Sept. 8.—8 a.m. Sea trip to Portofino, thence by carriage to S. Margherite, Rapallo, Ruta and Recco, returning by steamer.

Friday, Sept. 9.—9 a.m. Fourth Scientific Session (Great Hall). 2 p.m. Fifth ditto.

Saturday, Sept. 10.—7 a.m. Excursion to Ventimiglia and Mortola, and visit to Mr. Thomas Hanbury's garden.

Sunday, Sept. 11.—Excursion from Ventimiglia to the Col di Tenda.

The duration of the Congress may be prolonged if deemed advisable. Anybody "occupied in botanical study or cultivating any special branch of botany" will be entitled to take part. To be inscribed as a member, a printed form distributed for the purpose must be signed and forwarded to the Organising Secretary, Professor O. Penzig, R. Université, Genoa, or a book provided for the purpose may be signed during the Congress. Every member, except those who are members of the Italian Botanical Society, will pay a fee of ten francs on receiving his admission card. This card admits to all the sittings of the Congress, and to the museums, libraries, and collections, which will then be open to the members, and also gives the right to take part in the excursions and entertainments proposed. All the sittings will be held in public, but the right to speak and take part in the discussions is reserved for members. The official language will be Italian, but everyone will be free, when speaking or in discussions, to use the language with which he may be most familiar. No particular subdivisions of the various branches of botany will be instituted, with separate meetings for each, unless the number of members present and the abundance of subjects to be discussed should render this necessary. No special subjects have been fixed for discussion, but "the Reform of Botanical Nomenclature will be treated in accordance with O. Kuntze's recent book"; lectures are also announced on F. Delpino's theories on Phyllotaxis and Pseudanthis. Notices of subjects for discussion must be sent to the Secretary not later than August 15. After the Congress the Committee will print a brief account of the meetings, together with the original memoirs presented; each member of the Congress will be entitled gratis to a copy of these "Proceedings," while authors of the memoirs will receive gratis fifty separate copies. The Congress will take place in the Palace of the Royal University (Via Balbi). Arrangements have been made with the Italian and foreign railway companies and the chief Steam Navigation Companies, so as to obtain the usual reduction of fares for members of the Congress. An office where information concerning lodgings can be obtained will be opened at the Municipality of Genoa.

OBITUARY.

JOSEPH MARTIN.

THE death of this enthusiastic explorer was announced on May 26, at Marghilan in Russo-Turkestan. Mr. Martin, who succumbed to an illness brought on by privation and fatigue, has added much of late years to our knowledge of Central Asia, having traversed Thibet and the country bordering the southern frontier of China. He was a somewhat wealthy man, and devoted the major part of his money to exploration.

A REPORT of the death of Emin Pasha has been circulated, but so far, there is nothing to show that this is more than one of the rumours which all African travellers have to put up with. It has received the inevitable contradiction, which is probably as reliable as the original statement.

OBSERVATIONS AND CORRESPONDENCE.

PRE-GLACIAL MAN IN BRITAIN.

In reference to our observations on this subject in the April number (*supra*, p. 85), Mr. W. J. Lewis Abbot writes a long letter, from which we extract the essential points:—

"What can the writer mean when he says 'the discussion of man's antiquity in Britain has, by common consent, been allowed to slumber for some years'? Can he quote any seven years in which a greater store of information has been amassing in favour of man's antiquity than the last? The next sentence is ambiguous, namely, that the evidence 'was scarcely such as to command the respect of geologists,' and joined to this is the assertion that 'it seemed also probable that before long facts would be discovered that would definitely settle the question.' What are these facts? and upon what is their probable discovery based? The writer goes on to say that 'many weak links exist in the chain of argument by which he [Professor Prestwich] attempts to prove that man existed in this country prior to the denudation of the Thames Valley and the Weald.' Why are not some of these 'weak links' enumerated? One would say that no geologist or anthropologist would question the priority of the plateau men. The writer objects that 'many of the supposed flakes yield but doubtful evidence of human agency, and none of them have yet been found more than 2½ feet below the surface.' With regard to the first statement, the flakes, although apparently so asymmetrical, show a persistency of type, an identity of curvature, not to be excelled by river-drift specimens; while as for the depth at which they are found the plateau drift is rarely penetrated deeper, under conditions where one could find implements."

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NOTES AND COMMENTS.

The Imperial Institute.

THE recent announcement of the issue of a detailed prospectus of the aims and methods of the Imperial Institute, was welcomed by many who hoped that this would at last lift the veil from the great South Kensington "Hall of Mysteries." We fear, however, that most of those who had hoped to gain some clear ideas upon this subject are doomed to disappointment. The prospectus is a pamphlet of 64 pages, but by far the greater part of this is devoted to lists of the illustrious ones who serve as governors and on committees, to preliminary correspondence which we read years ago, to glowing descriptions of the scenes of "spectacular magnificence" at the "gorgeous pageant" when the foundation stone was laid, and to elaborate details regarding the architecture of the building. Only a modest 7 pages are devoted to the section on the "objects and purposes," and the statements here are written in a style that combines the foggy precision of a limited liability company memorandum with the profusion of promises of an election address. Among other benevolent functions, the Institute intends to undertake "the collection and dissemination of such information relating to trades and industries, to emigration, and to the other purposes of the charter as may be of use to the subjects of the Empire." The point in the programme on which stress is first laid is that of the formation of a collection of the raw products; by this, in association with libraries and conferences, it is hoped that the Institute will become "a central source of information upon all matters relating to the natural and industrial resources, the trades and handicrafts, and the commerce of every part of the Empire." Branch museums and

sample rooms are to be widely scattered; systematic colonisation is to be furthered; commercial education encouraged, and industrial exhibitions and conferences arranged. As if this were not enough, the Institute promises to do "anything incidental or conducive to carrying into effect all or any of the foregoing purposes," and all this on an endowment of £4,000 a year. The only item in the programme that does appear practical is that which proposes to use the Institute as a South Kensington club, with a "special refreshment room for Fellows," combined with all ordinary club privileges.

To all this there is simply one fundamental objection. It is the right thing in the wrong place. The original scheme was well worthy of the occasion it was intended to commemorate. Our colonial empire has been founded by commerce and for commerce, and upon this, more than on race or language, it depends for its continued union. There could, therefore, have been no happier method of commemorating the era during which our colonies have made their greatest progress, than by the establishment of an Institute whose duty it should be to prevent industrial effort running to waste, and to substitute for the hostility of commercial competition the belief that "the good of each is the common good of all." For such an Institute, however, properly to fulfil the purposes for which it was designed, it was absolutely essential that it should be placed right at the commercial centre of the empire; that, of course, fixes its proper place as in the City, and, if possible, within the few acres wherein are crowded the Bank, the Clearing House, the Royal Exchange, the Stock Exchange, the Mansion House, and the Baltic. South Kensington, however, may possibly be made the centre of English culture, science, and art, but the commercial centre it can never be until Macaulay's Maoris there hold auction with their long-lost and lamented green-jade gods.

The scheme, as at first proposed, was received with enthusiasm by men who recognised its full business value, and who were prepared to back it with the wealth and organising genius of the City; and this would assuredly have been a more stable foundation than the fickle enthusiasm of popular sentiment. The moment the site was changed to South Kensington, however, the whole basis was altered. The City men at once realised that a museum of commercial samples, located seven minutes' walk from a station nineteen minutes by train from the nearest point of the City, would be about as useful to them as a ready reckoner kept at Brighton. A heavy percentage was therefore deducted from their subscriptions to found a smaller but more accessible institution, and this not only reduced the income, but introduced the disorganisation inevitable to divided interests.

The selection of South Kensington was no doubt made from the impossibility of securing a sufficiently imposing site further east; but we think, under the circumstances, it would have been better to

have erected a compact and commodious hall, such as those with which the not so very impecunious City Companies content themselves, rather than a spread out and showy palace. As it is, the building has taken nearly all the funds at the disposal of the Institute, for though an endowment yielding £4,000 a year has been retained, a further £80,000 is required for the building fund. As the chances of collecting this within a short time do not seem very hopeful, the Institute only adds another to the Kensington group of unfinished edifices. The financial condition of the Institute is not the least unsatisfactory feature in its outlook; the endowment of £4,000 a year is wholly inadequate, and the annual subscriptions of the Fellows and Colonies are not likely to give it a firm basis for its multifarious projects. The maintenance of the museum alone ought to absorb more than its income. In fact, unless Parliament be willing to vote a grant in aid, the Institute seems likely to be of most use as a Kensington club, while, as the home of fashionable soirées, it may remind a larger audience of the fate that befalls even Imperial institutions when they choose to ignore the limitations of time and place.

Right of Way across the Norfolk Broads.

The recent decision in Chancery as to the rights of fishing and shooting on Hickling Broad was awaited with considerable anxiety by many who feared that the interests of the public were being seriously jeopardised by the reckless zeal of its champions. Mr. Justice Romer's decision, however, is more satisfactory than it might have been, for not only is the right of way across the Broad to Hickling Staithe fully maintained, but there is full liberty to leave the channel. The shooting and fishing on the Broad, however, are decided to be private, which, after all, is a mere detail now that full access to it is admitted. The defendants' claim that the Broad is tidal is simply preposterous; anyone might as well maintain that the reservoirs of the Battersea waterworks are tidal and accordingly claim the right to unlimited picnic on them. If the Court had upheld the view that Hickling Broad is tidal, that term would simply have had to be redefined. It is to be hoped that visitors and riparian owners will agree better now that their respective rights have once again been clearly defined. So long as the public have undisputed right of way across most of the Broads and as much free shooting and fishing as there is in the district, it is to be hoped that they will cease to infringe the rights of the riparian owners. So far the latter have, on the whole, acted with great consideration, and if the private Broads are ever closed the fault will rest with those members of the public who have abused privileges which otherwise would not have been questioned. The Broad landlords have never exhibited the spirit shown by some of the suburban landlords, to one type of which attention is this month called in our correspondence columns.

Newspaper Science.

In our first number we had occasion to protest against the feebleness of most of the stuff that usually passes in the daily journals for "Science." We are therefore glad to be able to congratulate the *Daily Chronicle* on not only maintaining a weekly column of "Science Notes," but on not leaving the office boy to write them. In the notes, however, of June 30, there is a paragraph on the origin of the Great Lakes of North America, in which Dr. Spencer's observations on the directions of the ice scratches are said to put the glacial theory out of the field. But this is hardly fair to Professor Newberry who knew all these facts, and whose hypothesis was based to a large extent upon them. He maintained that the lake basins were old river channels broadened out by the ice crossing them and constantly wearing back the southern bank against which it was piled up.

The Laws of Hybridism.

Professor Romanes explained, at a recent meeting of the Zoological Society, that speculations on heredity were in inverse ratio to the experiments. A comparison of the following extracts from the last number of Natural Science shows that, however few those experiments may be, the knowledge of them appears to be in the inverse ratio squared :—

Professor Romanes.	Professor Giard.
Natural Science, *July*, p. 398.	Natural Science, *July*, p. 358.
The experiments made "prove the error of those writers who assume that an act of fertilisation consists in the male and female elements intimately blending together, after the manner of a mere mechanical mixture, so that the offspring always presents characters more or less intermediate between those of its parents."	"*As all biologists know*, the progeny obtained by the union of two races cannot be intermediate between the two progenitors, but resemble closely one or the other. For instance, in a union between a female white mouse and a wild gray male, one constantly obtains young, and some of whom are completely gray others completely white."

Texan Geology.

In a paper on "The Cretaceous Covering of the Texas Palæozoic" (*American Geologist*, March, 1892), Mr. Ralph S. Tarr discusses the question whether the central Palæozoic and pre-Palæozoic region of Texas was completely covered by Cretaceous Rocks. His studies lead him to maintain that this was the case, and his conclusions thus agree with views expressed by Professor R. T. Hill.

From early Carboniferous times until the Cretaceous period, the ancient Palæozoic core of Texas has been subject to denudation. This is shown by the character of the Carboniferous conglomerates and

the subsequent uncovering by erosion of ancient bays and promontories that marked the margins of Carboniferous seas. Later on, extensive Permian rocks were laid down in inland seas.

The features marked out in the Carboniferous area prior to the Cretaceous covering are nowhere very striking; the hills recently uncovered are broad, flat-topped, and gently undulating. Thus, when the Cretaceous period began in Texas, it found a low-lying land of Carboniferous and Permian strata, undulating and nearly at its base-level, and an older Palæozoic highland district considerably diversified, but not distinctly mountainous. Upon this area the encroaching sea commenced the deposition of sands and conglomerates; then, with more complete submergence, followed higher beds of deeper water origin. This basal Cretaceous deposit is not an ordinary beach conglomerate, for it contains many limestone pebbles, and the matrix, fully one-half its bulk, is limy matter. The beds appear to have been formed from the hurriedly worked over soils and land *débris*, in a rapidly encroaching sea. Over the Permian strata they are red, agreeing with the colour of the underlying rocks.

The old Palæozoic land at present seldom reaches an elevation above 2,000 feet, and at 1,900 feet traces of the earlier Cretaceous beds have been found. The higher Cretaceous beds indicate deep-water conditions; they include thin beds that have been traced over hundreds of square miles, and a great thickness of true chalk. Their aggregate thickness is several thousand feet. These facts, taken in conjunction with the absence of littoral characters in the higher Cretaceous beds, and with the present dip of the strata in relation to the older rocks, indicate that all the higher ground must have been covered. The central highland region of Texas is at present much more elevated than the uppermost Cretaceous beds to the south-east; it was probably the first area to be upraised, and to suffer denudation, so that the streams now have adjusted themselves to the channels of the ancient watercourses.

With regard to "The Texan Permian and its Mesozoic Types of Fossils," we may state that a short memoir with the title just given has been published by Mr. Charles A. White (*Bulletin U.S. Geological Survey*, No. 77, 1891). Thirty-two species of Invertebrata are recorded, and some of these are figured in four plates. The species show a commingling of Carboniferous and Mesozoic forms.

"A Severe Critic."

In a pamphlet, printed for the author, and entitled "The Geological Map of the United States, and the United States Geological Survey" (Cambridge, U.S.A., 1892), Mr. Jules Marcou has severely criticised the conduct of the American Geological Survey. He thinks that "Ten or twelve years at most is a sufficient lapse of time to execute all the researches in the field necessary to fill up the Geo-

logical map with exact data. Then three other years to prepare the map, sections, reports, and descriptions of fossils; and in fifteen or sixteen years, at most, we should have a good geological map of the United States," &c. He estimates that the work could be done by thirty-three officers, and at a cost of about £800,000.

If only a sketch-map were wanted, the task could possibly be achieved in the manner suggested by M. Marcou, for he contemplates "a map in ten or twelve large sheets for the whole United States."

When, however, we read that "The first want and principal object is to construct a geological map . . . to be used by Government in all general questions which may arise for Industry, Commerce, Agriculture, Forestry, Mining, Railroad," &c., and that the map "should be correct in regard to chronology, determination of rocks represented in each geological basin, and also in regard to exact limits of formations," we fear that the critic has had little occasion to use geological maps for practical purposes. Even the one-inch scale of the British Geological Survey is too small for many practical purposes, and M. Marcou would have one about one-thirtieth of that scale! In fact, he is bound to admit that "it is impossible on a general map to mark distinctly the superficial deposits," and although admitting "the importance of the Mississippi river alluvial, and of the coast formations," he would leave surface geology for the most part to volunteers and free geologists, not in the pay of the United States Government. One would have thought that in matters of Agriculture, Forestry, Railroad, &c., as mentioned above, not to forget questions of Water-supply, the mapping of the Drift formations would have been as important as that of any other formations.

Again, M. Marcou complains of the "Extravagance in the Management of Publications." In this country the United States has usually been regarded as furnishing a noble example—for surely parsimony is detrimental to the growth of Science.

British Cretaceous Foraminifera.

For many years it was a matter of reproach to English palæontologists, that, although numerous monographs on Continental Cretaceous Foraminifera had appeared, no papers of any importance had been published on the Rhizopoda of our English Chalk or Gault. That reproach has now been partly swept away by the detailed report on the "Foraminifera of the Red Chalk," by Messrs. Burrows, Sherborn, and Bailey, which appeared in 1890, and by the commencement of a monograph on the "Foraminifera of the Gault," by Mr. Frederick Chapman. Both these papers have been published by the Royal Microscopical Society in their Journal, and, as they have been fully illustrated, will form most valuable reference papers for a long time to come. Of the "Foraminifera of the Gault,"

Part I. deals with the *Miliolininæ* and the *Peneroplidinæ*, and was issued in August, 1891; the second part, describing the Arenaceous forms, was published in June; further continuations are promised. Mr. Chapman has treated the subject in a most exhaustive manner; he has confined his attention mainly to the Folkestone sections, and has worked out large samples of the clay from every zone in the Gault at that place. In his papers, therefore, we get not merely a list of the species found in the Gault, compiled from lumps of clay collected at random, but a definite and exact report on the forms found at every horizon, with their distribution above or below. Thus, in dealing with *Miliolina venusta* (Karr.), we read: "This Foraminifera makes its appearance in the Gault in zone x., where it is very common; also in zone xi., 55 ft. from the top, very common; 50 ft., very common; 45 ft., frequent; 40 ft., frequent; 35 ft., frequent; 30 ft., frequent; 25 ft., very common; 20 ft., common; 12 ft., common; 6 ft., very rare." It is interesting to note the comparative rarity of some genera; thus, out of the many thousand forms examined, Mr. Chapman has only found one specimen of *Biloculina*, and one of *Spiroloculina*, and only one of the deep water form *Hormosina*. Others occur in only one of the eleven recognised zones, *e.g.*, *Haplostiche*, in zone xi., at one horizon only (20 ft. from the top). It is likely, therefore, when we possess the whole of Mr. Chapman's paper, we may be able to form a much better opinion of the formation of the Gault than we now can, and all students of the Foraminifera will await the completion of this careful monograph with interest. The Foraminifera of the Lias are also receiving attention at the hands of Messrs. W. D. Crick and C. Davies Sherborn, in the Journal of the Northamptonshire Natural History Society.

The Identification of Trees in Winter.

The *Botanical Gazette* for June contains an article by Aug. F. Foerste on the identification of trees in winter, when the flowers and leaves, on which a diagnosis usually depends, are absent. It is important to use those parts "which are certain to be present both in the young growth and in the fully matured plant, and which, during the various stages of development, . . . show essentially the same characteristics." The only part which approximately fulfils these conditions "is the crop of little twigs added each year to the tips or the sides of the branches, with the leaf-scars from the last season's leaves, and the subtended winter buds." If the length of the twig, which varies considerably, be left out of consideration, and only the larger leaf-scars and scaly buds be made use of, the result is a very striking constancy in the colour and markings of the bark, the character of the pith, the form and structure of the leaf-scars, the figures presented by the foliar bundles, and the form and structure of the scaly buds. This constancy of characteristics in individual

species is accompanied by sufficient variation among the species of a genus to allow of the determination not only of the genus but also, in the great majority of cases, of the species.

The author proceeds to discuss, in the order of their importance, the various characteristics presented by ligneous plants.

I. The determination of the phyllotaxy, as indicated by the leaf-scars. "It so happens that a classification of shrubs and trees into those with alternate, spiral, and opposite or whorled leaves gives three fairly equal sets, so that this determination at once excludes a large number." In the case of rarer phyllotaxies, *e.g.*, one-third (*Spiræa opulifolia*) or two-thirds (*Catalpa speciosa*), the identification becomes still easier.

II. The form of the leaf-scar and the arrangement of the vascular bundles seen as dots on the exposed surface. The arrangement may be circular (*Morus rubra*, *Rhus aromatica*, &c.), semicircular (species of *Fraxinus*), or horseshoe-shaped (*Ptelea trifoliata*, and also species of *Fraxinus*), varying usually with the shape of the scar. Often, however, the bundles form distinct sets, as in *Ailanthus glandulosa*, or species of *Juglans*, with a more or less heart-shaped petiole, where we find lunate sets. In other heart-shaped scars, and in most lunate ones, the bundles form small circular areas, and the number of areas in the fully-developed scar is fairly constant. Thus *Ulmus fulva*, *Spiræa opulifolia*, *Hamamelis virginica*, and others have three sets, while *Rhus toxicodendron*, and species of *Æsculus*, &c., have five.

III. The character of the winter buds. These may be concealed in the substance of the scar, or so covered by its anterior end that their development towards spring causes more or less splitting of the scar, as in *Robinia pseudacacia*. Sometimes they are sunk in the bark of the twigs, as in *Ptelea trifoliata*. Prominently-developed scaly buds can be classified into those showing exteriorly only one or two scales, or a glimpse of a third or fourth (*Liriodendron tulipifera*, *Rhus glabra*, *Tilia americana*, &c.), and those with four or more scales exposed. The latter class can be further subdivided into those with terminal buds much larger than the lateral (species of *Fraxinus*, *Juglans*, &c.), and those where such a difference is not particularly marked.

IV. The mode of termination of the branches. In the great majority of cases terminal scaly buds are always present, but sometimes the green tips of the new twigs are cast off each spring, and the place of the terminal bud is accordingly marked by a scar in the winter twigs, as in *Tilia americana* and *Ailanthus glandulosa*. In other species the tip of the branch shrivels up early, but is not cast off, the shrivelled tip remaining through the winter (*Diospyros virginiana*, and species of lilac).

V. The presence or absence of stipules, as indicated by their scars. As the stipules, when present, usually fall early in spring, a little practice is required to easily recognise their indistinct scars, which may more or less encircle the stem, as in the Tulip-tree, or as

is usual be considerably separated. Sometimes, as in *Robinia pseudacacia*, the stipules are represented by thorns.

VI. The presence of thorns in general. These often represent small axillary branches, usually supplemented by normal leaf-buds at their base, when the relative position and character of the thorns and buds may be of value for distinguishing species.

VII. The presence or absence of ridges on the bark. These are usually related to the leaf-scars, being frequently decurrent from them. The more or less rounded angles are also worth observing, as in the frequently eight- to ten-angled stems of *Sambucus canadensis*.

Besides these more important characteristics furnished by the annual growth of twigs, which "may be used in forming a sort of artificial classification," there are others which are very useful in distinguishing individual species.

The colour of the bark of twigs being usually grey or brownish, the constant presence of twigs with green bark, as in *Sassafras officinale*, or shades of red or purple, becomes a valuable feature. The character of the pith is at times useful.

Other characteristics are very good, if present. For instance, the form and character of next year's flowering buds, whether present as naked catkins, or enclosed in more or less scaly buds. The remains of last year's inflorescence may also help. The form of the bark may be of service; smooth, as in the beech, or peeling in sheets, as in the birch, or, especially in older trees, showing characteristic cracks. Finally, the habit of the shrub or tree is often very characteristic, although the general aspect, resulting from the mode of branching, may be quite different in a young and an old tree, as in the elm.

Although it must not be expected that these winter twigs, with their scars and buds, will furnish better means of distinction than the ordinary botanical ones, it is astonishing how successful they may prove. The author suggests that a ready means of distinguishing the ligneous plants would be a welcome addition to the curriculum of subjects dealt with in the botanical institutions existing in many States for the diffusion of knowledge of practical utility.

Botanical Nomenclature.

As the subject of nomenclature is now disturbing the minds of botanists, the following letter, reprinted from the *Botanical Gazette* of June, 1892, may be of interest. The last words of the well-known American systematist will seem the more opportune, as it is in the neo-American School of Botany that the disturbing influence has largely arisen. The letter was dictated by Sereno Watson in his last illness, and is as follows:

"For some time I have had a desire to give expression to my views upon botanical nomenclature. Under the circumstances I must speak briefly and somewhat dogmatically. In my opinion

botany is the science of plants and not the science of names. Nomenclature is only one of those tools which is necessary to botany, and this being the case, points of nomenclature should be subordinated to science.

"A principle of botanical convenience has been established by those who prefer one name to another on account of expediency or convenience. This principle should have a great deal of influence. It has been so recognised by the greatest botanists, and from their authority receives great weight. I prefer the word *expediency* as a better term than convenience to designate the principle, that the demands of science over-ride any merely technical claims of priority, &c.

"Priority of specific names appears to be based entirely upon one section of the code of 1867. That simply says that when a species is transferred from one genus to another, the specific name is maintained. This principle is usually understood and applied in the way that the oldest specific name has a right in all cases to be retained. It cannot fairly be so interpreted and applied, since it governs only to the extent that this should be the law, but it is not to be made an *ex post facto* law. Thus, when a transfer has been made, that ends the matter so far as the choice of a specific name is concerned, and no one is authorised to take up a different name. This practice of retaining the oldest name *under the genus*, no matter what older specific names there may be, was adopted by Dr. Gray in his later years, and by the Kew botanists, for the reason that, once established and pretty generally recognised, it would avoid the great mass of synonymy which is being heaped like an incubus upon the science. I must express surprise that Dr. Britton has not considered it his duty to publish the last written words of Dr. Gray, which were addressed to him upon this subject, and which expressed his positive opinions upon this point.

"There is nothing whatever of an ethical character inherent in a name through any priority of publication or position, which should render it morally obligatory upon anyone to accept one name rather than another; otherwise it would be applicable or true as well in the case of ordinal names, morphological names, teratological, and every other form of name to which now no one feels himself bound to apply the law of priority. The application of this law as at present practised by many botanists, which would make it the one great law of botanical nomenclature before which every other must yield, regardless even of common sense, is a mere form of fetichism exemplified in science. Many instances of the application of this law are not science, but are rather superstition.—*February* 22, 1892."

The letter is short, but to the point. Important as the subject of nomenclature undoubtedly is, there is great danger of over-estimating the importance, and forgetting that Botany is, after all, the science of plants, and not of names; and the incubus of synonymy which is being heaped on the science by the perpetual tinkering of

pseudo-scientists who might be better employed, will prove a serious obstacle to systematic work, and increase the tendency to take up other branches which are not thus hampered, to the neglect of what is by no means the least important aspect of Botany.

It would be extremely interesting to know what were Dr. Gray's "positive opinions on this point," and why Dr. Britton has not found it in his heart to publish them.

Observations on the Scent of Flowers.

In the "Acta Horti Petropolitani," vol. xi., p. 383, Robert Regel gives an account of some observations made at St. Petersburg on the relation of the smell of flowers to external conditions. The relation is an evident one, but is not the same in different plants.

Plants whose flowers smell during the day have a much more intense odour in warm than in cold dull weather, thus the Mignonette and Sweet Pea have a much stronger smell on a hot clear day, and flowers of *Platanthera bifolia* grown in open places are more scented than when sheltered in woods. Flowers of *Reseda odorata* and *Philadelphus coronarius* which have already opened and contain ethereal oil smell strongly in the dark, and do not lose their smell in a dark room until they wither. To ascertain whether the ethereal oil can form in the dark, some pots of *Reseda odorata* and *Matthiola incana*, with quite odourless buds, were put in a dark room, while others in a light room had the inflorescence only cased in boxes covered inside with black paper and thus kept quite dark. The flowers of the second lot opened and smelt perceptibly, though less than when developed normally in a light room, but in the first case only those flowers smelt whose buds were well advanced when put in the dark, the rest had no smell, and no ethereal oil could be detected in sections of their leaves. Hence it is evident that in these two plants ethereal oil can be produced in the dark at the expense of some nutrient substance which, however, can itself only be developed in the light.

The flowers of *Nicotiana longiflora* smell much more at night than by day; they are open at night and in the morning and evening twilight, but closed by day. After a hot sunny summer's day the smell is more intense than after a dull cool day. When kept in darkness they no longer close by day, and smell both day and night. In specimens of which only the inflorescence was enclosed in dark boxes, all the flowers when opened gave a fairly strong smell; even when plants were placed entirely in a dark chamber, it was only in flowers that did not open till after three or four weeks that no smell could be recognised.

Nycterinia capensis behaved somewhat differently. Here also the flowers are closed and have no smell by day, opening only in the twilight and at night, when they are sweetly scented. The smell is

probably due to the presence of Benzolaldehyde (oil of bitter almonds) in the floral leaves, judging not only by the smell but also by chemical tests. After a warm sunny day the smell is stronger than after a dull cooler day. When placed in a dark chamber the plant behaves on the first day just as in a light room, smelling only by night, the flowers being closed by day and open at night. But if, in addition, the temperature be lowered, the result is different; thus, specimens placed during the day in the dark near an ice cellar, at one time at 8° R. and another at 11° R. (instead of the usual day temperature of 14°—20° R.) began to smell perceptibly after an hour, although the flowers were only half open, or even in some cases quite closed. To ascertain the influence of light on the formation of the odoriferous substance, the following experiment was made. Specimens with still odourless buds were placed in the dark chamber—in some cases plants in pots, in others cut-off twigs in water-glasses. Other plants were placed in a light room and the parts of the inflorescence with odourless buds covered with dark boxes. The first flowers opening in these dark boxes smelt evidently, though fainter than normally, and it was only after two or three weeks, when the etiolated inflorescence had grown considerably in length, that no smell could be detected in the opening flowers; those opening earlier continued to smell till they withered.

In the case of plants kept wholly in the dark, the flowers which opened on the first day were scented, but not those opening later, and the former also gradually lost their scent. The single cut-off flowers, or inflorescences, in the water-glasses lost their smell in the dark chamber after three to four days. In every case the complete disappearance of the smell coincided with the complete consumption of the starch in the cells of the floral leaves, only in the stomatal cells could starch-grains be found, and it was possible to foretell with certainty which flowers would open and which would not, by investigating the starch-content of buds in a sufficiently advanced stage of development; the strength of the smell also depended to a certain extent on the quantity of starch in the floral leaves. As a further illustration of this relation, cut-off shoots of the same Nycterinia were placed in the dark chamber—some in distilled water, some in half per cent. sugar solution. After about four days the former had used up their starch and ceased to smell, but the latter did not lose their starch, nor did the flowers cease to smell till completely withered. Even flowers which had already lost their smell in the dark regained it in sugar solution with the formation of starch in their leaves, while shoots taken from the solution and placed in distilled water in the dark lost their smell as soon as the starch disappeared. As the presence of starch is an index of the activity of constructive processes in the plant, the simultaneous disappearance of the smell of the flowers and the starch from the floral leaves of *Nycterinia capensis* shows that the formation of the odoriferous substance stands in the closest connection with the life of the plant.

In this point *Nycterinia capensis* differs from all other scented flowers investigated. In the floral leaves of *Matthiola incana*, *Rosa centifolia*, *Lathyrus odoratus*, *Dianthus plumarius*, *Reseda odorata* and others, either no deposit of starch was found, or only in the stomatal cells, while in the flowers of *Philadelphus coronarius* it occurred only in the stamens. The corolla of *Nicotiana longiflora* is rich in starch-grains, but neither here nor in *Philadelphus* has their consumption the slighest influence on the smell of the flowers; in the latter plant flowers which already smell never entirely lose their scent when placed in the dark.

Experiments were also made on the relation between respiration and the scent in *Nycterinia capensis*. Shoots with the normal night-smelling flowers, when placed in an atmosphere of hydrogen, neither opened nor smelt; the effect was somewhat persistent, as the flowers when removed from the hydrogen did not open for some time, and even then did not smell. The next evening, however, they opened and smelt in the normal way.

Finally, the author points out that although normally only the open flowers of *Nycterinia* are odorous, yet the scent and the opening and closing are not absolutely dependent on each other, for the flowers do not usually begin to smell directly on opening, and we have also seen that under some circumstances, *e.g.*, in the dark, at a temperature lower than the normal, only half-opened or even quite closed flowers may be scented.

Women Fellows.

The Royal Geographical Society has decided to admit women as Fellows. For many years the Society has accepted papers from women, and on two occasions has singled them out for special honour, as when in 1860 Lady Franklin received the Gold Medal in commemoration of her husband's discoveries, and when in 1869—a case more to the point—Mary Somerville received the Patron's Medal for her publications furthering Geographical Science. The *Manchester Guardian* for July 7th has the following excellent comment on the decision:—"Papers by lady travellers have frequently been read before the Society, and it has for some time been felt to be an anomaly that travellers of the standing, say, of Mrs. Bishop (Miss Isabella Bird) should be excluded from the ranks of a Society for which they have done so much good."

While endorsing all this, it must be admitted that there is the strongest objection to the admission of women to scientific meetings as casual visitors; it would probably be advisable to allow only the lady Fellows to introduce ladies, as meetings of a Society held for scientific business are very different from Royal Institution lectures.

We do not remember that any Society has refused to accept a

paper from a woman because she has the misfortune to be a woman, but while accepting her work, most Societies refuse to allow her to be present to hear it read. The Zoological Society's principle and practice on the subject are inconsistent: the former admits women as Fellows, "with the same privileges, and under the same regulations in all respects as gentlemen," but while the rules admit women the doorkeeper excludes them from the meetings! The Geologists' Association still maintains its lead in this matter, for it has this year elected a woman, whose work ranks high, a Vice-President. The Geological Society, however, still refuses women the Fellowship, though it is reported by *Research* (vol. i., 1889, p. 216) that when the subject was last discussed, the reform was only lost by four votes; and, but for the over-confident zeal of the women's advocates, who refused any compromise on the lines suggested above, it would, doubtless, have been carried.

The Geology of the Bas Boulonnais has received very full description from M. E. Rigaux (*Mem. de la Société Academique de Boulogne*, vol. xiv., 1892). The strata exposed at the surface include Devonian, Carboniferous Limestone, and Coal-measures. Unconformably resting on these are various Jurassic strata, the Great Oolite and Cornbrash, Oxfordian, Corallian, Kimeridgian and Portlandian strata, and then the Purbeck-Wealden. Resting indifferently on Secondary and Palæozoic rocks are representatives of Lower Greensand and Gault (Aptian and Albian). The author describes the strata in detail, and records the fossils from each horizon, so that interesting comparisons can be made with the strata in this and other countries. Some new species of Brachiopoda and Mollusca are described.

We hear that it is possible that a botanist and geologist may accompany the Eclipse Expedition up the Salum River next April. The evidence for the Cretaceous age of the rocks at Cape Verde does not seem very satisfactory, while nothing is known of the place in the Palæozoic sequence of the older rocks that are supposed to cover a wide extent of country in the Gambia "Hinterland." A very important contribution to the geology of Eastern Africa has just been issued in the last volume of the *Denkschrift der Wiener Akademie der Wissenschaft* of which we hope to say more next month.

The last number (594) of the *Abstracts of Proceedings of the Geological Society*, contains the result of Professor Seeley's labours on the Saurischia of Europe and Africa. These bird-like reptiles are described as "terrestrial unguiculate ornithomorpha," and the paper discusses the various genera, and their relations and peculiarities. In the discussion which followed, the President, Mr. W. H. Hudleston, remarked "that the paper on Saurischia was received by the officers

of the Society early in April. Since that date Professor Marsh, in his Notes on Triassic Dinosauria (*Amer. Journ. Sci.*, June, 1892), had published, as regards *Zanclodon*, conclusions similar to those at which the author (Professor Seeley) had already arrived."

In the *Scottish Geographical Magazine* for July, Dr. H. Schlichter concludes his paper on the Pigmy Tribes of Africa, and gives a map showing their distribution. The map shows that with the exception of a few scattered races between 15° and 25° S. latitude, the whole of the tribes are confined to the district between 5° N. and 5° S. lat., *i.e.*, to the Gaboon Coast, the forest area of the Congo State, the west side of the Albert Nyanza and the north of Lake Rudolf. The paper is an excellent epitome of our present knowledge of these peoples, and is abundantly supplied with references to other authors. Professor James Geikie's paper, read to the Society on May 18th, on Recent Researches in Pleistocene Climate and Geography, is printed in the same magazine; it is accompanied by a map of Northern Europe, showing the areas covered by ice during the epoch of maximum glaciation, and by the great Baltic glacier and the local ice-sheets of Britain at a later date.

The question of the action of light on the colouring of vertebrate animals is dealt with by Sr. A. L. Herresa in a paper—"El Elima del Valle de México y la biologia de los vertebrados," in *La Naturaleza* (t. ii., c. 2), issued last month. The paper, which partly appeared in the previous number, deals with the production of colour by light reflected by coloured bodies, the modification or production of colours by white light or by reflected light, and terminates with a long series of conclusions and a short paper on the general influence of light. In the same number of *La Naturaleza* there is a paper, "Apuntes para la Geologia del Valle de México," by G. B. y Puga, dealing with the eruptive mass of El Peñón de los Baños, some four kilometres east of the City of Mexico. The hill consists of two roughly semicircular masses, one of volcanic agglomerate, and the other of Retinitic-Andesite, with an included mass of black basalt and an outer fringe of a similar rock. Analyses of the rocks are given, and several illustrations from photographs assist the reader. Some notes are printed by A. Duges on *Dendroica dugesi*, H. K. Coale, the passerine bird described as new in 1887, which show that it should rather be regarded as referable to *D. æstiva* (Gm.). Jose N. Rovirosa contributes a "Calendario botanico de San Juan Bautista y sus Alrededores, año de 1890," which, assisted by a chart, gives the times of flowering and other interesting information, including the local names so often of assistance (or hindrance) in determining plants. A variety, *floribunda*, of *Breweria mexicana*, Hemsley, is described and figured by M. M. Villada.

Mr. Halkyard continues his researches into the Foraminifera of the Channel Islands in the *Transactions of the Manchester Microscopical Society* for 1891. In the present paper, the distribution of the various forms around Guernsey, Herm, and Jersey is attended to, and lists showing their comparative abundance are given. *Carterina spiculolesta*, an interesting find, is recorded from Guernsey and Jersey. Rhizopoda appear a second time in the same publication, for Mr. James Cash has an illustrated paper on the freshwater forms of the Manchester district. M. Fornasini has just published, in *Mem. Accad. Sci. Bologna*, a note on *Nodosaria obliqua* (Linn.), showing the variations of this form. The paper is accompanied by an excellent plate.

Dr. Beneche, the director of the experimental station at Klaten, Java, has, according to *Nature*, offered a prize of 1,000 marks for the best essay on the causes of the red colour in the fibro-vascular bundles of *Sorghum* which accompanies the disease known as "sereh." A similar disease has recently become very destructive to the sugar-cane crop in Java. Dr. Beneche had already published some results of experiments for combating the disease in 1891 ("Proefnemingen ter bestrijding der 'Sereh,' 80, *Semarang*"), and in *De Indische Gids* for December, 1891. Attention is now called to a second paper, published in the "Medederling," of the Middle-Java Experimental Station, entitled "De bestrijding des onder den naam 'sereh' samengevatte ziekteverschijnselen van het suikerriet," dealing more fully with this serious trouble. Further information on this and many other plant diseases and pests will be found in the *Zeitschrift für Pflanzenkrankheiten* (Stuttgart), a journal edited by Dr. Paul Soraner, and which, besides supplying many original articles, collects those published in little-known serials.

Botanists will be glad to hear that Mr. Daydon Jackson's Index to Flowering Plants is in progress of printing by the Oxford University Press. Nearly the whole of the letter B is in type. The passing of the proofs of such a monumental work as this through the Press is extremely arduous, as collation with the whole of the rest of the manuscript is needful for completion. It is difficult to realise the labour involved in such a book of reference.

The library founded by M. Adrien Dollfus for the benefit of the readers of *La Feuille des Jeunes Naturalists*, grows apace. In fascicule 15 of the Catalogue, just issued, the additions received between January 21 and April 20, 1890, are recorded under numbers 19,233 to 20,519, a total of 1,286 books and pamphlets. The Catalogue is classified under subjects, and the books are available to all subscribers

dwelling in France, on payment of the necessary postage. It is greatly to be hoped that the readers of M. Dollfus' interesting publication avail themselves of the exceptional generosity of the editor in providing this library for them, which, so far as we know, is unique of its kind.

We are glad to see that, among others, the widows of the late Dr. P. H. Carpenter and Mr. G. T. Bettany have been awarded pensions from the Civil List.

The month of July has been remarkable for the succession of catastrophes that have been recorded during it. The most interesting of these is the reported destruction of Sanguir, an island about 130 miles from the northern end of Celebes; according to a Dutch captain this was blown to pieces on the 7th of June by a volcanic explosion, which recalls to mind the famous destruction of Krakatoa. The island covers a little over 100 square miles, and its population is estimated at about 12,000, but this would be a high average for a Malaysian island. If such an explosive eruption has really occurred, it ought to be verified by brilliant sunsets before authentic details can be received.

Etna is now in eruption, and serious damage has been done near Catania, while Vesuvius has also commenced operations, and a strong stream of lava is flowing down the crater into the Atrio del Cavallo, where we hope it will be content to stop. Simultaneous eruptions of Vesuvius and Etna are very unusual, and this adds to the interest with which further progress will be watched. We hope to give a detailed account next month.

Switzerland has been occupying public attention before the usual time, as the season of climbing accidents has barely commenced. The destruction of St. Gervais, however, has caused more deaths in a single night than have all the climbing misadventures of the century. Numerous proposals have been made for the prevention of such disasters in the future, of which the favourite one is a periodical inspection of glaciers; but if the inspection merely results in an order to the glacier to be on its best behaviour, as seems to be the Swiss method of dealing with doubtful boilers, it will not do much to allay uneasiness. The only effective means of preventing such disasters would be to avoid building hotels in narrow gorges, for so many of these bear upon their banks evidence of similar inundations. They have not, however, been often recorded, and the descriptions of the present flood may well replace those of the similar irruption of the Lac de Bagnes down the Dranse valley, which as one of the stock illustrations of geological text books has long been worn threadbare.

I.

The Permanence of the Great Oceanic Basins.

THAT the great Oceanic Basins, as well as the land areas of the globe, have persisted throughout a large portion, if not the whole, of known geological time, is a proposition which has been accepted by writers of such eminence, and is supported by so many distinct lines of evidence, that it seemed likely to become one of the established teachings of geology. Professor Dana was led to it by a study of the development of the North American Continent; Darwin upheld it from his study of Oceanic Islands, and the facts he adduced have since been strengthened by the discovery that the two supposed exceptions to the generalisation that no ancient sedimentary rocks occur on such islands—Rodriguez and St. Paul's Rocks—are no exceptions at all. Two successive heads of the Geological Survey of Great Britain, Sir Andrew Ramsay and Sir Archibald Geikie, have advocated similar views; while Mr. John Murray, of the "Challenger," holds that the vast mass of evidence now accumulated as to the nature of the deposits on the floors of the great oceans, indicates that they are distinct in character and origin from any of the widespread formations which make up the series of the sedimentary rocks. Coming to the subject from a totally different point of view, that of the physicist and mathematician, the Rev. Osmond Fisher arrives at similar results. In the latest issue of his important work, "Physics of the Earth's Crust," he gives as one of his conclusions—"and lastly, that the great oceanic and continental areas have never changed places"; and, in the summary of the whole work, he says: "The occupation of an entire hemisphere by one great ocean is a remarkable circumstance, and we have seen reason for believing that this is a very ancient division of the surface, and that it is probably a mistake to suppose that every part of it has been sometimes raised above the sea, and sometimes depressed beneath it. The truth seems to be, that the region subject to these alterations of conditions does not extend very far away from the present coast lines."

When studying the causes which have brought about the geographical distribution of animals, I was compelled to deal with this question, because I found that it had been the custom of many writers to solve all anomalies of distribution by the creation of hypothetical lands, bridging across the great oceans in various directions

and at many different epochs; and, having arrived at the conclusion that the distribution of organisms could be more harmoniously and consistently explained without such changes of sea and land, which usually created greater difficulties than those they were intended to explain, I gave, in my "Island Life" a brief statement of the evidence which appeared to me to render such changes exceedingly improbable. This evidence was mainly a summary of the facts and arguments adduced by the eminent men referred to above, and to this I added in my "Darwinism" a difficulty founded on mechanical considerations which seemed to me to furnish a preliminary reason why we should not accept the doctrine of the interchange of continental and oceanic areas without very clear and cogent reasons. Since then some other arguments of this nature have occurred to me, and as the theory of permanence has been recently attacked, by Mr. W. T. Blanford in his presidential address to the Geological Society in 1890, and by Mr. Jukes-Browne in his "Building of the British Isles," it may be as well to consider these difficulties, which furnish, in my opinion, a very powerful argument against the interchange of oceanic and continental areas, and which has the advantage of not requiring any knowledge of the higher mathematics in order to estimate its validity.

And, first, it is necessary to clear away some misconceptions as to the proposition I really uphold, since arguments have been adduced which in no way affect that proposition. Thus, Mr. Jukes-Browne quotes Professor Prestwich as saying, "It is only the deeper portions of the great ocean troughs that can claim the high antiquity now advocated for them by many eminent American and English geologists." But this is all that is claimed. For practical purposes I at first took the 1,000-fathom line as, generally and roughly, indicating the separation between the oceanic and the continental areas, because at that time it did accurately divide the continental from the oceanic islands, as defined by a combination of geological and biological characters. It has, however, been since shown that two ancient continental islands—Madagascar and New Zealand—are separated from their respective continents by depths of more than 1,000 fathoms. We must, therefore, go as far as the 1,500-, or, perhaps, in a few cases, to the 2,000-fathom line, and this will surely mark out "the deeper portions of the great ocean basins," since only isolated areas exceed 3,000 fathoms.

Now, if we look at the deep ocean basins marked out by the 2,000-fathom line, not on Mercator's projection which greatly exaggerates the shallower portion situated in the temperate and polar regions, but on an equal-area projection, such as the map which illustrates Mr. J. Murray's paper,[1] we shall see that by far the larger part of all the great oceans are included by this line, and that, for

[1] "On the Height of the Land and the Depth of the Ocean," *Scottish Geographical Magazine*, 1888.

the purpose of indicating the isolation of the continents from each other throughout the equatorial and most of the temperate zones, there is very little to choose between the 1,000-fathom or the 2,000-fathom boundary. The latter, however, allows more scope for possible land extensions between the three southern continents and the Antarctic lands, which, during mild epochs, and by means of intervening islands, may, perhaps, have served as a means of communication between these continents. All that is necessary to maintain, therefore, is that existing continents with their included seas, and their surrounding oceanic waters as far as the 1,500-fathom, or in some extreme cases the 2,000-fathom line, mark out the areas within which the continental lands of the globe have been built up; while the oceanic areas beyond the 2,000-fathom line, constituting, according to Mr. Murray's data, 71 per cent. of the whole ocean, have almost certainly been ocean throughout all known geological time.[2]

It will now be seen that this is a problem which deals with the very broadest contrasts of the earth's surface, and that its fundamental data are on so vast a scale as not to be materially affected by the smaller details of physical geography, or by differences of opinion as to the exact meaning of certain terms. Whether a particular island is more correctly classed as oceanic or continental, whether a certain portion of the ocean should be placed within the oceanic or the continental area, and whether certain rocks were formed in very deep or in comparatively shallow water, are of slight importance, except in so far as they may throw light on the real question, which is, whether the vast expanses of ocean beyond the 1,000-fathom line, forming about 92 per cent. of the whole oceanic area, have ever been occupied, or extensively bridged over, by continental land. It is towards the solution of this great problem that I now propose to submit certain general considerations which appear to me to lie at the root of the whole matter.

Comparison of Oceanic and Continental Masses.—In the paper already referred to, Mr. John Murray has carefully estimated both the area of the land and of the water on the earth's surface, and their bulk as deduced from the best available data. Taking the whole area of the globe as 100, he finds the land surface to be 28, the water surface 72. But the mean height of the land above sea-level is 2,250 feet, while the mean depth of all the oceans and seas is 12,456 feet. In this estimate, however, all the inland seas and shallow coast waters are included, and as these, at least as far as the 100-fathom line, are universally admitted to be within the "continental area," we omit them in our estimate of the mean depth of the oceans proper, which are thus brought to something over 15,000 feet, or nearly seven times as much as the mean height of the land.

[2] Mr. Murray gives his results for oceans and inland seas together. The above refers to the oceans only as usually understood; but the difference is not great.

The accompanying diagram (taken from my book on "Darwinism") will better enable the reader to appreciate these proportions, which are of vital importance in the problem under discussion. The lengths of the two parts of the diagram are in proportion to the areas of land and ocean respectively, the vertical dimensions showing the comparative mean height and depth. It follows that the areas of the two shaded portions are proportional to the bulk of the continents and oceans respectively.

The mean depths of the several oceans and the mean heights of the several continents do not differ enough from each other to render this diagram a very inaccurate representation of the proportion between any of the continents and their adjacent oceans; and it will therefore serve, roughly, to keep before the mind what must have taken place if oceanic and continental areas had ever changed places. It will, I presume, be admitted that, on any large scale, elevation and subsidence must nearly balance each other, and, thus, in order that any area of continental magnitude should rise from the ocean floor till it formed fairly elevated dry land, some corresponding area must sink

Diagram of proportionate mean height of Land and depth of Oceans.

to a like extent. But if such subsiding area formed a part or the whole of a continent, the land would entirely disappear beneath the waters of the ocean (except a few mountain peaks) long before the corresponding part of the ocean floor had approached the surface. In order, therefore, to make any such interchange possible, without the total disappearance of the greater portion of the subsiding continent before the new one had appeared to take its place, we must make some arbitrary assumptions. We must suppose either that when one portion of the ocean floor rose, some other part of that floor sank to greater depths till the new continent approached the surface, or, that the sinking of a whole continent was balanced by the rising of a comparatively small area of the ocean floor. Of course, either of these assumed changes are conceivable and, perhaps, possible; but it seems to me that they are exceedingly improbable, and that to assume that they have occurred again and again, as part of the regular course of the earth's history, leads us into enormous difficulties. Consider, for a moment, what would be implied by the building up of a continent the size of Africa from the mean depth of the ocean. By

comparing the area of Africa with that of the whole of the land, and the depth of the ocean with the mean height of the land, we shall find that if all the land of the globe above sea-level could be transferred to mid-ocean, it would not be sufficient to form the new continent, but would still leave it nearly 2,000 feet beneath the surface.

It thus appears that, if the elevation of the ocean floor, and the corresponding sinking of whole continents, constitute a portion of the regular change and development of the earth's surface, there would be not only a chance but a great probability of entire continents disappearing beneath the waters before new continents had risen to take their place. Even the total disappearance of all the large land masses might easily happen; for we see from the diagram that they might one after the other disappear with a corresponding rise of the adjacent portion of the ocean bed and still leave the ocean over the whole earth almost as deep as it is now. But, as will be shown further on, the geological record, imperfect as it is, teaches us that no such submergences have ever taken place.

Contour of the Ocean Floor as indicating Permanence.—Before extensive soundings revealed the depth of the ocean and the form of its floor, it was supposed that it would exhibit irregularities corresponding to those of the land, such as mountain-ranges, great valleys, escarpments, ravines, &c. But we now know that the main characteristic of the ocean floor is, that it is a vast undulating plain, the slopes rarely exceeding a hundred feet in a mile except near the margins of the continental areas, while usually the gradients are so slight that they would be hardly perceptible. Contrast this with the forms of all mountain ranges whose general rise for long distances is often five hundred feet in a mile, while slopes at angles of from 20° to 60° are by no means uncommon.

Now if we suppose that considerable portions of the ocean depths have been formed by the subsidence of continents, we should certainly expect to find some indication of those surface features which characterise all continents, but which appear to be absent from all deep oceans. In order to account for the actual contours of the ocean on this theory, we must suppose that, during subsidence, all the mountain ranges, peaks, valleys, and precipices were reduced to an almost uniform level surface by marine denudation, which, unless the process of subsidence were incredibly slow, seems most improbable. Mr. Jukes-Browne, however, does not hold the view that they have been thus denuded, for he approvingly quotes Mr. Crosby as saying that—"the oceanic islands are, of course, merely the tops of submerged mountains, and it is only with the highest points of continents that they can properly be compared." If this is correct, then we ought to find in the vicinity of such islands all the chief features of submerged mountain ranges—precipices, deep valleys and ravines, arranged in diverging groups as they always

occur in nature. But in no single case, that I am aware of, have any such features been discovered.

But a still greater difficulty remains to be considered. If oceanic and continental areas are interchangeable, it can only be because the very same causes (whatever they may be) that produce elevations and subsidences in the one produce them also in the other, and at first sight it appears probable that this would be the case. But if these causes have been at work upon the ocean floor throughout geological epochs, they would have produced irregularities of surface not less but far greater than on subaerial land. This must be so, because subaerial denudation continually neutralises much of the effect of upheaval in the continental areas, while in the ocean depths no such cause or anything analogous to it is in operation.

The forces which have been at work in every mountainous region have sometimes tilted up great masses of rock at high angles, upheaved them into huge anticlinal curves, or crushed them by lateral pressure into repeated folds, which in some cases appear to have fallen over so as to reverse the succession of the strata. But, notwithstanding these various forms of upheaval, involving vertical displacements which are sometimes several miles in extent, the surface of the land often shows no corresponding irregularities, owing to the fact that subaerial denudation has either kept pace with upheaval or has even exceeded it, so that the position of an anticlinal ridge may be, and often is, represented by a valley. Now, if we suppose that similar forces have been at work on that portion of the earth's surface forming the bed of the ocean, where there are no such counteracting agencies, we should expect to find irregularities in the ocean floor far greater than those which occur upon the land surface.

Still more difficult to explain would be the absence of precipitous escarpments due to faults, which, though frequently showing an upthrow or downthrow of the strata to the amount of many hundreds and sometimes thousands of feet, rarely exhibit any difference of level on the land surface, owing to the fact that subaerial denudation has kept pace with slow and intermittent elevation. But in the ocean depths no such denudation is going on; and we can therefore only account for its very uniform surface on the supposition that it is not subject to the varied and complex subterranean movements which have certainly acted within the continental areas throughout known geological time.[3]

Equal Range of the Geological Record in all the Continents.—There is one other general consideration which indicates the permanence and continuity of the Continental Areas, and which renders it very diffi-

[3] The Rev. O. Fisher has arrived at this conclusion from his own researches. He says: "The compression which has caused the thickening accompanied by corrugation, such as characterises most elevated tracts, is properly a continental phenomenon, and has no analogue beneath the oceans." L.c., p. 253.

cult, if not impossible, to suppose that they have ever changed places with the great oceans. It is, that on all the present continents we find either the same or a closely parallel series of geological formations, from the most ancient to the most recent. Not only do we find Palæozoic, Mesozoic, and Caniozoic rocks everywhere present, but, in proportion as the continents are explored geologically, we find a tolerably complete series of the chief formations. From Laurentian to Carboniferous and Permian, from Trias to Cretaceous, and from Eocene to Quaternary, the geological series appears to be fairly represented not only in continents, but also to a considerable extent in the large continental islands, such as Great Britain and New Zealand.

Now this is certainly not what we should expect if the present continental areas had, at different epochs, risen out of the deep oceans. In that case some would have commenced their geological history at a later period than others, having either a late Palæozoic or some Mesozoic formation, or even an early Tertiary for their lowest stratified rock. Others, which had become oceanic for the first time at a later epoch, would exhibit an enormous gap in the series, either several of the Mesozoic formations, for example, being absent, or some considerable portion of both Palæozoic and Mesozoic, or Mesozoic and Tertiary. This would necessarily be the case, because we cannot believe that so vast a change as the subsidence of an entire continent till its site became a deep ocean, and its subsequent elevation till it again became dry land, could possibly be effected in any less extended periods, if at all.

Whenever such gaps, or smaller ones, now occur locally, they are generally held to imply the existence of terrestrial conditions, as in the case of China, which, according to Richthoven, has been continental since the Carboniferous epoch. In many cases there is distinct evidence of such conditions in lacustrine or freshwater deposits, dirt-beds, &c. But if a gap of such vast extent both in space and time as that here referred to were caused by the interchange of a continent and a deep ocean, the fact that it was so produced would be clearly evidenced by an almost uniform deposit either of organic or clayey ooze, similar to those now everywhere forming over the oceanic area. Even if we make the fullest allowance for denudation during elevation, sufficient indications of so widespread a formation should be detected. Such a deposit would, in fact, have every chance of being largely preserved, because, long before it rose to the level where it would be subject to denudation by waves or currents, it would, almost everywhere, be overlain by a series of shore deposits, and wherever these latter were preserved on the land surface the oceanic formation would necessarily be found under them. That no such enormous deficiency in the geological series characterises any of the continents, and that no widespread deposit of organic or clayey ooze at some

definite horizon has been anywhere detected, though such a deposit must have been formed and largely preserved if the whole or any considerable part of a continent had risen from ocean depths at any period of its history, constitutes of itself a very strong argument against any such interchange of oceanic and continental areas having occurred.

I have now shown that there are three distinct groups of phenomena which are either altogether inconsistent with any general interchange of oceanic and continental areas, or which render it exceedingly difficult to understand how such interchange could have been brought about. These phenomena are:—(1) The enormous disproportion between the mean height of the land and the mean depth of the ocean, which would render it very difficult for new land to reach the surface till long after the total submergence of the sinking continent. (2) The wonderful uniformity of level over by far the greater part of the ocean floor, which indicates that it is not subject to the same disturbing agencies which throughout all geological time have been creating irregularities in the land-surface, irregularities which would be far greater than they are were they not continually counteracted by the lowering and equalising effects of subaerial denudation. (3) The remarkable parallelism and completeness of the series of geological formations in all the best known continents and larger continental islands, indicating that none of them have risen from the ocean floor during any portion of known geological history, a conclusion enforced by the absence from any of them of that general deposit of oceanic ooze at some definite horizon, which would be at once the result and proof of any such tremendous episode in their past history.

I submit that these facts, and the conclusions to be logically deduced from them, form a very powerful *à priori* argument as against those who maintain the interchange of continents and oceans as a means of explaining certain isolated geological or biological phenomena; such, for instance, as the much-disputed origin of the chalk, or the supposed necessity for land-communication to explain the distribution of certain groups of reptiles or fishes in remote geological times. Before postulating such vast revolutions of the terrestrial surface in order to cut the gordian knot of difficulties which may be mainly due to imperfect knowledge, it will be necessary to show that the considerations here adduced, as well as the great body of facts which have caused many eminent geologists, naturalists, and physicists to hold the doctrine of oceanic permanence, are either illogical or founded on incorrect data. For, surely, no one will suggest that so vast a problem of terrestrial physics can be held to be solved till we have exhausted all the evidence at our command, and have shown that it largely preponderates on one side or the other.

The present article is intended to supply a hitherto unnoticed class of arguments for oceanic permanence, and these must of course

be taken in connection with the other evidence which has been summarised in the 6th chapter of the writer's "Island Life," and also with the admirable "Summary" of the purely physical argument in the 2nd edition of Mr. O. Fisher's "Physics of the Earth's Crust." It is certainly a remarkable fact that writers approaching the subject from so many distinct points of view—as have Professor Dana, Mr. Darwin, Sir Archibald Geikie, Dr. John Murray, Rev. O. Fisher, and myself—should yet arrive at what is substantially an identical conclusion; and this must certainly be held to afford a strong presumption that that conclusion is a correct one.

ALFRED RUSSEL WALLACE.

II.

The Climate of Europe during the Glacial Epoch.

CLIMATIC changes in byegone times may account for many peculiarities in the present geographical distribution of animals and plants. The last "Glacial Epoch," however, has been held responsible for so many anomalies, that it is not surprising to find the more cautious naturalist somewhat chary of referring anything to this cause. Still, it is impossible to doubt that important changes of climate have taken place within the lifetime of existing species, and that these climatic changes necessarily led to much extermination and forced migration. Evidence of this may be found, no doubt, in the present distribution of animals and plants; but clearly to understand the origin of the existing faunas and floras, we must first master the outlines of their former distribution. We must learn, also, what climatic and orographic changes have taken place, and what parts of the world these changes affected.

All this may seem self-evident. But though biologists now understand that embryology is essential, and that the study of extinct forms cannot be neglected, yet the writer on geographical distribution is often content to speculate on what may have been, without reference to the geological evidence as to what really did happen. It is as if we read our history, like a witch's prayer, backwards; or as if, in writing a biography of a celebrated man, we commenced with his funeral, or described nothing but his last moments. Such a method of studying history would be absurd; but it is the method often adopted in natural history.

A rapidly increasing body of naturalists recognises, however, that there is an evolution of faunas and floras to be studied, as well as an evolution of species. For the benefit of these naturalists it may be useful to summarise the present state of our knowledge of the later Tertiary changes in Western Europe. This I venture to do; but it must be confessed that we are as yet merely commencing a very large enquiry, and that unexpected and often puzzling pieces of evidence are met with daily. We shall ultimately, I hope, be able to write a fairly continuous epitome of the most recent chapter in the earth's history; at present we can only deal with more or less

detached episodes. Yet, as with casual finds of ancient documents, a study of these episodes will often throw light on what happened much later, though the intermediate history may still be a blank.

The first question here to be dealt with will be, What was the temperature in Britain during the Glacial Epoch, and how far did the cold affect more southern regions? In discussing this matter, I do not propose in any way to speculate on the cause of the climatic change; under the present circumstances it is, perhaps, better to treat the question simply as one of evidence. There are several theories in the field; if one of them can be made to fit the facts, well and good, but we must not allow any preconceived theory to bias us in this enquiry.

When we attempt to learn what climatic conditions held during past times, several different classes of evidence are available. We can treat the question as a purely physical one; we can treat it from a botanical point of view; or from a zoological. Finally, we can combine the independent chains of evidence, and if they all lead to the same conclusion we may consider that we have established a safe working hypothesis.

To the naturalist the most interesting part of the enquiry will probably be, What was the lowest annual temperature met with during the Glacial Epoch? This question, till lately, would have been difficult to answer with any confidence, but recent advances in our knowledge of the present condition of the Arctic Regions, and recent studies in Pleistocene geology, make a fairly accurate estimate quite possible.

The temperature of the sea and of the air do not necessarily correspond in the same regions. We will, therefore, first discuss the evidence as to the lowest temperature of the seas around Britain. For this purpose the former southern limit of floating ice, and the southern limit of the formation of shore ice, or "ice-foot," ought to give a fairly accurate idea as to the temperature of the water. No doubt a large iceberg may travel a long distance through comparatively warm water before it entirely melts away; but shore ice, such as forms every winter in the Arctic Regions, must float in water nearly as cold as itself if it is to carry its burden of beach-stones and erratic blocks any great distance.

The English Channel, during the greatest severity of the Glacial Epoch, seems to have occupied the southern limit of the formation of ice-foot—or, at any rate, of shore-ice sufficiently thick to transport anything but small stones. Ice-foot such as collects every winter in the Arctic Regions then fringed our south coast, and beset the shores of Brittany and of the Channel Islands. When, in the spring, this ice became detached it transported its burden of included rocks hither and thither, even across the Channel. We thus find at Selsea Bill, in Sussex, erratics weighing several tons, but undoubtedly transported from Bognor or from the Isle of Wight. Others, equally large, have

probably come from the Channel Islands and the coast of Brittany; one block of granite is probably from Cornwall. This fact of the transportation of large erratic blocks for distances of at least a hundred miles, shows that the temperature of the water in the spring, though sufficiently high to dislodge the ice, was yet too low to melt it rapidly. Even with a strong wind a flat mass of shore-ice would take several days to cross the Channel.

To compare this ancient ice-laden English Channel with existing seas, it is necessary to travel northward, till we cross the isotherm of 32° F., and are near the Arctic Circle. The English Channel, being almost land-locked, no cold current like that of Labrador can have had much influence on the temperature of the water. It is possible, however, that the Channel was then much less salt and froze more readily, but even in that case the winter temperature can scarcely have been less than 20° lower than at present.

Turning next to the evidence yielded by the marine fossils, we are still unacquainted with the arctic species of the south of England. The deposits which probably contain them are, unfortunately, beneath the sea-level, and we are unable, till a marine fauna is obtained, to speak confidently of the temperature of the water, or to say whether it was salt or brackish. The shores of the North Sea, however, yield much more satisfactory evidence; for at Bridlington, in Yorkshire, the Boulder Clay contains some included shelly masses with the most intensely arctic marine fauna yet found in Europe. Indeed, this fauna is as thoroughly arctic as any now living in the Arctic Regions, and must have flourished in a sea whose temperature was little above the freezing-point. The fauna of the "Bridlington Crag" does not, however, mark the maximum intensity of the cold, for the Till in which these shelly masses are embedded indicates a still lower temperature, which caused solid ice to fill the whole of the North Sea, and to plough up the marine clays previously deposited. As far as the evidence will allow us to judge, the North Sea, then as now, was a few degrees colder than the English Channel. No trace of ancient glaciers has yet been found on the shores of the Channel.

Carrying our investigations further south, we find in France few indications as to the temperature of the sea during any part of the Glacial Epoch. In the Mediterranean region, however, certain upraised sea-bottoms on the flanks of Mount Etna yield the evidence for which we are searching, for they indicate by their included fossils a temperature a few degrees—perhaps 5°—lower than that of the present sea. Of course, it is not certain that these Sicilian deposits were laid down during the greatest intensity of the cold; still the series of Pliocene and Pleistocene strata seems there to be complete, and careful search has not led to the discovery of any fossil fauna of more boreal character in other parts of the Mediterranean basin. If no indications of greater cold are to be found in the ancient marine

deposits of the Mediterranean, we may, therefore, consider that the severity of the Glacial Epoch died out rapidly towards the south, a refrigeration of 20° in Britain being represented by one of 5° only in the South of Europe.

Thus far we have dealt solely with the temperature of the sea. We will now study the evidence as to the temperature of the air during the same period, and here again we can employ both physical and biological data. The evidence for the former existence in Northern and Central Europe of vast sheets of ice, of greatly extended glaciers, and of deeply frozen soil, is overwhelming, and is so familiar that it need not again be discussed. All the country north of the Thames, and north of a line drawn eastward through Saxony, was buried under a perennial sheet of ice and snow. Near this limit, and apparently advancing or retreating with the ice, stretched a belt of barren and frozen land, with dwarf birch and willow and other arctic plants, but without trees. In this belt flourished also a mammalian fauna like that now inhabiting the Arctic Regions.

Remains of this boreal fauna and flora have now been found so abundantly, and the list includes so many species, that it would be impossible in the limits of a short article to give more than one or two examples. Taking the examples from what is now the warmest part of our islands, we may instance the dwarf birch and the bear-berry found in clays at Bovey Tracey, in Devonshire, though both these species are arctic and alpine plants not now living in the South of England. In several parts of Norfolk and Suffolk a still more arctic flora is found, for it includes the *Salix polaris*, a plant now extinct in Britain, even on the highest mountains. Arctic mammals, also, must formerly have been abundant in the South of England, for besides the lemmings and musk-ox of the Thames Valley, we have at Fisherton, near Salisbury, quite an extensive arctic fauna.

An arctic flora like that just described has lately been traced through great part of Central and Northern Europe by Professor Nathorst; and the corresponding mammalian fauna, with the interesting addition of certain species now inhabiting the cold deserts of Central Asia, is gradually being brought to light, principally through the exertions of Professor Nehring.

To compare then with now, we must again go to high northern regions, for it is only within the Arctic Circle that we can find physical conditions, and a fauna and flora, like those which once characterised our islands. Britain in those days was probably much like the existing Greenland, though with important differences caused by geographical conditions. After making every allowance for the probable excessive snowfall in Western Europe, it seems impossible to place the average temperature at the southern margin of the ice-sheet higher than the freezing-point—that is to say, 20° lower than at present. It must be remembered that we are dealing mainly with a low flat country, not much above the sea-level, and distant

from any mountains. If the temperature had been much above the freezing-point the ice would melt before reaching London. Even an excessive snowfall would scarcely allow the ice to travel so far unmelted, unless the air were very cold.

The arctic plants and animals found fossil in Britain point to a similar conclusion, for the same species now flourish in a climate fully 20° colder than that of the lowlands of Britain.

Further south, though the Alps, Pyrenees, and Caucasus show evidence of a former greater extension of the glaciers, yet, when we

ISOTHERMAL MAP, SHOWING THE PROBABLE MEAN ANNUAL TEMPERATURE (FAHRENHEIT) DURING THE GREATEST SEVERITY OF THE GLACIAL EPOCH.

NOTE.—No attempt has been made to restore the ancient topography; as far as yet known there was little change in the boundaries of sea and land during the Glacial Epoch.

remember that extensive snowfields must cause fogs and greater condensation locally, it does not appear that the climate in the lowlands of Southern Europe can have been so much as 20° below the present mean; 10°, or perhaps less, seems more likely to have been the extent of the refrigeration in the Mediterranean region. Though the reindeer was once abundant in Central France, few remains of northern mammals have yet been found near the Mediterranean.

All the evidence yet available seems to show that during the Glacial Epoch the temperature increased rapidly towards the south, and that the effects of the cold did not extend very far towards the equator. Future discoveries in North Africa may necessitate a modifi-

cation of these views; but it should not be forgotten that the northern shores of the Mediterranean have been well explored geologically, and that Italy and Sicily especially have long been classic ground for the student of Pleistocene geology.

To indicate more clearly the probable condition of Europe during the greatest intensity of the cold, I have ventured to construct an isothermal map from the data now available. Of course, the isotherms are mere rude approximations, to be corrected and improved as materials accumulate; but for the purpose of the student of natural history, and especially of geographical distribution, I think that the map may be useful. It will show at least that extensive regions must have been quite uninhabitable for their present fauna and flora. In other regions the crowding of the isotherms would probably bring into competition species that seldom meet under present conditions; and this crowding may help to account for some of the anomalies in the occurrence of the Pleistocene mammalia. A great deal of the supposed discordance in the evidence results, however, from careless observation, and from the deposition of already fossilised bones among others of later date. My own observations have shown few genuine cases of discordant evidence as to climate among the mammals, and none at all in other groups.

An objection, apparently fatal, will occur to any naturalist who attempts to verify the conclusions above arrived at. He will remark that surely in all the thousands of years that have passed since the origin of existing species, many forms must have become acclimatised, and if the physical change were sufficiently slow the animals and plants would accommodate themselves to the new conditions. Had this question been put a few years since I should have felt that the objection was perfectly valid; but one of the most unexpected results of the study of later Tertiary natural history has been to show that, in a wild state, species seldom become acclimatised. They are exterminated, or die out locally, or, perhaps, in a few cases, change into other so-called species, with each climatic change; but every recurrence of cold or warm conditions causes the reappearance of the appropriate fauna and flora.

Only one phase of the Glacial Epoch, the lowest temperature reached, has been dealt with in these notes. It still remains to speak of interglacial mild episodes, of non-glaciated areas surrounded by ice ("Nunataks," as they are called in Greenland), of arid episodes, and of the extremely hypothetical "Pluvial Period." These subjects can, perhaps, be better treated of in a separate article, but it may be advisable to give a word of caution to naturalists who are inclined to treat the severity of the cold and the accumulation of snow as the only climatic factors to be taken into account. Recent observations have shown more and more clearly that throughout Central Europe there was also a period of *dry* cold, causing that region to resemble the arid wastes of Central Asia.

These desert conditions seem to have extended, in a modified degree, into the South of England. In certain deposits we find remains of various locally-extinct species which seem to have nothing in common among themselves, except that they all now live in sunnier climes than ours.

CLEMENT REID.

III.

Death.

THIS makes no pretension of being an elaborately thought-out essay on "Death." It is the result of an attempt to clear up my own ideas on a subject which, when vitalistic conceptions are spreading so rapidly among biologists, seems to be a last outpost of the other school, and yet an outpost which may be the centre of its victory.

I propose to offer no historical introduction; for just as nothing so entirely unfits one to understand current political problems as what is called the study of history, so, however interesting to the curious, the past history of any scientific controversy is the most misleading of all guides to its subsequent development.

In biology, as in morals, simplicity of term invariably covers confusion of idea. The apparent opposites life and death, like the apparent opposites right and wrong, extend as a vast tangle of intermeshing ideas. As out of a naïve acceptance of right and wrong as simple ideas come the cruel follies of the conscientious, so out of a naïve acceptance of the simple words life and death as corresponding to simple facts comes the grotesque abstraction—vitalism.

There are three ways in which an organism may come to an end. By some gross accident of life, as when one cleans an amœba off a slide with a duster, or oneself trifles with nitro-glycerine, an organism may be swept out of existence. Or, and specially in the case of the higher organisms, some alteration in the machinery, some slight defect in the mechanism, may interfere fatally with the whole. Or, lastly, the wrought iron itself may slowly oxidise, or by constant vibration become crystalline, the cells and protoplasm of the living organism may decay. Of course, the divisions of this classification, like those of all classifications that do not deal with beans or buttons, or the subject matter of metaphysics, are not mutually exclusive. They represent only the point of view for the moment convenient to the classifier.

With the first mode of death, though it probably enormously preponderates in the animal world—so enormously as to make it doubtful whether natural selection can have anything to do with fixing the term of natural life—we have at present little to do.

The individual life has been destroyed by a mechanical force: the potential energy of the organism provides food for luckier organisms, and sooner or later passes from the world as heat. But in the large organism, as in the small, what has taken place is mechanical interruption of a complicated process, resulting ultimately in chemical death—the running off of the potential energy.

The leading idea in this paper is to find in the various phenomena grouped under the term death, never the opposition of a kindred mysterious force to the mysterious vitalism, but the action of known mechanical or chemical agencies, and to suggest the inference that the life of the organism or of the cell, coming to an end as it does by known agencies, cannot itself depend in essence on any transcendental force; that, in fact, as the negative conditions of life are purely chemical and physical, life itself, however complicated, must be a congeries of phenomena ultimately to be resolved into purely chemical and physical forces.

When we consider the higher animals we find that death is always more or less violent. As Michael Foster says, "The central factor of life is the circulation of the blood, and the blood is not only useless but injurious unless it be duly oxygenated, and in the higher animals the oxygenation of the blood can be duly effected by means of the respiratory muscular mechanism presided over by the respiratory centre in the bulb. Thus, the life of a complex animal is, when reduced to a simple form, composed of three factors—the maintenance of the circulation, the access of air to the hæmoglobin of the blood and the functional activity of the respiratory centre, and death may come from the arrest of any one of these three. As it has been put, death may begin at the heart, or the lungs, or the brain."

It requires little medical knowledge to refer the final symptoms of any fatal disease to one of these—the three gates of death of the older writers. Turning over a few of them, one finds the proximate causes of death to group themselves simply.

Death through the heart may come by inhibition, as when a blow on the abdomen or iced milk on the stomach stimulates the peripheral ends of the vagus. Or it may come by cardiac failure as in fatty degeneration, or in valvular diseases.

Death through the lungs may come as asphyxia from lung diseases, from capillary bronchitis, or from pneumonia. Or asphyxia may result from anæmia caused by loss of blood from hæmorrhage or want of hæmoglobin. Or the respiratory nerves may be paralysed, as in diphtheria or bulbar paralysis.

And, lastly, death through the brain may come in a multitude of ways. Centres may be suddenly destroyed by wounds, or by the culmination of tumours, or they may be paralysed by high temperature, or by a multitude of organic poisons arising from organic diseases, like Bright's, or produced by parasitic bacteria.

Even in death from old age we find the dark mother throwing

open one of the three gates. The frame of a higher animal does not, like the autocrat's one-horse shay, slowly and equally decay until there is a simultaneous collapse of everything—until each cell is senile and moribund—but the heart or the lungs, or some centre in the brain suddenly gives way. But here, perhaps, more plainly than in other cases, we see that the violence or suddenness of death, on which so much stress has been laid, is the merest accident, due to the complexity of the organism. There has been going on for long a gradual degeneration of the walls of the blood-vessels—a degeneration showing itself by an accumulation of fat or of lime—products usually oxydised or excreted, and this slow cellular change has, by weakening the circulation, acted on all the organs of the body, and reacted on itself until some point is reached at which the gross mechanism breaks down. So, in the cases of most causes of death, we can trace back the chain of events until we find that what is said to cause death is only an accidentally last term in a long series of protoplasmic changes; changes, as in lung diseases, where the lung cells have been gradually decaying, or, as in Bright's disease, where kidney cells have been failing to do their work.

In short, protoplasmic changes have been going on, and when the results of these have sufficiently accumulated, the gross mechanism is suddenly arrested; or, more directly, an interference may result when some natural mechanism is accidentally overstimulated, as when reflex stimulation of the vagus stops the heart, or when a lethal alternating current is switched through the spinal cord; or again, a poison may paralyse a brain centre, and so the heart or the lungs be stopped.

It is clear that in all these cases—the modes of death treated of in the medical books—a confusion of ideas exists. The source of this confusion is clearly seen when one finds, for instance, Foster suggesting that the point of death cannot be determined, as it depends on the skill of the experimenter endeavouring to resuscitate a flickering vitality; or Weismann and Gotte gravely discussing whether there can be death without a corpse, and whether death is a subtle attribute of the metazoa teleologically built up by natural selection into its present form. We have to distinguish between protoplasmic death, and the stoppage of a complicated organism.

We know with absolute certainty that whatever be the details of the processes going on in a living cell, the sum of the activity is the acquisition of potential energy either indirectly by the transformation of kinetic energy, as in green plants, or directly by the absorption of chemical compounds possessing high heat equivalents—I mean compounds which, if oxydised in a calorimeter, melt relatively high quantities of ice: the change of these chemical compounds into substances that can melt relatively smaller amounts of ice. The difference of heat equivalent represents the work done by the cell, and the waste in doing the work. Protoplasmic death is an

arrest of this process, an arrest causing in the first place a diminution or cessation of absorption of fresh food—of more potential; and in the second, an increase in the heat waste, and consequent lowering in the heat equivalents of the discharged products. This lowering may be rapid, as where the cell tissues are rapidly oxydised in the air, or it may be a slow process, gradually going on in the bodies of animals or tissues of plants. But the essence of protoplasmic death is that the self-feeding gear is disarranged, that the cell either stops taking up more food, or that it uses up its food more quickly than it can take it, and so burns itself away.

In a complicated aggregate of cells, protoplasmic death of any one of them must necessarily be reflected on the others in some mechanical way, the simplicity of the path depending on the simplicity of the cell aggregate. The suddenness or violence of the interference will similarly depend on the degree of complexity of the cell aggregate. In an organism like man the arrest of the cell activity of the respiratory centre causes what is called sudden death of the entire organism. But obviously the suddenness, as the death itself, depends on the correlations between the organs—correlations of clearly mechanical nature—and the death of the entire organism, protoplasmic death of all the cells—comes only gradually, as the destructive changes set going creep along the mechanical channels. There is, of course, absolutely no reason why the skin of a man killed by chloroform or gunshot should not succeed as a graft quite as well as the skin from a living man, provided it were taken before the ripples of nutritive and other disturbance had spread to it. The tissues die slowly, and the time of death varies as the mechanical relations.

It is clear, then, that death of the organism, the death of the medical books and the popular imagination, is a function, not of the organism but of the complexity of the organism. It forms a mechanical middle term between protoplasmic death of some of the tissues and protoplasmic death of all the tissues. We may distinguish this as somatic death.

Running down the animal kingdom from the more integrated to the less integrated, we find somatic death gradually fading away till, in the protozoa, there is no distinction between somatic death and protoplasmic death. A shock that is fatal to civilised man is not infrequently borne with comparative ease by savage man, and might have little effect even on one of the higher mammalia. Brain operations and accidents instantly fatal to man can be survived for many days by pigeons, and the removal of great parts has little effect on the life of an amphibian.

Precisely the same condition of things can be inferred if we study the results of artificial section, or the conditions of natural budding. Bisection certainly would kill all the higher animals. In its natural form it becomes an increasingly common form of reproduction, as we

descend the animal scale. Moreover, in the embryonic stages of the higher animals—in those stages where integration is less complete—fission as an internal or as an external accident produces, not death, but double monsters or identical twins. So, also, if there be considered the suddenness of somatic death. The heart of a higher animal stops suddenly, and this accidental moment is named the moment of death. But the heart of a tortoise that has been in a dissecting dish for two or three days, begins to beat as the knife reaches it. The watchers by the death-bed of a fish or a mollusc would continue to watch until putrefaction was obvious. I have fed a semi-putrid anemone with pieces of mackerel, and have seen a star-fish with the tips of its arms rotten and pulpy crawl gaily about and evert its stomach on a pecten. In both of these cases the animals were moribund, and soon afterwards actually died. It is a commonplace of observation how rapidly the lower animals putrefy, and how necessary, to secure their histological preservation, it is to kill them as soon as possible after capture.

All this merely means that, in the first place, the stages of death creep over their simple framework so quietly that they are decaying before we know that they are dead; and, in the second, that protoplasmic death is more nearly identical with somatic.

Finally, in the descending scale, we reach unintegrated colonies of cells or single cells. Here there is identity between the protoplasmic individual and the soma; there is no somatic mechanism to break down, no chain along which protoplasmic death can creep from one cell to another.

It is clear, then, that somatic death is an attribute of the metazoa, a merely necessary result of their complexity of organisation, and that somatic death becomes a more and more striking occurrence as the metazoon becomes higher and more complex. It is hardly necessary to point out that a line of argument precisely similar applies to somatic life, and that what are usually meant by life and death are merely somatic life and death, the merest mechanical results of complex mechanism.

It is true that the protozoa are untouched by somatic death, that it has and can have absolutely no place in their life-cycle; that were somatic death the only death in the world, protozoa would be not potentially, but actually, immortal. It is equally true that no metazoan, however lucky, is at all likely to escape somatic death, is at all likely to be immortal. Even in the simplest metazoon, the life of the soma is bound up with the life of the units—or, at least, with the life of groups of the units—and the chances of protoplasmic death of any cell, or group of cells, being followed by somatic death, continually increases with the complexity of the organism, and increases in geometrical ratio as the number of component parts liable to accidental or intrinsic break-down.

The protozoa, then, and chains of single cell-life, like the

unicellular chain of germ cell-life of which the metazoa are the carriers, may be called potentially immortal, and the metazoa mortal. But all this merely deals with somatic death, and is, so to speak, a biological accident. And so discussion or argument dealing with the possibility of immortal protozoa giving rise to mortal metazoa—by, for instance, some unequal fission separating a hypothetically mortal part of the plasma from a hypothetically immortal, is futile.

There is left, then, protoplasmic death. It is certain that protoplasmic death is not a necessary concomitant of protoplasmic life. We know of no means to secure the set of conditions which we call life, without the presence of some small piece of living material; we have considerable reason to suppose that spontaneous generation does not occur, at least as a normal factor in the organic world; we know with absolute certainty that it never occurs in metazoa. From this it follows that a direct continuous chain of cell-life binds all animals with at least unicellular animals; that, for instance, we ourselves are at the end of a simple chain of cell division stretching through an inconceivable number of generations to an inconceivably remote past. So far as experience goes we can certainly say that living matter does not necessarily carry within itself the seeds of death. On the other hand, we can with equal certainty say that death apparently comes sooner or later to all chains of life; though it may be only that in chains of life, as in single lives, old age brings with it increased chances of death. Thus if we review the procession of animal life in the past history of the earth, we find that always they are the topmost branches that wither; that sets of animals highest in the scale at any geological period are constantly dying out and being replaced, not by their own descendants, but by the descendants of a younger and fresher stock. I take it that this does not necessarily imply the otherwise improbable conception that death is a necessary incident in life. Just as the older a weapon or an ornament is, invariably it is the more battered or rusted, because, however jealously guarded, increase of years means increase in number of chances of exposure to accident; so, however nicely adjusted to environment, however constitutionally plastic and ready for adaption to change, lapse of years must almost necessarily bring the one set of unsuitable conditions, the finally fatal moulding of the plastic constitution of an organic stock.

Exposure to accidents and somatic death are sufficient causes of death in the metazoa. It is not necessary to suppose that their cell protoplasm differs from the cell protoplasm of protozoa in having a limited duration of life.

Now let us turn to the two cases of practical immortality to see whether there be any special conditions.

First for the protozoa. It has been too easily assumed that in their case death follows from the gross accidents of life. The protozoa of the Crag, or of the Chalk, or of older beds, are not identical

with living forms. In these beds, as in the ooze of existing seas, the invariable presence of large numbers of uninjured shells seems to point to protoplasmic death as having at least a possible place in protozoa. But existing protozoa as other existing animals are the direct descendants of the protozoa of long ago. They have established a weighty claim to be considered conditionally immortal. They enjoy two special advantages. By living singly they stand or fall by their own luck; they are not tied to the accidents of other cells in a cell-commonwealth. Their easier contact with external media, and readier means of getting rid of waste products, or of accidentally-taken dangerous substances, makes them less subject to the intrinsic accidents associated with cell-life.

Germ cells, again, unlike the working cells of the body, lie comparatively dormant within the tissues until the time comes for the playing of their own part. Practically all the ova a woman produces are already formed in the newborn babe. They lie in the ovary, secluded from the wear and tear of cell-life, subject to few dangers, and as far from the vicissitudes of somatic life as possible. And, lastly, the invariable extrusion of at least one polar body from every ovum is a mysterious and unique condition; a condition which may be a special effort to remove from the cell bye-products the slow accumulation of which in tissue-cells ultimately causes disease or disturbance.

We know that all other cells do ultimately die, and that death of all kinds can be reduced to cell-death. The meaning of cell-death and the meaning of cell-life will be discovered together. But already we know something of the processes going on in living cells. In the first place there is decomposition. We cannot trace all the details, but we know that a number of substances are taken in, so to speak, at one end, and that a number emerge at the other. It is futile to say that the processes are not the processes of chemistry, as we do not know the constitutional formulæ of many of the substances which enter or emerge. But the sum of the activity is a slow feeding of substances of a high potential; a complicated flux of these within the cell; and an emergence of substances of a lower potential. Clearly there are a large number of ways in which such a process can be disarranged. A sudden stimulus may set off the liberation of energy so rapidly that there is none left to do the work of bringing in more food, and starting the chemical reactions. Some waste substance produced accidentally or usually in small quantities, and not regularly excreted, may slowly accumulate until it clogs the whole action. Even in inorganic chemistry, reactions are often apparently so capricious that we know they must be influenced by a multitude of apparently trifling conditions. Lastly, the introduction of some unwonted substance may arrest or supplant the normal reactions.

For the elucidation of all these, there is wanted much experimental organic chemistry. Above all, there is wanted a careful

comparison of the constitution with the effects of the different poisons which act on protoplasm. This is already leading towards a knowledge of the actual occurrences in a living cell, and it will no doubt get more and more clear that the events which cause protoplasmic death are simple interferences with the cell economy arising from internal or external accidents. As in bacteriology, the idea that the fatal influence of these parasitic organisms is due to a struggle between life and life, has been displaced by the idea that it is due to the direct action of a poison accidentally discharged by the parasites, so the causes of protoplasmic death will be attributed less and less to any inherent mortality in protoplasm. There seems no good evidence that protoplasm in suitable conditions, with sufficient food, should ever die, just as there is no reason that a flame, with a constant supply of fuel, and removed from violent draughts, should not burn from the beginning of time to the end of time. But the more complex flame of life is disturbed by many agencies, and these agencies cause protoplasmic death. In the metazoa, where each cell may suffer by the accidents of every cell, a new danger, the danger which is here called somatic death, is so urgent as to be fate rather than accident.

P. Chalmers Mitchell.

IV.

The Geology of the Central Himalaya.[1]

THE physical structure of the Himalaya must always be a subject of great interest to geologists, since our knowledge respecting the birth and growth of great mountain-systems is derived very largely from this region; and the imagination is stirred by learning that the main disturbance which brought the rocks to their present elevation took place after the deposition of the London Clay.

For more than fifty years information has been accumulating on the geology of the Himalaya, but no systematic survey had been attempted of the region now described, until Mr. Griesbach commenced work among the higher mountains in 1879.

We can understand how unintelligible the geology appeared at first, the slopes being covered with masses of *débris*, and the peaks being, most of them, inaccessible. Pushing on, however, from one tract to another, the author found many areas where the rocks were exposed, and in course of time the comparisons that could be made, enabled him to piece together the evidence and establish the sequence of rocks.

In the first part of his memoir, Mr. Griesbach deals with the Physical and Stratigraphical Features, and in the second he gives particulars of the sections, and a summary of results. The first part and the summary are naturally the more readable portions of the volume. Attention is drawn to the work in the same or neighbouring regions of other geologists, amongst whom we note R. Strachey, Stoliczka, H. H. Godwin-Austen, McMahon, Middlemiss, and Lydekker.

The highlands of Central Asia comprise the great Tibetan plateau, tablelands traversed by some mountains, and bordered on the north by the Kuen-Luen range, and on the south by the Himalaya. On these tablelands we find the head-waters of a few of the great rivers which intersect the mountain-belt of the Himalaya, and thence find their way to the sea across the plains of India. Thus, the Sutleej rises in the southern part of this plateau, in the region known as Hundés.

[1] *Memoirs of the Geological Survey of India*, vol. xxiii The Geology of the Central Himalayas, by C. L. Griesbach, C.I.E., Superintendent of the Geological Survey of India. 1891. Pp. i.–x., 232, and i.–xix. (index).

The Central Himalaya is divided into two portions:—(1) the Southern Range, which, including many of the higher mountains, may be said to begin with the mass of the Nampa Peaks on the north-west frontier of Nepal, and to end near the Spiti valley; and (2) the Northern Range, which stretches parallel to the other range, in some places separated by broad valleys 15,000 feet or more above sea level, and in others connected by ridges.

The Southern Range of the Himalaya averages 20,000 feet in elevation, and from 16,000 feet upwards there is perpetual snow. It is a true range though not a continuous ridge, being deeply furrowed by rivers that rise to the north. The rocks are mainly crystalline, being gneiss and schists with great granite intrusions.

The Northern Range is more continuous, but of lesser elevation, and comparatively free from snow. It consists of a system of great flexures of sedimentary rocks, ranging from Palæozoic to Tertiary, and their description forms the chief subject of the present memoir.

Some notion of the physical features of the area may be gathered from the foregoing statement; but it may be interesting to quote the following passage:—

"When travelling northwards from India to Tibet, through the deep valleys and over the great passes of the Himalayas, the eye becomes so accustomed to stupendous mountain masses, and a seemingly interminable succession of ranges and steep cliffs, that when finally reaching the last crest of the watershed, the view over the Hundés Basin which meets the eye, fairly takes one by surprise. At one's feet stretches, as far as the eye can see, an apparently level expanse of country, a level in which, at that distance, all lower ridges and inequalities seem to disappear. All of it is more or less of a brownish-green colour—bare patches of gravelly soil with streaks of bright green pasturage. Not a soul, nor living creature apparently, inhabits this immense waste, only rarely and at great intervals one may detect a solitary black tent belonging to a party of nomadic shepherds, whose flocks may be grazing in the sheltered depressions of this great expanse, where better pasturage is possibly found."

Beyond this great expanse of flat country may be seen the serrated elevations of the Kaílas range, sparingly covered with snow, although the heights average about 18,000 feet.

The structure of the Himalaya is broadly that of a succession of inverted flexures, leaning over towards the south-west. Two great anticlinals, formed of immense flexures of older crystalline formations, can be distinguished. Fossiliferous stratified rocks are preserved in the synclinals, but faulting and the intrusion of igneous rocks have modified the normal order in places; for the inverted flexures are often pushed one over the other along planes of dislocation, that form oblique faults of great magnitude, and produce the "scale-structure" so common in most areas of folding. The Southern Range is formed perhaps of the larger of these main anticlinals, and there is only left what might be taken to be a conformable series of metamorphic strata. North of this great inverted flexure there follows a huge

synclinal trough in which the highly contorted and crushed sequence of sedimentary rocks—great thicknesses of Palæozoic, Mesozoic, and even Tertiary strata have been upraised to stupendous altitudes. Later Tertiary and Recent deposits have been laid down in areas between the great lines of parallel flexure, and these help to form the great tablelands of Tibet and the Gobi.

Most of the great lines of flexure must have existed in Palæozoic times, "yet the great lateral compressions, which pushed up the enormous masses of the Central Asian plateau, with its fringing rims of mountains, were evidently formed after the Miocene beds were deposited, for we see the latter contorted and crushed, under a cover of almost horizontal young Tertiary deposits, which so far have suffered little from the slow action of folding, which it may be assumed is still going on."

The oldest rocks consist of granitic gneiss, regarded as metamorphic granite; they are apparently overlaid by a schistose series, termed the "Vaikrita system" (from a Sanskrit word indicating "metamorphism"). The rocks of this system comprise mica-schists, talc-schists, and thin bands of gneiss; and they are regarded as of sedimentary origin. They seem to pass gradually into a newer series of quartzites, phyllites, and slates, with coarse conglomerates, grouped under the name "Haimanta system."

This system (taking its name from a Sanskrit word signifying "snow-covered") appears to be constant in lithological aspect, and to have a thickness of from 3,000 to 4,000 feet; but the beds are extensively crushed and folded. The rocks underlie conformably Lower Silurian strata, but as they may comprise not only the Cambrian but still older groups, it was thought best to apply a distinct name, even though it be a provisional one.[2] The name itself is meant to indicate the fact that the beds, with few exceptions, lie within the area of perpetual snow. Hence they cannot reveal their history very readily to the geological enquirer. The Haimanta system has yielded only a doubtful Crinoid, a doubtful Bivalve, and an undetermined species of *Bellerophon*.

Conformably above the Haimanta system come beds grouped as Lower and Upper Silurian. Limestone is the predominant rock of the Lower Silurian, and quartzite of the Upper. They follow on without break, passing gradually one into the other by alternating beds. The Lower Silurian rocks are estimated to have a thickness of from 200 to 300 feet only. The fossils, which include Encrinites and Corals, await description, but the fauna of the "Coral-limestone" is said to correspond with that of the Lower Silurian of Europe. The Upper Silurian strata attain a thickness of 1,000 to 1,200 feet, but their fossils, which include *Orthis* and other Brachio-

[2] It should be mentioned that somewhat different names were previously adopted by Mr. R. Lydekker for the great groups of rocks in the adjoining area of Kashmir, &c. *See Mem. Geol. Surv. India*, vol. xxii, 1883.

pods, as well as Corals and Encrinites, also await determination. We must therefore suspend judgment concerning the European equivalents of these strata, and the same remarks apply to succeeding formations of Coral-limestone, Crinoid-limestone, white quartzite, and limestone, that are said to rest conformably on the older strata, and to represent formations from the Lower Devonian to the Upper Carboniferous. The limestones are variable in thickness, and the entire series varies from 1,450 to 2,500 feet. In the rocks grouped as Devonian there are *Orthoceras* and *Productus semireticulatus*.

The occurrence of *Athyris royssii* and of *Productus* in the highest band of limestone affords the only particular evidence stated of its Carboniferous age. The distribution of the highest Carboniferous beds affords evidence in overlaps of physical changes of a widespread nature, and the author remarks that perhaps no other formation in Asia has a wider area of distribution. He lays perhaps too much stress on the lithological change between the beds grouped as Carboniferous and those regarded as Permian: even abrupt changes from quartzites associated with limestone to "crumbling black shales" afford no criterion of *great* physical changes as proved by the record of our British Carboniferous rocks. The fact, too, that the succeeding beds in the Himalaya consist of *Productus*-beds of Permian age (yielding a form near to *Productus latirostratus*) tends to show, as the author admits, that the break could have been of no great extent as regards time. The evidence of a break lies chiefly in the uptilting and partial erosion of the Upper Carboniferous beds, so that successive strata are overlapped in an unconformable manner by Permian rocks. Thus the Carboniferous beds, which are purely marine, are followed by a great system of strata that are in a large measure littoral; and the author considers that the wrinkling process, which resulted in the elevation of the Himalaya, was partially brought about in Permian times, so that the main outlines of the great range, which had previously been determined, were then more distinctly foreshadowed.

The older rocks are regarded as forming a continuous series; and now, after the unconformity just mentioned, we find the records of another sequence from the Permian shales to Liassic limestones. The Permian beds exhibit a thickness only of 120 to 250 feet, but they have suffered much from crushing and folding, and the more rigid Triassic rocks have been thrust over them on to the Carboniferous formations. Passage-beds occur between the Permian and Trias containing a fauna akin to both, and noted as the *Otoceras* horizon, or zone of *O. woodwardi*. The Triassic rocks attain a thickness of from 1,500 to 4,000 feet, and consist mainly of limestones and shales. They are divided into Bunter, Muschelkalk, and Keuper, and are characterised by fossil zones, of which Brachiopods are the chief indices. Casts of *Corbis* occur in the Upper Trias, and in some of the beds *Ceratites* and *Arcestes* are preserved. The author remarks that a great likeness exists between the Trias and Rhætic

beds of the Himalaya and the Alpine regions. As there is a complete passage upwards from the Permian, there are probably represented some strata not found in localities where there is evidence of a break between Permian and Bunter.

The Rhætic beds comprise a great thickness of dolomites and limestones, characterised by *Lithodendron* and *Megalodon* in the lower beds, and with Crinoid-limestones and Brachiopods of Upper Kössen facies in the higher portion. Strata regarded as Passage-beds between the Rhætic formation and the Lias, yield *Pecten valoniensis*, *Cardium rhæticum*, some Liassic *Limas*, and other fossils. The thickness of the Rhætic formation is upwards of 2,000 feet; and the beds are surmounted by black shales and dark earthy limestones, with oolitic structure altogether about 100 feet thick. These beds are assigned to the Lias; and they contain *Ammonites annulatus*, *Belemnites*, *Gervillia*, &c.

Some confusion is caused by the employment of the term Jurassic in the sense in which Oolitic series is used in this country, so as to exclude the Lias; a grouping that is not according to general custom.

The beds that succeed the Lias are the Spiti shales, so named by Stoliczka; they attain a thickness of 1,300 feet, and embrace the entire series of deposits between the Liassic strata and the grey and greenish sandstones that are assigned to the Lower Cretaceous system. From these Spiti beds, which are, perhaps, the best known and most widely-distributed of the Himalayan formations, numerous fossils have been collected, not only by geologists and travellers, but by natives of Tibet and India. The author tells us that from ancient times a trade in the Ammonites has existed; great quantities of them being brought every year to India, chiefly to the holy places of Hindu pilgrimage, to be sold as relics to the worshippers.

Although no unconformity can be traced in the stratification of the Lias and the Spiti shales, there is an alteration in lithological character, accompanied by a sudden change in fauna. There is, apparently, an absence of the Lower Oolitic fauna (Lower Jurassic of the author), but the lowest portion of the Spiti formation has yielded no fossils. Higher up there are found Ammonites of Oxfordian type; and the upper Spiti shales are of Tithonian age.

The Spiti shales are much affected by the grand flexures; they have frequently been squeezed into the narrowest folds, while the rigid Cretaceous strata have been pushed over them.

The Spiti beds pass gradually into strata that are grouped as Lower Cretaceous or Neocomian, and the same succession is found to occur through the northern part of Persia and along the shores of the Caspian. These "Lower Cretaceous" beds, 1,200 to 1,500 feet thick, consist mostly of sandstones and shales. They yield a few crushed *Belemnites*, and it is just possible they may be of Upper Tithonian rather than of Lower Cretaceous age. They are overlaid by

white or grey limestone, containing *Rudistes*, *Inoceramus*, and other marine Upper Cretaceous fossils. These upper beds are widespread, and although in the Himalaya and immediately adjoining areas there is no visible unconformity with the beds below, yet in areas beyond there is evidence of decided overlap accompanied by unconformity. Regarding the Tertiary strata, it is found that there is a marine Nummulitic formation, apparently conformable to the Upper Cretaceous, much disturbed and altered by masses of igneous rock. This formation is overlaid unconformably by sandstone, probably of Miocene age, and resembling some lower Siwalik Beds, and above these rest irregularly the younger Tertiary strata of Hundés. These younger Tertiary strata form a striking feature in the geology, as they are found at elevations of 14,000 to 16,000 feet, and preserve an almost horizontal character. They are of great thickness, and cover all the older Tertiary strata, and perhaps the Secondary rocks which form the trough of Hundés. Mr. R. Lydekker has shown that these beds are probably younger than the Siwaliks, almost certainly not older than Upper Pliocene, and very likely of Pleistocene age. The position of the strata and their structural relations forbid the notion that any great change in physical conditions has taken place since their deposition. They appear to be of lacustrine formation.

The work is admirably illustrated. There are two large geological maps, six plates of sections, seven panoramic views and profiles, taken with the aid of the camera lucida, and sixteen photographic pictures reproduced by the process of heliogravure. These pictures are exceedingly well executed, and they show various forms of mountain scenery, glaciers, rock-structure, and contortions.

H. B. Woodward.

[Since the above work was published, we learn that Mr. Griesbach forwarded to Vienna many fossils of the Mesozoic age, which he had collected during his geological explorations in the Central Himalaya. These fossils were examined by a specialist in Alpine palæontology, and found to correspond in a remarkable way with those of the alpine strata. Such a fact has so interested the authorities of the Royal Imperial Academy of Science, Vienna, that they have, with the co-operation of the Government of India, determined to send out an exploring party to the Central Himalaya to compare their geological features with those of the Eastern Alps. Dr. Carl Diener, Lecturer in Geology at the University of Vienna, has been chosen leader of the party.]

V.

The "Albatross" Expedition.[1]

THE brief sketch of the work accomplished by the U.S. Fish Commission steamer "Albatross" last spring in the Pacific Ocean, now given to us, is most interesting, and stimulates the expectation with which we look for more detailed results in the near future. The cruise lasted from February 22 till April 23, and may be said to have consisted of four distinct stages (1) from Panama to Cocos Island and Malpelo Island and back to Panama, (2) from Panama to Galera Point and the Galapagos Islands, (3) from the Galapagos Islands to Acapulco, (4) from Acapulco to Guaymas in the Gulf of California. The work accomplished comprised dredging and townetting, both on the surface and at various depths, temperature and current observations, and some investigations into the physical and biological conditions of the Galapagos Islands. We shall endeavour to summarise as briefly as possible the results of these various lines of research.

The contour of the sea-bed on the Pacific side of the Isthmus of Panama is very different from that observed on the Atlantic shore. On the latter we have great submarine banks extending from Yucatan, Honduras, and Venezuela, with deep basins in the Gulf of Mexico and the Caribbean Sea. On the western side there is a deep trench more than 2,000 fathoms deep, widening as it approaches the land, and maintaining this depth till it is not far away (often within 100 miles) from the shore. The Galapagos Islands form the summit of a large bank, shaped like an inverted comma, the 1,500-fathom line stretching out in a point which takes in Cocos Island, and almost meets the line of similar depth from the shore just off Cape Mariato. Comparatively few soundings have been made among the Galapagos Islands themselves, and no detailed contours are given on the chart of this group.

The bottom deposit was most often characteristic Globigerina ooze, and on one occasion the trawl was completely filled with a species of *Rhabdammina*. At two places near Cape Mariato patches of modern greensand were discovered. It is important to notice that

[1] General Sketch of the Expedition of the "Albatross," from February to May, 1891. By Alexander Agassiz. *Bull. Mus. Comp. Zool.*, xxiii., pp. 1–89, 22 maps and plates, 1892.

even at the greatest depths, and at the greatest distance reached from the shore, distinct traces of terrigenous material were found in the deposits. Sometimes this was a fine sticky mud, at other times there were large quantities of decayed vegetable matter. In fact, during certain parts of the cruise "there was hardly a haul taken which did not supply a large quantity of water-logged wood, and more or less fresh twigs, leaves, seeds, and fruits in all possible stages of decomposition."

The temperature observations are given in considerable detail, and as the region investigated is traversed by some half-dozen different currents, it would be impossible to give an adequate summary of the results without the aid of maps and diagrams. The most interesting point to notice is that no sufficient evidence was obtained of the updraught of cold water along the western coast of equatorial America, to which Dana and others have attributed the absence of coral reefs in those regions. Agassiz rather believes this to be due to the amount of silt brought down from the land.

The next section of the Report is the longest, and one of the most interesting. It is devoted to a discussion of the bathymetrical distribution of pelagic life. Professor Agassiz first reviews the older inquiries in this direction, and shows that with scarcely an exception they are all vitiated by defects in the apparatus, and that they are quite inadequate to demonstrate the existence of a populous fauna at intermediate depths, as stated by Professor Haeckel in his "Plankton Studien." In fact, he enters upon quite a sharp passage of arms with the philosopher of Jena, so sharp, indeed, that it must be read to be appreciated.

Passing by this controversy to consider the facts obtained by the "Albatross," we may remark that the cruise will be memorable for the use which was made of a really practical opening and closing net, "which worked to perfection at 200, 300, 400, and 1,000 fathoms, and had the great advantage of bringing up anything it might find on its way up above the level at which it was towed." This apparatus was due to the ingenuity of Captain Tanner. The net is conical in form, and is suspended from a ring between two ropes, which are weighted at the bottom. About one-fourth up the net two strings encircle it, passing through loops; to the ends of these strings are attached weights which, when allowed to fall, draw them tight, and constrict off the lower portion of the net. They are, however, held up by a crank, and can be liberated when required by a sliding weight or messenger, which is allowed to run down the line. The difficulty of opening the net under water is obviated by the simple expedient of letting it down with the pointed hinder end first, the pressure of the water being sufficient to keep it pressed tightly together like a folded umbrella. Arrived at the desired depth, the vessel is put in motion, and rope enough is paid out to keep the net moving horizontally, this being judged of by the slant of the rope.

After towing for, say, twenty minutes, the net is again brought to the vertical, and the messenger sent down to close it; after which, it is brought up to the surface, and the contents of the upper and lower portions of the net examined separately.

This net was used on several occasions in water of considerable depth, and undoubtedly failed to produce evidence of a mid-water fauna of any variety or abundance. The number of unexceptionable trials at depths of 400 fathoms and upwards was, however, too small to justify any far-reaching conclusions.

The Galapagos Islands occupy the next section of the Report. This group consists of some nine or ten moderate-sized islands with several small ones. With the exception of a small colony maintained on Chatham Island, they are, as when first discovered, uninhabited. Their vegetation presents three distinct belts or zones. The first, in close proximity to the shore, is characterised by stunted salt-loving plants; among them are a Verbena and an Acacia with small leaves and inconspicuous flowers, and a large tree, the Palo Santo. Where the lava prevents the growth of anything else, we still have an *Opuntia* and a *Cereus*. As we ascend the latter disappears, as also the Verbena, while the other two increase in size. The second belt is found where the lava has become decomposed into soft reddish earth, on which grow woods of small trees resembling the Andean *Polylepis*. From this there is a gradual passage to the third region, which is bare of trees and covered with coarse grass. The bulk of the vegetation seems to have been derived, as might be expected, from Central America. The amount of decaying vegetable matter brought up by the trawl has already been alluded to, and, as Mr. Agassiz remarks, "certainly offers a very practical object-lesson regarding the manner in which" these islands must have received their vegetable products. Autotype reproductions of a number of photographs taken on the expedition show very clearly the general features of the vegetation.

The land fauna does not seem to be abundant: the giant tortoises, once so numerous that single vessels have been known to carry off seven hundred at once, have now disappeared. The only tortoise obtained by the "Albatross" was got on Duncan Island: it was sent alive to Washington. A bat is known, but appears to have been on these islands only for the last six or seven years. The insects are not numerous; caterpillars were very abundant, but only of few species. Quite a remarkable number of domestic animals have now become feral.

A great deal of coral rock, in isolated patches, is found about these islands, and the so-called "sandstone" proved, on examination, to be a slightly oolitic coral rock. There are, however, no true reefs either on these islands or anywhere in the Panamaic region.

The author discusses the recently expressed views of Dr. Baur on the origin of the Galapagos Islands, and comes to the conclusion that

they are distinctively and entirely volcanic, and have never been connected with the mainland of South America, from which they are now separated by a wide plain nearly 2,000 fathoms in depth.

The observations upon the deep-sea fauna are most important and interesting. The "Challenger" Expedition traversed the centre of the Pacific Ocean, along a line extending through the Sandwich Islands to Tahiti. The "Albatross" examined a wedge-shaped portion of the wide expanse lying to the eastward of this line. One of the most striking facts elicited was that there exists in this area a "considerable fauna, which finds its parallel in the West Indies, and recalls later Cretaceous times, when the Caribbean Sea was practically a bay of the Pacific."

Some differences may be noted—in the Pacific area the scarcity of Ophiurids contrasts strongly with their abundance in the Atlantic, "where it often seems as if ophiurans had been the first and only objects created."

The general absence of Crinoids and *Comatulæ* was noticeable, although one very interesting new genus (*Calamocrinus*) was obtained from 782 fathoms off Mariato Point, an elaborately illustrated description of which has already been published.[2]

Among Bryozoa further examples of the interesting genus *Kinetoskias*, first obtained by Danielssen off the Norwegian coast, and subsequently by the "Challenger" at several localities in the Atlantic Ocean, have been dredged.

Several Acalephs (*Atolla*, *Periphylla*), certain *Beryx*-like fishes, Scopelids, Stomias, Melamphäes, must now be excluded from the abysmal fauna in which they have hitherto been reckoned, as they have been captured by the Tanner net in depths less than 300 fathoms.

A brief comparison is presented to us of the relations existing between the Echinoid fauna of the two sides of the Isthmus of Panama, with a promise of a more detailed statement hereafter. Only one genus of urchins (*Salenia*) has been discovered in the deep waters of the Pacific side which has not previously been found on the Atlantic side. The interesting Echinothurids appeared, but these, as is well known, have an extensive Pacific as well as Atlantic distribution.

An important feature of the work on board the "Albatross" was the number of coloured sketches prepared by the artist, Mr. Westergren. Some of these have reached this country, and will be most useful in supplementing descriptions of specimens preserved in spirit.

A considerable amount of information is given in the present Report regarding the colours of various deep-sea types; these are very variable, but there seems to be a preponderance of violet. In many cases there seems to be a great lack of adaptation to the

[1] See NATURAL SCIENCE, No. 2, p. 93.

greenish-grey mud in which the animals live. Other creatures, however, furnish just as conspicuous examples of mimetic resemblance, witness a small crab, whose back resembles a bit of muddy area covered by corals, while its huge white arm suggests a fragment of an Isis-like Gorgonian.

The detailed reports upon the various groups of animals will be written by different specialists, and the whole work when complete will furnish a most welcome addition to our knowledge of the Pacific Ocean. The "Challenger" gave a rough sketch of the conditions of deep-sea life; such expeditions as that of the "Albatross" help to fill in the details of the picture.

W. E. Hoyle.

VI.

Professor Lloyd Morgan on the "Grammar of Science."

PROFESSOR PEARSON'S "Grammar of Science" occupies an almost unique position among English books which treat of the nature, scope, and limitations of Scientific Method. Whereas most other writers have treated the subject descriptively and historically, as did Whewell, Herschel, and Baden-Powell, or have busied themselves with the logical processes employed, as did Jevons, Professor Pearson has tried to bring about a more accurate use of scientific terminology by a criticism of the fundamental concepts of modern science. In this criticism he has endeavoured to co-ordinate the concepts of the sciences with those of the theory of knowledge.

Professor Lloyd Morgan is fully alive to this feature of Professor Pearson's work; but to a careful reader of the latter it must appear that in many respects the reviewer has either misunderstood the doctrines which he criticises, or has failed to establish a consistent ground for his rejection of them. Professor Pearson adopts a philosophical position which may be fairly described as thoroughly idealistic. In this respect, indeed, he is at one with most writers of text-books on Physics. Only with a difference. The text-book writers are generally as idealistic as Hume or Mill in their opening paragraphs, but they fail to show their faith by their works in the chapters which follow. "The Grammar of Science," however, gives in a clear but concise manner the author's own grounds for the position he has taken up, and, what is even more to the point, the actual bearing of this theory of human experience upon the thought and language of science is illustrated by abundant example as well as by precept.

Thus, even if the book had no other value, it would at least be of service to abate the assurance of those writers who suppose that though Idealism sounds well in the lecture room, it is nevertheless doomed to inevitable scientific bankruptcy in the fields of practical research.

Now the odd thing about Professor Lloyd Morgan's position is that, although he is himself, as he tells us, fully impressed with the general truth and value of these idealistic results in their proper place, he yet seems to think that their proper place is on the shelf.

In truth, Professor Lloyd Morgan is too good a psychologist wholly to reject them; but he has not quite the courage of his convictions, and he hesitates to trust his full weight upon a bridge the soundness of which he professes not to doubt, but which still, to his mind, seems too shadowy to be solid. He fears that confusion will result. Professor Pearson shows that the danger of obtruding idealistic results, in themselves true and valuable, into the conclusions of the several sciences is an imaginary danger, but that harm has already come about through the neglect of them.

We may grant that the analysis of the nature of experience is on one plane of investigation, and molecular physics on another, but by shutting our eyes to the relation between them we (even now) run some risk of framing a theory of Physics the language of which may prove to be incompatible with our theory of Thought.

In his discussion of the various problems raised by Professor Pearson's book, Professor Lloyd Morgan begins with that of the "external object" and the nature of "reality."

Here, at any rate, by his own confession in "Animal Life and Intelligence," it would seem that the reviewer is in substantial agreement with his author; but he is strangely diffident. Often he seems to say, "I know that this is all true, but let us make believe that it is not."

Concerning "object and eject," Professor Lloyd Morgan, by a strange oversight, has somewhat missed the point in dispute. The question is not, as he supposes, as to whether or no some physical link is necessary to enable one mind to appreciate the thoughts of another. It is whether or no, by any imaginable means, the thoughts of A. could become something more than matter of inference, and in short be directly an object in the consciousness of B.

Passing over the chapter on "Scientific Law" (perhaps the most practically important in the book), and those on Cause and Effect, and on Probability, Professor Lloyd Morgan deals next with the subject of Space and Time. There can be no doubt that the doctrine as to the nature of space and time which is associated with the name of Kant is the most important positive addition which has been made to the idealistic theory of human knowledge since the days of Bishop Berkeley; and it is one of the noteworthy features of the "Grammar of Science" that here, perhaps for the first time in the writings of English men of science, we find at once a full recognition of the general truth of Kant's doctrine, a short but clear exposition of it, and, in subsequent chapters, an explicit application of it to the problems of Matter and Motion.

Now, on this view of Space and Time there is much to be said. Supposing it to hold its ground as a truth of philosophy, and to acquire the place which it deserves in any complete account of human psychology, there is still room for debate as to the best mode of stating it, both for general purposes, and especially for the more

technical purposes of actual physical science. Here, if anywhere, comment and criticism were to be hoped for. But our hopes are doomed to disappointment. All we get from Professor Lloyd Morgan is the following :—

"The question is whether, given certain sense impressions which suggest the percept clock, there are any objective relationships which I can describe by using the adjectives distant, extended, and successive."

Now it is almost incredible that one who has carefully read Professor Pearson's earlier chapters, and who may also fairly be supposed to be not wholly unacquainted with the writings of Kant, should really for a moment suppose that *this* is the question. The real question, of course, is not whether there are any relations of objects which may be so described, but what it is in truth which we are stating when we make use of these terms of relationship. The adjective "objective" (which Professor Pearson has happily avoided in his chapter on "Space and Time"), it is almost needless to say, settles nothing. But to Professor Lloyd Morgan the blessed words "objective," "frankly objective," and "objective existence" (without a hint of further definition), seem to be conclusive shibboleths as to the great problems upon which Berkeley, Hume, and Kant spent the best working hours of their lives.

What does Professor Lloyd Morgan mean by "objective relationships"? Does he mean that they have an existence, as such, in the absence of the percipient mind? This he expressly denies. Does he mean a relationship between objects? No one has denied objective relationships in this sense, and Professor Pearson on his part declares, "Space has no existence if objects are withdrawn."

It would seem that what Professor Lloyd Morgan means by "objectivity" is "empirical reality." In other words, just as Dr. Johnson supposed that Berkeley's views on Matter must lead men to run their heads against brick walls, so Professor Lloyd Morgan has forgotten that human experience is none the less real, either as to weight or measure, even if it can be shown that the terms which express that experience owe all their significance to their inseparable association with the nature of our perceptive faculty.

Physics, no doubt, is wise, as Professor Lloyd Morgan remarks, in dealing with space and time in frankly objective terms. This Professor Pearson has not disputed, at least in any intelligible sense of that phrase. What he has disputed is the conventional, or uncritical view, which takes space and time for "things," instead of taking them for an order of things, and which supposes that bodies are capable of being philosophically dealt with as if they had a "reality" not conditioned by the nature of our faculties for knowing them.

Moreover, whether true or not, all this is to Professor Lloyd

Morgan out of place in Physics and Biology. And yet the question *has* practical bearings as will be evident to any reader who carefully studies the chapter on Probability, or the concluding section of the chapter on Space and Time in the "Grammar."

Happily, there can be no two opinions about the importance of clearly distinguishing between Perceptions and Conceptions, and by the aid of an analogy Professor Lloyd Morgan very clearly illustrates the process by which, in many cases, scientific conceptions are reached. But for whose benefit has Professor Lloyd Morgan written the sentence which follows, in which we are told that "the perceptual conception (*sic*) as such has neither a continuous boundary as idealised in geometry, nor a molecular constitution as idealised in physics"? Professor Lloyd Morgan must have forgotten a passage in Mill's "Logic," which tells the reader that "although I cannot dig the ground unless I have the idea of the ground, still it would be ridiculous to describe digging as putting one idea into another."

The Geometry of Motion is left to the Physicists to criticise.[1] This chapter, together with those on Matter and the Laws of Motion, contain a full exposition of what the author means by mechanism in Nature, but the reviewer can say nothing more than that it appears to him more simple and more scientifically accurate to regard matter and motion as frankly objective and ("perceptually") external—a dictum which does not call for further comment.

Some remarks in Professor Pearson's chapter on Life have given Professor Lloyd Morgan an opportunity to defend his well-known views upon the origin of consciousness. The reviewer holds that consciousness has been developed from something which he calls Metakinesis, which something is said to be of the same nature as consciousness, but to have not yet risen to the level of consciousness.

Professor Pearson thinks that we might as well talk of life as having been developed from something "which-is-not-yet-life-but-which-may-develop-into-life."

There seems but small chance that the author of the "Grammar of Science" will be led to believe in Metakinesis by Professor Lloyd Morgan's new defence of it, for, judged by the principles of that work, the scientific legitimacy of the theory is more than doubtful.

It is one of the canons of Professor Pearson's scientific method that a Conception, in order to be valid, must be derivable from Perception; also, it is essential to the validity of the Conception that it should be self-consistent. But what phase of our perceptual experience is described by Metakinesis? and what can we say of its self-consistency when we are told in one place that it is phenomenal, and in another that it is arrived at by carrying to a conceptual limit that which is suggested in the sphere of the perceptual?

[1] *See* review in *Nature*, June 2, 1892, and correspondence in *Nature*, June 30, July 7, July 14.

Professor Lloyd Morgan has himself done much admirable work in a difficult field of descriptive science; but it may be doubted whether his criticism of the "Grammar of Science" will (at any rate to those who have read the book with attention) enhance the well-deserved reputation which the first eleven chapters of "Animal Life and Intelligence" have won for him.

He seems to be serving two masters. If certain results *are* true in psychology, we cannot afford to despise them when we come to deal with Physics and Biology. Still less can we neglect them if we must needs discuss the origin of consciousness.

R. J. Ryle.

VII.

Technical Education in Surrey.

IT requires no genius to see that education is as apparently endless as the civilisation with which it is indissolubly connected, and progress in the direction in which the most important nations of the world are developing is dependent very largely on the introduction of mind into labour—the more complete guidance of the hand by a cultivated brain. In England we have been, in some degree, slow to recognise the value of scientific training, and our Continental neighbours are, at the present moment, ahead of us, in the fact that they possess a combination of educated employer and employé. Prosperity in the industries of this country is due primarily to scientific knowledge, and for complete success in its application it is necessary that the efficiency of the worker should not be reduced by want of intelligence.

The Government Grant to the County Councils has rendered it possible for something to be done, not only by way of raising the general level of intelligence, and so indirectly altering the interests of life among the masses, but at the same time in imparting information which will have a practical bearing on definite occupations.

During the last year, nearly every County Council in England has been intensely active in carrying out its Scheme of Technical Education, and the work, as a whole, is undoubtedly a step forward which augurs well for the future. The want of precedent as a guide is sufficient to justify the expectation of failure in some cases, and the gigantic scale of the work, together with its diversity, makes it of great difficulty, even so far as its mere organisation is concerned.

At the outset, the difficulties to be met with seemed almost insuperable, but owing to the energy of the organising secretaries, and the interest manifested locally, classes were held, lectures delivered, and work done in many branches of science at hundreds of towns and villages during the winter months. The great aim of the work has been to combine practice with the science underlying it, and the meaning of Technical Education in its legal aspect hints that the latter should have due prominence.

The most interesting, and at the same time most difficult part of the work is that which is concerned with rural districts. It is in these places that the greatest amount of ignorance prevails, coupled

with a natural apathy for mental exercise, but in power of perception, and even enthusiasm for scientific teaching, our experience with the Surrey labourer leads us to conclude that the stupid countryman is to some extent a myth.

We are far from a universally enlightened state among the rural population, but it only needs a continuous application of well-organised teaching of a carefully-selected kind to make Technical Education among village communities a real power in the country, which will not only lead to a rise in the tone of society, but to an improved physical condition of the people.

The causes of failure, which has been small, are not far to seek. Want of adequate advertisement and explanation of the design is responsible for a good deal. At the commencement last September few districts had any conception of the nature or object of the work, and various schemes were put in force without suitable or sufficient warning. With a people who are inclined to move slowly and consider matters, nothing is more injurious to success than a sudden call to arms—chaos results, and a bad beginning, instead of leading to a good end, leads to total extinction.

Fortunately little injury of this kind has happened, but traces were bound to occur here and there. One of the first requirements in the accomplishment of satisfactory progress in these country districts is the formation of a representative local committee which will endeavour to rouse interest in the general scheme. Too frequently has the work devolved upon single individuals whose efforts, though in many cases excellent, are not likely to have the character of permanency.

It was necessary at the commencement to make some already-existing committee a basis for operations. These, however, were merely temporary, and in over 120 towns and villages in Surrey properly constituted committees exist, and work has been carried out.

The peripatetic system has been resorted to, and a considerable variety of subjects have been taught—health lectures, nursing, dairy work, drawing and woodwork, together with different branches of science—the average attendance at each of the science classes and lectures being between thirty and forty. The writer has been chiefly occupied with the teaching of Botany for agri- and horticulturists, and it is with the experience gained and the conclusions arrived at that this article is concerned.

The practical value of the science which deals with plants is obvious, when we consider that the animal creation depends for its existence on the vegetable kingdom, and no system of education for the population in rural districts can be considered complete without giving a large share of attention to scientific teaching in regard to the life processes and structure of plants.

No science, however, in Great Britain is in a more purely

academic position than the science of Botany. Perhaps this is true to a considerable extent in other countries as well, but the bridge between the science as taught and its practical application, is of such a nature that we may almost say it is non-existent at home. In Germany, France, and even Norway and Sweden, "practice with science"—the motto of our Royal Agricultural Society—is more of a reality than a name. No better evidence of the danger of continuous specialisation—which is very often heedless of the claims of the practical man—is seen than that which confronts us when we compare English scientific botanical literature with the books on which a gardener or farmer has to rely for his information. In the former, pure science for its own sake—not to be heedlessly condemned—exists in a state of supreme ignorance of its co-partner on the side of practice. The illustrations of structure and application of physiological principle in the modern text-book, savour of curiosity merely, or are worn threadbare by being compelled to do duty on all possible occasions. The botanist has gone on and on into minute and endless paths, far ahead of ordinary life, and is lost to the cultivator of field or garden. The gardener, on the other hand, is primarily concerned with the practice of his art, and has, by force of circumstances, specialised in his own peculiar manner, and become almost a mere machine. He has found it difficult to account for success or failure, and has been compelled to register methods minutely, and the result is he is lost to the botanist in blind and often meaningless adherence to custom. Scientific explanation or statement of principle in his books is missing, or, if given, is often worse than if it had not been present at all. The scientific man and the practical man in botany, so far as England is concerned, have left each other almost completely; whatever the one possesses belonging to the other is ancient and valueless. An exchange of books, under such circumstances, can easily be imagined. The gardener, in beginning to study botany, finds himself in a foreign land, and the botanist is not much better off when he attempts to improve his acquaintance with horticulture.

Up to the present time, in this country few attempts have been made to remedy this state of things, but abroad the step between the two is firmly laid, and new discoveries are adapted to everyday requirements several years before we avail ourselves of them.

It is essential for the proper development of scientific work in its best form that it should be kept apart from practical considerations to some extent, but isolation may take place between the advanced specialist and what should be his colleague, and the gap between them becomes so great that they refuse to recognise each other. This is the condition of affairs between Horticulture and Agriculture, and one of their chief corner-stones—the science of Botany.

It is time we made an attempt at a remedy. Up to a certain point the requirements of the farmer and gardener, so far as this subject

is concerned, are alike, and it has been our lot to lecture on "Plant Life" with a view to explain the nature and organisation of a plant, so that reckless treatment of what is too slightly recognised as a living structure should be avoided.

The nature of a seed, its germination—the structure and use of root, stems and leaves, and their treatment—the food materials and their method of introduction into the plant—movement of "sap," water, and reserve materials, all these points and others were taken and scientifically explained in a manner suited to the audience without sacrificing accuracy. At some of the most successful centres the leading employers of labour, the parson, small tenant farmers, gardeners, and labourers made up the gathering. It is a mistake too commonly, perhaps unconsciously, committed, to estimate the success or failure by the number of mere labourers present, but although an important item, there are others whose rank and elevation are higher in whom, perhaps, the first fruits of this scheme will show itself.

In connection with the weekly lectures, questions were given and answers sent in from practically every centre. At one village five or six gardeners sent in papers for the whole course, and passed the examination at the end with credit. It is, indeed, an achievement to have succeeded in obtaining weekly papers from men to whom writing and expressing themselves on paper is a new and troublesome self-denying operation.

Sequence in subjects is aimed at, and the teaching is made more complete and lasting by keeping a correct balance between theory and practice. Lectures on "Plant Life" have been, or will be, supplemented by courses of horticulture by gardening experts. In this department most successful work has been accomplished, and it is only by this combination that progress will be made. It has been stated that horticulture cannot be taught by means of lectures, but it is very doubtful if the science connected with an art or industry can be taught in any other way. The practical results from such teaching last winter are already becoming manifest by the increased yield in small gardens we have seen. Only a few days ago a schoolmaster showed us the effect on his currant bushes which some simple lessons on pruning had had; and after simple scientific lectures on the structure and mechanism of roots or leaves, almost invariably some of the audience have made accurate deductions from the principles involved, and have explained failures in some of their previous experience. It has often been encouraging to learn of the existence of this quiet thought, and have evidence of the living interest taken in the work.

Labourers in possession of a cottage garden are those who are open to the reception of teaching, but for the country gardener, who is often most ignorant of the things he deals with daily, little attraction seems to be possessed by this effort to improve his position.

Few men like to be told of their faults, but there are, indeed, many excellent exceptions to be met with among the younger men in the gardening class. It is astonishing how little real interest in his work is manifested by the average so-called gardener, but we have had considerable experience among them of ability and earnest desire to benefit by the opportunities placed before them. This, perhaps, is especially so where their employers are not wholly ignorant of the principles underlying sound cultivation.

The most promising work is seen when the gentry of the district are interested in the movement, and estimable examples of self-sacrifice and genuine regard for the education of the labouring classes have been met with in several parts of Surrey. A clergyman, in one instance, went to the trouble and expense of hiring a "Technical Education Van," to convey ten or fifteen people to the lectures from a neighbouring village during the whole winter. Aid in many ways has been given for carrying out the scheme, and supplementary teaching has been voluntarily undertaken.

Taken as a whole, the centres whose appreciation stands highest, and where the interest has been well maintained throughout, are comparatively small villages. There the organisation is more easily managed, and there is less likelihood of complication arising from attempts to do too much.

The least successful places are those which are neither towns nor villages, but hybrids between the two. Want of unanimity of sentiment is prevalent among the population, and discontent sometimes arising into a species of party feeling is a block to progress.

In such districts failures are met with which must be attributed, however, to attempts at too diversified a programme. One of the greatest dangers to be guarded against is that which arises from burdening a district with excessive educational opportunities. It is impossible for anyone engaged in manual labour all day to keep up healthy mental exercise after his daily work, and yet many attempt this, only to become disgusted with the scheme in the end. In one of our districts, where results were distinctly worst, *bonâ fide* students were found suffering from want of leisure. Every evening from Monday to Saturday was occupied in some kind of improvement, and time to digest the mental pabulum was missing.

The work which is being inaugurated throughout the country, and which we hope is in a fair way towards completion so far as its preliminary arrangements are concerned, is destined, if properly carried out, to bring about a material and social advance among the masses. No class is more in need of this than the rural population, and we can hardly help thinking that the easiest way of improving the worker in the fields, both in regard to his efficiency and his influence as a factor of society, is by giving him increased interest in his occupation by education, such as is being attempted by the County Council. Nothing seems to be more certain than that the possession of

a cottage garden by all labourers, of such dimensions that it is worth calling by this name, would be a boon to the village community generally in agricultural districts. Those who are intimately acquainted with the country are aware that the material happiness and comforts of the labourer are very often bound up with the possession or want of, not three acres and a cow, but a garden. Time spent in the latter nearly always means time lost to the public-house, and this, together with the material benefits derived from the culture of fruit and vegetables, and the pride evinced in the success of his energies, is sufficient to justify a considerable expenditure and sacrifice of time and money on his education. In this simple matter there is undoubtedly a key to the solution of some of the social problems which are at the present moment beginning to trouble the minds of all thinking people.

It is necessary to consider both the adult population and the rising generation, to some extent, apart from each other. With the former, so long as it lasts, we must be content to make the teaching immediately of service, and suited to their mental capacity; with the latter we must deal somewhat differently, and endeavour to lay the foundation of a more extended education than their progenitors have had.

At one centre practical work in Horticulture has been attempted, with very good results. The local committee have had a piece of land placed at their disposal, which after being fenced in and divided into plots has been handed over for cultivation to the lads of the village who have left school. The work is superintended and explained by professional gardeners, and advice and instruction on the work in hand is given at intervals in the form of an address by one of the County Council's lecturers. Seeds, tools, &c., are provided by the Council, and the produce becomes the property of the pupil. Arrangements are being made to extend the scheme in other parts of the county.

A great deal of power is lost if education is allowed to slip into the background when a boy leaves school for good at the age of 12 or 14. In the country, where there is little incentive or opportunity for improvement, at 20 he is frequently a blank. This is to be kept in view, and in Surrey, schemes for continuation schools are already in active preparation. For this work competent teachers will be required, and a large number of County Councils have set aside and already expended much on lectures and classes for elementary teachers, in order to fit them for extended usefulness. Zeal and intelligence have been displayed by all these students, and it has been possible to make rapid progress. Classes in horticulture, botany, chemistry, and physiology have been carried on during three sessions, and forty scholarships of £10 each have been competed for, which will enable the successful candidates to avail themselves of the opportunity for pursuing their studies during the present month at the University

Laboratories of Oxford and Cambridge, where special courses for County Council students have been arranged.

During the summer, open-air "Lessons in Gardening" are being given also at other centres, and the attendance of thirty or forty people at each demonstration has exceeded the expectations of the Committee.

In all this rush for technical education it is necessary to carefully avoid pandering too much to the tastes of the practical man. Unless the science is kept well to the front, the teaching degenerates into a mere catalogue of dry operations as dead as the average gardener's manual; this does, indeed, help its student to grow various kinds of produce, but the result is due to the inherent power of Nature to assert herself under adverse circumstances, rather than skill or intelligence on the part of the gardener. It is time we lifted gardening and agriculture out of the blind empirical stage, and this cannot be done by mere repetition of operations, however sound a basis these may have. Great praise is due to the Royal Agricultural Society for its recent efforts to spread scientific teaching in the preparation of a simple and reliable text-book on "Agriculture," by Dr. Fream, under their authority. We hope to follow up this example by a "Structure and Physiology of Plants," for the benefit of the gardener.

The machinery for this huge scheme of Technical Education cannot possibly be perfected or completed in a day. Let us hope, however, that the work already begun will develop into a system which will produce as intelligent and skilful a workman as the best of our Continental competitors, and at the same time be peculiarly English in character.

J. PERCIVAL.

SOME NEW BOOKS.

VOLCANOES: PAST AND PRESENT. By Edward Hull, M.A., LL.D., F.R.S., Examiner in Geology to the University of London. *Contemporary Science Series.* London: Walter Scott, 1892.

THIS volume is published in good company; it belongs to a series which has hitherto well maintained its claim to represent the state of contemporary science. It is not without other credentials,—the author is a Fellow of the Royal Society, a doctor of laws, and was, until lately, the Director of the Geological Survey of Ireland. We shall therefore confidently expect to find in it many things both new and true; indeed, the author expressly promises in his preface "the most recent conclusions regarding the phenomena and origin of volcanic action," illustrated by "examples drawn from the districts where these phenomena have been most carefully observed and recorded under the light of modern geological science." A past Director of Geological Survey, writing with these aims, may be fairly expected to know something of the volcanic rocks of which he makes frequent mention throughout the book, yet there is something in his use of terms which, as we read, begins to excite uneasy suspicions, and leads us at last to consult the Appendix, in which is given "A Brief Account of the Principal Varieties of Volcanic Rocks." Let us examine this "in the light of modern geological science." Whatever arguments can be adduced to prove that granite is essentially a deep-seated rock, are equally applicable in the case of gabbro. Every English geologist might have been presumed to have known this much since the appearance of Professor Judd's epoch-making work on "The Ancient Volcanoes of the Highlands of Scotland," published so far back as 1874; yet on page 261 we read that gabbro *is* a volcanic rock, and a little later that granite *is not* a volcanic rock. We regret that we are not favoured with reasons for this distinction; they would no doubt be interesting in the light of "Contemporary Science." We shall discover more contemporary science in the "Tabular View of the Chief Igneous and Volcanic Rocks" given in the Appendix: the well-known sub-division into Basic, Intermediate and Acidic rocks is adopted, but, altered and unaltered, volcanic and plutonic rocks are all jumbled up together, as though each were a species of the same value. But this is a trivial defect in comparison with graver errors; thus we find the Porphyrites placed in the Basic group, and the Andesites, of which they are the altered equivalents, in the Intermediate group, along with Syenite and Mica-trap, while Trachyte is separated from its plutonic representative, Syenite, and placed with the Acidic rocks in company with Domite and Phonolite. And this is Contemporary Science!

Had the author only consulted Professor Judd's work on "Volcanoes," published in the International Science Series in 1885,

which is, indeed, a work of Contemporary Science, he would have been saved from these and worse errors. Perhaps it is because the author proposes to give only the more important varieties of igneous rocks that he omits all reference to the interesting group of Peridotites; but if so, on what grounds is Domite, a mere variety of Trachyte, given such undue prominence? In the definitions of the different kinds of igneous rocks errors are no less plentiful than gross. Basalt is assigned the "high" specific gravity of 2·4 to 2·8; familiarity with the specific gravity of igneous rocks would have rendered such an error as this impossible, 2·4 is a low specific gravity, and the numbers should run from 2·8 to 3. Of Diorite we read that mica (which species?), epidote, and chlorite are present as "accessories." This abbreviation, which recalls the police news, is hardly fairly employed in the case of the last two minerals, which, using the author's phraseology, should rather be spoken of as "secondaries." Syenite is referred to the "Felstone family," though what this family is there is nothing in the work to show; to the same mysterious family the Mica-traps are assigned, also called Lampophyres, so spelt in each of the three or four places where the word occurs; but we must humbly apologise for noticing a trivial error in spelling; there are much better things in the syntax, to which we refer our readers for real enjoyment. In defining Andesite, the author cannot go far astray, as he here commits a plagiarism (no doubt unconscious) on one of those authorities (Dr. Hatch) whom he "is unable to follow," either in classification or definition. Them we cannot commiserate! Trachyte is defined as consisting of certain minerals, which are enumerated, and again "accessories," a style of writing which reminds us of Sam Weller's "leg of mutton and trimmings." We are grateful, however, for the information that the mica is biotite, since, for the most part, the unfortunate mica is made to do duty for all its varieties, like a maid-of-all-work. Here, however, a delicious passage occurs, of which we would by no means deprive our readers; we need merely quote "The trachytes occupy a position between the pitchstone lavas on the one hand, and the andesites and granophyres on the other." And this is Contemporary Science! In a footnote, Mr. Allport is said to have discovered "Leucite basalt" in the Wolf Rock. In another place the author rightly refers to this rock as Phonolite.

Under the head of granite we learn that the quartz is the paste or ground mass in which the felspar and mica crystals are enclosed, and this after the elaborate descriptions of this rock which have lately appeared in the English language; further, that "its representatives among volcanic ejecta are to be found in the granophyres, quartz-porphyries, felsite, trachyte(!), and rhyolites . . . and to one or more of these the so-called granites of the Mourne Mountains, of Arran Islands, and of Skye are to be referred." And this is Contemporary Science. Truly the author has the names of Judd, Teall, Rosenbusch, and Zirkel on his lips, but their precepts are far enough from him.

The Appendix closes with some plates, which it is hoped may providentially prove of assistance to somebody. If we turn to the body of the work we shall find the same Babel of terms, thus (p. 70) the Isles of Lipari are said to consist "of pumice and obsidian passing into trachyte." Surely if the author is not familiar with augite andesite, which enters into the composition of these islands, he has at least heard of Liparite, and this should have prevented him from speak-

ing of obsidian passing into trachyte: on page 119 the hornblende andesite of the Wolkenburg is styled trachyte, and the same term does duty for the tephrite of Niedermendig: on page 234 granophyre is said to be a variety of andesite! And this is Contemporary Science!

The greater part of the work is a compilation of descriptions of volcanoes and volcanic areas, and is certainly not salted with the salt of modern science. Before commenting on the grave errors that render this part of the work really misleading, let us point out a few of those small blemishes which may serve like a rash to diagnose the disease within. On p. 63 we read "Val del Bove (Valle del Bue)"; little pedanticisms like these may be an adornment when correct —but when they are wrong! Let us have it Valle del Bove (Val di Bué). On page 198 the discovery of the volcanic nature of the rocks of Llyn Padarn is attributed to the Rev. J. F. Blake, though Professor Bonney had preceded him in this matter by some ten years (Bonney, On the Quartz-felsite and Associated Rocks at the Base of the Cambrian Series in N.W. Carnarvonshire, *Quart. Journ. Geol. Soc.*, vol. xxxv., p. 309, 1879). On page 68 we are led to understand that we owe our knowledge of the chemical and microscopical characters of the lavas of Vesuvius to Haughton and Hull. The chemical analyses of the first-named author are of undoubted value: they are cited in standard works, but the microscopical investigations of Hull do not seem to have obtained, owing to some reprehensible oversight, the same recognition. Errors are so congenial to our author that he actually goes out of his way to make them. There was no pressing necessity to explain the sky-effects following the eruption of Krakatoa; the author says as much, yet on this difficult problem he cannot be silent. After reading his explanation on page 214, let the reader turn to the elaborate discussion on sky-glows in the Report of the Royal Society—verily a little knowledge is a dangerous thing! To account for the order of emission of the different kinds of igneous rocks from a volcano the early crude form of Durocher's hypothesis published in 1857 is resuscitated, and no mention is made of Richthofen's results (1868) and the explanations of Teall and others. And this is Contemporary Science! On the hypotheses of volcanic activity ventilated by our author we cannot afford to dwell, they are expressed in such loose and unscientific language that it is difficult to get at their meaning with any approach to precision. To anyone who thinks it worth while to consider them we will utter only one caution, let him first make himself acquainted with the strength of materials under pressure and tension, and he will be proof against many of the fallacies of this part of the work.

Where the author is describing things immediate to the senses, he wields, not unskilfully, a fluent pen, which might not discredit G. A. Sala, but directly phenomena involving exact observation, or depending on inductive reasoning for their explanation, are concerned, his efforts are painful. Let us take, for instance, his account of volcanic eruptions. An unscientific writer might be pardoned for an impressionist description of phenomena, and we do not greatly blame a newspaper writer for colouring his accounts of "burning mountains" with "fires" and "flames," but this is not science, and the work before us claims to describe phenomena "which have been most carefully observed and recorded under the light of modern geological science." Just, therefore, is our indignation when we find our author ignoring the true cause of the volcanic glow, and indulging in all the

paraphernalia of "burning mountains," "flames," "smoke," and "fire," and not only so, but inventing a new error, as where he enhances the brilliancy of his pages with "illuminated gas." No doubt exists about the occurrence of flames during an eruption, but they are so feeble that acute observers like Spallanzani and von Waltershausen were led to deny their existence. But Pilla described them so far back as 1833, and they have repeatedly been seen since, one of the last exact accounts of them being that of Fouqué; but they do not play a prominent part, and certainly do not explain the volcanic glow. For a modern writer to thus recklessly revive ancient and long-exploded errors under the name of "Contemporary Science," is to commit no less than a crime against science. We should not have wasted so much space over this work, which was, perhaps, not intended to be taken seriously, but for our fear that it will, in many partially instructed minds, sow the seeds of error, and we abstain from pointing out its lost opportunities; those sins of omission which are as grave and numerous as those of commission. We have now done with our author, whose ignorance of Contemporary Science is of that most fatal kind, the invincible ignorance which is ignorant even of its own existence.

THE ETIOLOGY AND PATHOLOGY OF GROUSE DISEASE, FOWL ENTERITIS, ETC. By E. Klein, M.D., F.R.S. Pp. xii., 130. 53 illustrations. London: Macmillan & Co., 1892. Price 7s.

IT would be hard to point to any investigation of disease and its cause showing greater acuteness of apprehension, honesty of method and resourcefulness in direction than this research of Dr. Klein's. The elusive *causa causans* of grouse-disease has at last been captured. The only certainty about the matter in the minds of those who have tried to grasp the cause of this disease has hitherto been the insufficiency of the late Dr. Cobbold's theory of parasitism by a nematoid worm. Grouse that die of this disease beyond doubt very frequently exhibit the presence of this and other parasites, but these parasites also occur in other birds not subject to it; they are not invariably present, and, above all, the dead birds frequently appear plump and without sign of emaciation. Suggestive contributions to the solution of the mystery were made by Drs. Colquhoun, Macdonald, Farquharson, and Mr. Andrew Wilson, but it was left to Dr. Klein to capture the bacillus, cultivate it, and successfully inoculate mice and birds with the virus. The great difficulties attending the inoculation of grouse have prevented the absolute demonstration of the matter, since inoculation in an infected area would prove nothing, and their inoculation on a moor free from the disease would be somewhat expensive, to say the least, while the difficulties with regard to tame grouse are hard to surmount. It will hardly be objected, however, to this research that there is any lack of corroboration, since the inoculation experiments on other animals adequately answer such objections. There are, moreover, very interesting and hopeful observations with regard to an attenuated virus.

The accounts of a pathogenic bacillus resembling that of grouse disease, of fowl enteritis, and of "cramps" in young pheasants are all masterly contributions to the rapidly-growing knowledge of disease of this class. There is nothing so impressive in the book as the deadliness of aim shown by its author throughout—it works up to a positively artistic effect when the reader comes to the final investi-

gation. One can scarcely pay higher tribute to Dr. Klein's genius as an investigator than by describing his book as a very readable one—in spite of the unfortunate fact that it is almost as badly written as any scientific book in the English language. That invites many odious comparisons, but it is surely time to make protest when so admirable a work in other respects appears seamed with disfigurements of this kind. Finally, the illustrations and "get up" of the book deserve a very hearty word of praise.

The Physiology of the Invertebrata. By A. B. Griffiths, Ph.D. Pp. xvi. and 477. London: L. Reeve & Co., 1892.

It is not often that one receives such a thoroughly disappointing book as Dr. Griffith's Physiology of the Invertebrates, for while a reliable compilation upon this subject has long been a desideratum, Dr. Griffith's work is so scrappy and untrustworthy that it cannot be recommended to the students to whom it ought to be of most use. The book consists of thirteen chapters, which deal in succession with introductory principles and classification, the chemistry of protoplasm, digestion, absorption, the blood, circulation, respiration, secretion and excretion, nervous systems, special sense, locomotion, and reproduction. The chemical side of the subject is the best treated; the illustrations of the apparatus employed, and the tabular summaries of the results of recent work, such as those on the digestive processes, are certainly useful. The author, however, here assumes far too much knowledge in his reader, *e.g.*, on p. 11 he talks about the specific rotary power of albumen without the slightest explanation for the non-physical student. In other departments the author is less successful; the value of his classification may be judged from his retention of the Eurypterids and Trilobites among the Crustacea, of *Actinophrys* in the Radiolaria, of the Edrioasterida and Rugosa as separate classes, his inclusion of the Hirudinea in the Annelida (p. 256), and his obsolete classification of the Mollusca. This weakness in knowledge of Systematic Zoology is only here of consequence as it leads the author into a series of serious physiological blunders: he makes statements about whole classes which are only true about some members or divisions of those classes. Thus, on p. 34 he puts the possession of an anus as among the characters of the Asteroidea, and of a jaw apparatus among those of Echinoidea; on p. 414 he says that the latter have five genital glands. In many important points, moreover, the author's views are very antiquated and striking recent researches have been omitted. Thus, he states on p. 368 that the Polyplacophora have no eyes, in neglect of Moseley's memoir; his remarks on the eyes of the Echinodermata are rendered valueless by the absence of reference to Sarrasin's discovery of a series of eyes in *Diadema*; the section on the nephridia of the worms is also sadly incomplete, and the few references to papers given suggest a scanty acquaintance with the literature of the subject. In the chapter on respiration, the orthoptera are the only insects to which is referred cloacal respiration; the tissue respiration of the Asteroids is well described, but this is counterbalanced by the omission of reference to the dermal branchiæ. The statement (p. 243) as to the renal function of the pyloric pouches of the Echinoids is somewhat premature, especially in face of the absence of reference to the renal nature of the so-called "heart." There is all through the book a considerable amount of dogmatism; thus, in

dealing with the nervous system he claims unhesitatingly "the origin of the vertebrate brain and spinal cord as the outcome of the nervous system of the Arthropoda"; the space devoted to these remarks might well have been occupied by more precise descriptions of the Arthropod nervous systems, and some attempt to explain to the student the difference between the archicerebrum and syncerebrum, neither of which terms are as much as mentioned. The omission of any reference to the nervous nature of the radial cords in the Crinoids is also unfortunate; the eight pages devoted to the nervous system of the Echinodermata well illustrate the casual nature of the author's method, for with the exception of a few remarks about the eyes of the Starfish, the whole section is restricted to the one type *Echinus*, while the other classes are quite neglected. Extracts of great and unnecessary length are made throughout the book, but it seems difficult to see on what principle the author has made his selections. We should imagine he had bought up a job lot of Mr. Romanes' pamphlets and incorporated most of them. Probably the most serious omission is that there is absolutely no reference to the phagocytes, and this alone would be quite sufficient to condemn any book on Invertebrate Physiology.

LIFE IN MOTION. By J. G. McKendrick, M.D., &c. London: A. and C. Black, 1892.

PROFESSOR MCKENDRICK'S book (of 200 pages) is written in a pleasant and popular style, such as we might fairly expect from the author. His lectures at the Royal Institution, substantially reproduced in this little book—as, indeed, the occasionally interspersed "my young friends" reminds us—were probably even more interesting than the report of them before us. Some of the effect of the subject is lost, for apparatus is much less convincing in pictures than when seen in action. With the exception, however, of the phrase quoted above, and repeated more than once, the book is not written in lecture form, and is all the better for that. "The delivery of the lectures," says Dr. McKendrick, "was a pleasure to myself"; and he expresses the hope that, when in print, they will interest not only the young, but "even those who may feel that they are no longer young." We ourselves are in the sere leaf and belong to the latter category, and we have undoubtedly been most interested in reading the work. Its object is to give an idea of the principal facts in muscle physiology; the title, therefore, is a little misleading, as there is no account of ciliary movements. Though professedly written for young persons of all ages, we notice an occasional lapse into unexplained technicalities; thus, on page 163, the author speaks of an instrument which has a resistance of 86,000 ohms, an expression which can hardly have been understanded of the people. Besides the illustrations of apparatus there are numerous cuts, illustrating other points; diagrams, for example, are given of the musculature of the human arm and the frog's leg; *apropos* of the arm muscles, Dr. McKendrick intimates that young ladies, although they possess a biceps, are not so much interested in its welfare; in these days of lawn tennis, of lady champions, and of Girton College, we are disposed to think that the biceps is greatly in favour with the other sex. On page 79 is a reproduction of a tracing of the muscle curves, obtained during the writing of a sentence, which illustrate the innumerable contractions necessary to make up what appears to be a single effort on the part of the muscles; the curiously shaky result which is thus produced is indeed

suggestive of a New Year's entertainment, but not at the Royal Institution. Chapter V. contains some valuable applications of physiology to practical life; it is reassuring to those of us who are compelled to lead a more or less sedentary existence to learn that the best form of exercise is walking. But if we are to select that form of exercise which is most in accord with our physical organisation, we ought not to forget that the arms should be used as well as the legs; Dr. McKendrick omits to mention that according to "them perfane Darwinists" climbing would probably be an excellent pursuit; the safest and best imitation of this is afforded by the Gymnasium.

The Microscopic Determination of Rock-forming Minerals. Tabletzui dlya Mikroskopicheskagho opredyeleniya porodoobpazuyushchikh Mineralov. By F. Loewinson-Lessing. 4to. Pp. xii. and 16, and folding tables. St. Petersburg, 1891.

As an indication of the great strides now being made in Russian geology, we are glad to be able to call attention to this admirably-compiled set of tables of the rock-forming minerals, which, but for the fact that they are written in Russian, would probably prove of greater service to general students than either of those previously issued. Dr. Loewinson-Lessing points out in his preface that he has been impelled to write this work by the fact that so many Russian students are unable freely to use those in French and German. He has, however, arranged the work on a plan that seems a great advance on those previously attempted. The tables of Rosenbusch Michel-Lévy, and Lacroix, and the earlier ones of Doelter and Hussak, give the name of a mineral first, and after that its characters; thus, if we know the mineral, these give us an admirable method of verifying the determination. But if a student is quite at sea as to the name of some mineral new to him, these tables give him but little assistance. In Dr. Loewinson-Lessing's tables, however, the method is reversed; if you know the characters of the mineral you can determine its name. In addition to a large synoptical table, he gives five tables, each using one set of characters. The first table is based on examination in polarised light; the minerals are grouped as opaque, semi-transparent, and transparent; the last is divided into colourless and coloured, and, by aid of the nature of the extinctions, the pleochroism, notes on the general aspect, &c., the species may readily be traced. The remaining tables are devoted to the external morphological characters, the discrimination of the crystalline systems and their application, and the more technical optical characters. As these tables are intended mainly for students and general working geologists, only those minerals are included which are of practical importance. A little over 80 are included, and the long lists of little-known minerals that fill up the tables of Rosenbusch and Michel-Lévy, are wisely excluded. Dr. Loewinson-Lessing's experience, both as a teacher and a skilled petrologist, is sufficient guarantee of the wisdom of his plan and the accuracy of its execution.

English Botany. Supplement to the Third Edition. Part II. (Orders xxiii. to xxvi.). Compiled and illustrated by N. E. Brown. 4to. Pp. 59–160, with one coloured plate. London: G. Bell & Sons.

In Natural Science for June we noticed Part I. of the Supplement to the Third Edition of English Botany. Part II. is now to hand,

and is, like its predecessor, the work of Mr. N. E. Brown, of the Kew Herbarium. Of the 104 pages, a few lines are given to a note on *Acer campestre*, ten pages to the Leguminosæ or, as they are styled, Leguminiferæ, while the remainder is taken up with the Rosaceæ, and almost entirely with the two genera *Rubus* and *Rosa*. Mr. Brown admits that he has never studied these two difficult genera, and can, therefore, express no opinion. Professor Babington has revised the proofs of *Rubus*, besides supplying notes for some of the species, while in the Roses Mr. Baker's grouping of the varieties has been followed. A good deal has been done in these genera since the publication of the third edition, and we are inclined to think the present account will form one of the most valuable parts of the Supplement, even though the author does not venture to express his own opinion and it be but a compilation. Mr. Brown is a painstaking compiler, and has had work of the very best from which to draw, and some British botanists may set a higher value on such compilation than on more original notes, for British botany must be learnt in the field, and the many varieties and critical forms studied as they grow. Conclusions drawn from herbarium specimens are not always borne out by the living plant, and although the facts relating to plants and their synonymy have been assiduously collected for the Supplement, yet we seem to miss the matured experience and confident criticism of the practised field-worker.

The paper and printing are as excellent as those of the previous part, while the solitary plate (*Potentilla norvegica*) is as poor as its predecessors. It looks wiry, and is evidently not the work of a competent artist.

GRASSES. By C. H. Johns, M.A. Fcap. 8vo. Pp. 96. London: S. P. C. K. Price 1s. 6d.

THIS little book is styled "An Appendix to the late Rev. C. H. Johns' Flowers of the Field"; referring, of course, to the well-known work by the Rev. C. A. Johns, also published by the S. P. C. K. The title is misleading, as a third of the work is occupied by the Sedges or Cyperaceæ, while the decoration of the cover with the common Bulrush (*Typha latifolia*), a member of an Order not very closely related to either Grasses or Sedges, is hardly commendable.

A handy popular flora devoted to these glumaceous monocotyledons has long been a desideratum, but we fear the arrangement and descriptions in the present work are not sufficiently popular to supply the want. In "get up" it is distinctly inferior to "Flowers of the Field," the letterpress is not so clear, and the illustrations, which form so essential a part in such a flora, are very poor. It is a pity we cannot have a new edition of the whole work brought up to date, and including not only Sedges and Grasses, but also the Ferns and allies. The thirty pages occupied by an account of the Linnæan arrangement might be left out, and some of the space devoted to a fuller explanation of terms now rather crudely disposed of in 14 pages.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE German Anthropological Society holds its annual meeting at Ulm on the 31st July.

M. A. F. BATALIN has been nominated Director of the Botanical Garden of St. Petersburg in the room of the late E. Regel.

THE International Congress of Physiologists will take place at Liège, from the 28th to 31st of August.

A NEW Zoological Museum is shortly to be built at St. Petersburg, and the Imperial Academy has sent Dr. Pleske to inspect the principal museums of Europe to report thereon. A paragraph in our last issue announced a similar enquiry on the part of the Japanese Government.

THE Natural History Museum will shortly receive a piece of the skin, with wool attached, of the Mammoth. This specimen forms part of an exchange with the Imperial Academy of St. Petersburg, and comes from an individual discovered in the province of Jakutsk, Siberia.

THE Imperial ukase requiring that all lectures given in Dorpat shall be delivered in the Russian language, instead of in German as hitherto, comes into force next year. This necessitates the resignation of several of the Professors, including, among others, Professors Lemberg and Rosenberg, who hold the chairs of Geology and Comparative Anatomy respectively.

THE National Chrysanthemum Society are expecting an interesting arrival from New Zealand. Mr. John Earland, of Wellington, having raised some chrysanthemums from seed, is anxious to obtain the Society's opinion on the blooms. He has accordingly made arrangements to have them frozen in blocks of ice at the meat-freezing works and forwarded to London in the meat steamer. As soon as the ship arrives, a meeting of the General and Floral Committee is to be called to examine and report upon the flowers.

THE Professorship of Geology and Mineralogy at Breslau, left vacant by the death of the late Professor F. A. Römer, is to be subdivided: The Professor Ordinarius will occupy the chair of Petrology, and the Professor Extraordinarius will have charge of the department of Palæontology. It is considered probable that Dr. Fritz Frech, of Halle, one of the leaders of the younger school of German geologists, will receive the latter. Dr. Frech is well known as the Monographer of the Triassic Corals, and as a writer on past zoological distribution.

Mr. F. A. Bather, of the Geological Department of the British Museum, has been awarded the Rolleston Scholarship for his work on Fossil Crinoids and the Phylogeny of the Cephalopoda.

In the recently-issued list of Doctors of Science of the University of London are included the names of Mr. Bernard Dyer, the well-known agricultural chemist, and Professor T. Jeffrey Parker, of Otago.

Mr. J. Bretland Farmer, M.A., Fellow of Magdalen College, Oxford, and Demonstrator of Botany in the University, has been appointed to succeed Dr. Scott as Lecturer on Botany at the Royal College of Science, South Kensington, Dr. Scott having undertaken the charge of the Jodrell Laboratory at the Royal Gardens, Kew.

Mr. F. A. Bather contributes a paper to the last Report of the Museums Association on the Fossil Crinoids in the British Museum, in which he describes his "attempt to put into practice modern ideas of museum arrangement." He accepts as his rule the American maxim that a museum is a collection of labels illustrated by specimens. The paper mainly consists of a reprint of the thoughtfully prepared labels he has attached to the collection, and will serve as an admirable model for educational museums. He also makes the sensible suggestion that museums should exhibit more chairs, and a few light "dumb waiters on wheels" for the use of visitors. He thinks, however, the use of these as go-carts by children should be discouraged.

We have received from Dr. A. Jentzsch a copy of his "Führer durch die Geologischen Sammlungen des Provinzialmuseums der Physikalisch-Oekonomischen Gesellschaft zu Königsberg." This is a pamphlet of 104 pages, illustrated by 75 process blocks, and two stratigraphical tables. The guide serves as an admirable summary of the general geology and palæontology of Eastern Prussia. The most important collections in the Museum are the amber plants, and the inter-glacial mollusca and plants. In the preface short reference is made to the history of the Museum and the principal collections it contains. We wish other German museums would follow Dr. Jentzsch's example.

The third part of the second volume of the *Journal of the Marine Biological Association* contains several papers of much practical value. The most important is a report by Mr. J. T. Cunningham, "On the rate of growth of some sea fishes, and the age and size at which they begin to breed." The paper is a somewhat long one, and is crowded with statistics respecting the cod, flounder, dab, sole, herring, sprat, pilchard, anchovy, &c. The most interesting addition to the fauna of the Plymouth district is a Siphonophore, which was comparatively abundant last autumn, and even worked its way into the Sound; it is described by Mr. Cunningham as a new species, *Muggiæa atlantica*, and has affinities with a species living round the Canary Isles. Mr. Hughes is still continuing his experiments on artificial bait, but, so far, with few practical results.

While the National Laboratory at Plymouth is naturally devoting most of its attention towards problems having an economic bearing, the Liverpool Marine Biological Association is continuing its splendid work on the fauna and flora of the Irish Sea. As the Association's old station at Puffin Island was rather inconvenient of access, and as five years' steady work has nearly exhausted the fauna of the limited shores of that island, headquarters have now been removed to Port Erin, at

the south end of the Isle of Man. A new volume of the Association's reports upon the Fauna and Flora of the area within the Association's sphere of operations is announced.

THE celebrations in connection with the Tercentenary of Dublin University passed off successfully. Each day, from July 5th to 8th, was occupied by a succession of academical gatherings. The number of delegates and guests was not so large as had been expected; the unfortunate coincidence of the festivities and the General Election prevented the presence of many invited from England, and the representatives of natural science, in particular, were less numerous than had been hoped. The proceedings began on the morning of the 5th with a procession in academical robes to St. Patrick's Cathedral, where a commemoration service was held; in the afternoon the Provost, Dr. Salmon, gave a garden party, and in the evening, after the performance of the Tercentenary Ode, the Lord Mayor gave a reception and ball. On the 6th, after the conferring of honorary degrees, there was a garden party given by the Lord Lieutenant, and, in the evening, a grand banquet by the University at Leinster Hall. On the 7th, the delegates presented addresses of congratulation, on behalf of their universities, to the University of Dublin; another garden party by Lord Wolseley oocupied the afternoon, while the evening was devoted to a performance of the "Rivals" by University students. On the 8th, some of the delegates addressed the students, a party visited Lord Rosse's castle and observatory, near Parsonstown, and in the evening the festivities concluded with the University Ball.

ENTOMOLOGISTS will be interested in an exhibition of tropical Lepidoptera by Mr. Wm. Watkins, of Croydon, now being given at 21 Piccadilly. The collection includes many fine specimens of African rarities, such as a pair of *Papilio antimachus* from the Gaboon, and also several of the Madagascan *Urania rhiphæus*. Many striking instances of protective mimicry are shown in the collection, which most entomologists will find worthy a visit.

OBITUARY.

KARL HERMANN KONRAD BURMEISTER.

Born 1807—Died May 1, 1892.

BURMEISTER was born at Stralsund, and studied medicine at Greifswald and Halle, obtaining his M.D. at the latter place in 1829. In 1837 he became Professor of Zoology in the University of Halle, but resigned his chair in 1850, having taken an active share in the political struggles of the time. He went to Brazil on a scientific journey, and returned to Germany, where he resumed his professorship at Halle. Again returning to South America in 1856, he travelled about the Argentine provinces, and made a journey across the Andes by a new route. Once more visiting his native country in 1860, he finally resigned his chair at Halle in 1861, and became Director of the Museum of Natural History in Buenos Ayres, which he had established and furthered. In 1870 he was appointed Curator of the University of Cordova. Burmeister was chiefly known from his long series of papers on vertebrata, published in the *Anales del Museo Publico de Buenos Aires;* his "Systematische Uebersicht der Thiere Brasiliens" (1856); and his "Description Phisique de la République Argentina" (1876). He died on the 1st May, and received the honour of a State funeral, at which the President attended in person.

Memorial to Professor Duncan.

A memorial brass is being prepared in memory of the five members of the staff of Cooper's Hill College, who died last year. Of these the best known was Professor Peter Martin Duncan, and geologists will therefore feel a special interest in this tribute to the memory of one of the most philosophic of palæontologists of our time.

OBSERVATIONS AND CORRESPONDENCE.

Life at High Altitudes and at Great Depths.

[The following remarks have been suggested by Mr. Whymper's conclusions recently noticed in our review of his work (pp. 219-220), but Mr. Whymper has of course considered the question of the experiences of æronauts and dismissed them on account of the suddenness of the alterations of pressure.—Ed.]

Experienced hunters upon the Himalaya pursue game, without feeling any appreciable inconvenience, at altitudes exceeding 20,000 feet above the surface of the sea, and balloonists have penetrated up to 37,000 feet, or fully 7 miles above the sea's level.

Mr. Glaisher in one of his balloon ascents remained in apparent health up to 26,400 feet, or 5 miles, but, on arriving at 29,000 feet, 51 minutes after leaving land, he lost all muscular power, soon followed by insensibility and fainting, possibly due to the glut or engorgement of blood in the lungs from increasing insufficiency of oxygen, intensified by diminished skin-action in a dry temperature of 37° Fahrenheit *below* the freezing point of water.

Hitherto high balloon ascents have been performed with such speed that either 1,000 feet upwards or 4,000 feet downwards have been readily accomplished in one minute, while in mountain travelling an elevation of 1,500 feet is seldom completed within the hour.

Experiments indicate that by gradual ascents, long training, frequent acclimatisation, and cautious diet, high altitudes above 30,000 feet (now considered by many fatal to man) are, with precaution and practice, securely attainable.

The air-hunger experienced by man at high altitudes occurs also in fish when suddenly removed from a very low depth to the surface. Thus, from the experiments of the "Challenger," it appears that fish hooked at great depths often reach the surface in fragments, owing to the disintegration of the tissues, caused by the enormous expansion of the gases. In any case, the fish are dead and rigid, or the stomach evaginated through the mouth; considerable protruberance of the eyeballs may also occur. It is very interesting to observe that when nearing the surface the fish, owing to the expansion of its gases, is literally shot through the water, and even projected above the surface faster than the fisherman can draw in his line.

J. Lawrence Hamilton, M.R.C.S.

30 Sussex Square, Brighton,
1*st July*, 1892.

Sir,—I was struck by a remark in Professor Lloyd Morgan's masterly critique of the "Grammar of Science" in your last number.

He says that "considering how enormously complex are the influences which have conspired to bring about man as the crowning

product of evolution on this earth, we should judge it to be indefinitely improbable that human beings, as such, have been evolved on any one of those myriad planets" (p. 303).

No one can speak with greater authority on those "enormously complex influences" of evolution than Professor Morgan, but at the same time no one ought, more than he, to recognise that these complex influences have the value of fixed laws.

He well knows that celestial matter seems to respond to the same laws that govern terrestrial; that there are many celestial bodies of a similar constitution to our sun, and surely among their satellites there must be some of a similar kind with our earth.

Though we know nothing of life outside our planet, yet what we know of it here is seen to exist under fixed laws, and surely, granting life outside the earth, we would have it developing along those broad principles we know a little of. That is, in an earthly type of planet, accompanying a solar type of sun, we may expect to find a biological development of, at least, a recognisable similarity to our own. What marvels me most is, that Professor Pearson (the apostle of scientific Nihilism) should have written the passage which Professor Morgan attacks.—I am, yours obediently, J. Bell Findlay.

Lewisham, S.E., *June* 26, 1892.

Dear Sir,—I send you a copy of a notice which is posted in the bridle-path which runs along the foot of White Hill, near Caterham. What will the poor London botanist do if this becomes the general rule with South London landlords?

"£2 Reward to anyone giving information leading to conviction of persons stealing Berberis or other things, damaging shrubs, trees, fences, &c., *gathering roots*, *flowers*, or otherwise trespassing on the Estate. *Dogs will be used* day or night, if necessary, to help the capture of such offenders."

Fancy the innocent botanist pursued by an infuriated landlord and a pack of dogs. You will, I am sure, kindly warn all Rambling Clubs to avoid picking buttercups and daisies in the neighbourhood of Quarry Hangers, White Hill.—Yours truly, R. Paulson.

Some Habits of Birds at the Zoological Gardens.

It is unnecessary to point out at length the expediency of paying a close attention to habits and movement in the study of a group like the birds. The minor "tricks," as they may be called, are often suggestive of affinities in a group where these are especially hard to diagnose; and again actions are often witnessed which afford valuable indications of the way in which complete and important changes of habit have been effected. The London Zoological Gardens offer especially good opportunities to anyone desirous of acquainting himself with the traits and manners of many groups of birds; and in many cases also departures from these may be noticed, which for the reason above given are equally interesting. Gulls, for instance, have been noticed, when at large, to not unfrequently perch on trees; and in the large netted inclosure known as the Night Heron's Pond this may be often seen, the Common and Black-Headed gulls there confined appearing to find the top branches of

the dead trees a convenient perch. The single rather deep pond which this enclosure contained has been rightly done away with, and three smaller and shallower ones substituted; in which old pond I have more than once been interested by observing the Night Herons swimming—usually for only a few strokes, it is true, when they got out of their depth in bathing or reaching after floating food. But on one occasion one bird, driven into the water from an island by another, swam quite easily and composedly to the shore. Although the Common Heron is well known to be capable—as apparently are most birds of wading habits—of natatorial feats, I am not aware that similar performances have been before observed in the case of *Nycticorax griseus*, which sits as lightly on the water as a gull.

These occurrences are interesting, as showing how a shore bird might gradually and naturally, through seeking food on steep banks, become a swimmer; and others, noticed in these gardens and elsewhere, tend to show that the surface-feeding Anatine ducks could easily develop a diving habit. In the Northern Pond, last year, I was fortunate enough to see a male Mandarin Duck (*Aix galericulata*) dive repeatedly and energetically for food, and Mr. C. Bartlett informed me that the Summer Duck (*A. sponsa*) will likewise do so. The young of the Chilian Pintail (*Dafila spinicauda*), yet in down, repeated the performance elsewhere in the gardens at another time. This accomplishment may prove to be more common among the *Anatinæ* than is generally supposed; on four occasions I have detected it in the half-domesticated Mallards in the London parks, the birds being in one case ducklings in down. Swainson states that the Gadwall (*Anas strepera*) dives as easily and frequently as the sea-ducks; but I have not yet witnessed the action on the part of this species, though examples are at present in the Society's collection. The distinct little group of Tree-Ducks appear also to have the plunging habit, which I have noticed in the case of three species (*Dendrocygna major*, *D. vagans*, and *D. viduata*) before the deep pond in the Night Herons' quarters, where they are placed, had been altered. Of course, the surface-feeding ducks dive when in play, but the action in that case is very different, and easily distinguishable from the purposeful plunge of a feeding bird.

That the Tree-Ducks should dive is not to be wondered at, as their general build and the large size of their feet, in spite of their long eggs, distinctly recall the Fuliguline subfamily.

While on the subject of the ducks, it may be mentioned that the Common Foxes in the Zoological Gardens readily ate a specimen of the Common Sheldrake. As this bird is reputed to nest unharmed in an occupied fox-earth, and to be more than indifferent human food, it seemed possible that its very conspicuous appearance, in both sexes, and even in the first plumage, might possibly be of warning significance. It would be, however, very desirable to repeat the trial with a perfect specimen, as the bird given to the foxes had been plucked and disembowelled, circumstances having prevented me from carrying out my original intention of making myself the *corpus vile* of the experiment. I have no doubt, however, that I should have been able to discuss Sheldrake with toleration, as I could not discover any undue unpleasantness, or even fishiness, in the flavour of the Red-breasted Merganser or the Great Crested Grebe, to say nothing of the Scaup and Golden-eye, which appear not to be considered very good eating.

Two Red-billed Hornbills (*Toccus erythrorhynchus*) being at present

in the Parrot House, there is a good opportunity of comparing the smaller species of this interesting family with the larger forms to be found in the Eastern Aviary. Their greater activity is especially striking, and they do not seem to object to coming to the ground, where they hop about quite nimbly, in a manner very unlike the clumsy jumps of the Elate Hornbill (*Ceratogymna elata*) on the other side of the gardens. They are also fond of meal-worms, and it is probable that these small species generally are more insectivorous and less exclusively arboreal in their habits than the larger, a fact not without interest, with regard to the relationships of this family to the Hoopoes.

Why some birds should hop and others walk when on the ground it is rather difficult to say; and authors are not always very correct in describing the gait of birds. The more arboreal forms seem, as a rule, to be most given to this mode of progression; yet parrots walk, and the common sparrow, so much upon the ground, always hops. I particularly noticed, when the Kea (*Nestor notabilis*) was first brought to the gardens, that it distinctly walked on the floor of its cage, for, from accounts given, this genus would appear to progress by hops assisted by the wings, as may be seen in many birds when they wish to advance rapidly. But in the limits of a cage one could hardly expect to see this action; since it is not at all unusual to see the same birds advance sometimes by running and sometimes by jumps, when they have space—the Crows and Touracous for example; and the alternate hop and run of the Thrushes is familiar to everyone.

That very peculiar form, *Struthidea*, at present in the Parrot House, is a walker; and also may be seen to crack canary seed like a finch, a curious action in a bird of its decidedly corvine appearance.

A comparatively recent addition to the collection in this house is a specimen of the Sulphury Tyrant (*Pitangus sulphuratus*) which seems to display the adaptability of appetite characteristic of birds of this form, a portion of mouse and some butterflies (*Pontia*) appearing equally acceptable.

FRANK FINN.

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NOTES AND COMMENTS.

The New Geology.

THAT the main features in the physiography of the earth's surface are not arranged haphazard, but in accordance with some definite, if unknown, system, was recognised at an early stage in the study of geography, and has become familiar to fourth-form schoolboys through the maxim that the peninsulas all point to the south. No serious effort, however, to formulate the law that governed the evolution of the Continents, or even to systematise the leading facts of the problem, was made till recently, with the exception of that of Elie de Beaumont; and this author's clock-face diagram of the orientation of mountain ranges has long since been dropped from geological text-books into the same category as the cycles and anti-cycles with which the Neo-Ptolemists bolstered up the astronomical system of their master. That Elie de Beaumont's speculations were of no practical value is not surprising, since insufficient data had been collected for the establishment of the laws that govern the denudation of highlands, the redeposition of sediments thus formed, and especially of the re-elevation of those sediments into plateaux and mountain chains. This has now been done and, as we have just been reminded, stratigraphers have been reduced during the past few years to filling in local details, though such work as Dr. Lawson's Report on Rainy Lake has shown that even in this branch of geology sensational results may still be achieved. The resting stage, however, has not been for long: a whole host of new and fascinating problems has been raised by Professor Suess' "Antlitz der Erde," a

work which promises to hold the same relation to the schemes of Elie de Beaumont that "the Origin of Species" held to the theories of Oken and Lamarck. During the past two or three years, the heresies of Suessism seem to have been steadily gaining ground, and some measure of their present influence may be estimated from the fact that they have formed the theme of two of the Presidential Addresses at the recent meeting of the British Association at Edinburgh.

The "New Geology," as Professor Lapworth has aptly named it, may be defined as the application of the results of the old geology to the discovery of the factors that have determined the remarkably symmetrical arrangement which, often disguised by local and superficial variations, rules in the existing physiography of the world. Its scope may best, perhaps, be comprehended by a perusal of the two addresses.

The main interest in Professor James Geikie's discourse to the Geographical Section on "Coast Lines" was, that he so clearly stated the differences between what Professor Suess has called Atlantic and Pacific types, the distribution of which has been figured by Neumayr.[1] Professor Geikie showed, moreover, that the cause of the difference was simply due to age: that round the great oceans, the main north and south coast lines probably date from the same primeval age, but that around the Pacific, and most typically on the American margin, comparatively recent earth movements have raised mighty mountain chains, now studded with volcanoes and closely following the extreme verge of the continental plateaux. On the Atlantic, on the other hand, the ranges upon the ocean border are of an enormous age, and therefore have been denuded into low rounded hills, are indented by gulf and fiord, and have been cut far back from the margin of the oceanic trough. In Professor Lapworth's address, on the other hand, the fundamental identity of the structure of oceans and continents was clearly demonstrated, the former being the troughs that balance the elevations of the latter: in each depression corresponds to depression, and ridge to ridge, and on both alike the lines of volcanic action lie along the weakened limbs of the great earth-fold. There is at first sight some apparent contradiction between the two addresses, but as far as such actually exists it is mainly on points of detail. At first it does seem as if the fact that the dominant north and south lines were developed in primeval antiquity were in favour of the permanence of the oceans and continents; but the mid-oceanic ridges probably date from the same time, and if amid the numerous relative changes of level they had stood above the ocean surface, and the existing continents had subsided, they would have furnished quite as strong an argument for their having been always continental, and existing land areas been always oceanic.

[1] Erdgeschichte, vol. i., p. 345.

The two addresses agree that the great mountain ranges of the world, whether above water or below them, had their foundations laid in the pre-geologic era of the solidifying globe, as the later crustal movements seem to have followed the original lines of weakness; and this not only applies to the great meridional chains, but to the intersecting east and west lines, such as the Mediterranean-Caribbean depression and the corresponding alpine line of elevation, which dates at least from Palæozoic times. Similarly they both conclude that the oceanic islets of the Pacific indicate an area of subsidence, and the submerged ridge of the land area there once cut up the Pacific Ocean, as the probable connection of the Atlas with the Venezuelan Cordillera once broke the continuity of the Atlantic.

It, therefore, seems probable that the new stratigraphers will allow the palæontologists the principal transoceanic bridges that they demand. Professor James Geikie suggests that the cosmopolitan distribution of Palæozoic species was really due to the absence of any oceanic abysses to bar the spread of littoral species. We thought it was now pretty generally understood that the supposed identity of, for example, the Carboniferous faunas of the world, was simply due to the diagrammatic figures of the European species having led to essential distinctions being ignored and species wrongly identified; closer study has shown that such world-wide faunas are myths, and that we have the same cases of some Palæozoic faunas common to the two sides of the Atlantic, with succeeding ones that are as sharply differentiated as any of later date. The study of extinct faunas is, however, as yet in its infancy, and it seems not unlikely that one of the results of the new geology may be the ultimate separation of the zoologists from the palæontologists, the former studying species and the latter faunas.

The problems suggested by the work of the Vienna school of geology are of intense interest but great difficulty, and will require for their solution the help of all the three branches of stratigraphy, petrology, and palæontology. It forms a curious comment on the oft-made statement that the future of Science is with the specialist, that the man who has done so much to set the pace for the new geology has written on nearly every branch of geological science, and is equally at home in the details of the stratigraphy of the Alps, the Central Plains, or the Tertiary basins. He has dealt with nearly every group of animals that has left traces for palæontologists to decipher, from sponges to mammals, and from fish to foraminifera, and has advanced as greatly the classification of Brachiopods as of earthquakes. He, moreover, as the author of *Die Zukunft des Goldes*, has taken rank as one of the ablest champions of bimetallism, and, until he incurred the wrath of anti-Semitic prejudice by a noble appeal for justice to the Jews, he was one of the very foremost of the leaders of the Liberal party in the Austrian Reichstag.

The Exploration of Mount Milanji, Nyassa-land.

A set of papers full of incident and interest was laid before Parliament last June. They relate to the suppression of slave-raiding in Nyassa-land, and tell how Commissioner Johnston, with the help of a few English officers and a handful of Indian sepoys, has been able to check and, for a time, completely paralyse the slave trade along one of the great routes from the interior to the coast, that, namely, which passes across the southern extremity of Lake Nyassa.

Central African expeditions do not always result beneficially to science. Through the Press or from the platform we get a faint idea of treasures still to be found, but nothing more tangible. The story is often one of aimless wanderings and perpetual fighting with the native tribes. The present series of operations, however, forms a pleasant exception. Mr. Alexander Whyte, who is described as a Naturalist and Scientific Horticulturist, was sent out by the British South Africa Company to work under Commissioner Johnston's instructions and thoroughly investigate the Natural History of British Central Africa, and pages 15—19 of the report contain an account of his exploration of the mountain and district of Milanji. "Milanji," he tells us, "is an isolated range of, for the most part, precipitous mountains, the main mass forming a huge natural fortress of weather-worn precipices, or very steep rocky ascents, sparsely clothed with vegetation. Many of its gullies and ravines are well wooded, and in some of them fine examples of grand African virgin forest are met with." It is about one hundred miles S.S.E. of Lake Nyassa and fifty miles east of the Shiré River. The ascent from the south-east face led at first over steep, grassy hills, down precipitous gorges and across rocky streams, then, after a more difficult climb, an interesting wooded gorge was reached where rocks and tree-roots afforded better foothold. Here, too, the plants of the lower slope gave way to more temperate forms, such as brambles and well-known papilionaceous and composite types. Ferns increased in number, together with selaginellas, lycopods, mosses, and lichens. What seemed an endless ladder of roots and rocks was at last succeeded by a dense thicket of bamboo; next a barrier of precipitous cliffs was surmounted by the aid of tufts of tussock-grass, and, finally, an hour's climb up a steep, grassy glen brought the party to the crest of the highest ridge. The view and the climate fully repaid the climb. The air was cool and bracing. Four thousand feet below, the thermometer had stood at 106° F. in the shade, while here they revelled in a clear dry atmosphere of 60° F. "Looking westward, we saw mapped out beneath us the plateau or basin of Milanji, with its rolling hills of grassy sward, its clearly-defined belts of dark-green forest, and its numerous ravines and rivulets, all shaping their course towards the principal valley through which the Lutshenya, the

principal stream, flows." The main peaks of the mountain rise directly from this river valley, which runs parallel with the southern base; the height of one of the two peaks has been calculated at 9,300 feet above sea-level. "Across the table-land in the distance is the somewhat isolated and precipitous Tshambi range, which, with its own smaller plateau, is separated from the Milanji table-land by the rocky valley and gorge of the Likabula River. To the front and left we have a continuation of the rolling grassy hills which encircle the plateau and are capped with rugged cliffs of scarped granite and gneiss rocks." Two weeks were spent on the plateau, working from three sites separated by from five to seven miles.

The flora proved a most interesting one. The most striking botanical feature was a fine cypress-like Conifer, apparently a new species of *Widringtonia*. This, however, seems to be rapidly disappearing before the annual bush fires. It was deplorable to witness the devastating effects of these fires, which, in the dry months of August and Sepember, originating from the villages on the lower slopes, creep up the precipitous cliffs from tuft to tuft of grass till the plateau is reached. Spreading quickly over the grassy table-land, the destroyer makes its way along the edges of the forests, burning or scorching the outside trees and killing the seedlings, sometimes even penetrating the interior where hundreds of giant trees lay prostrate and piled on each other in all stages of destruction. One of these, by no means the largest, measured 140 feet in length, with a clear straight stem of 90 feet, and $5\frac{1}{2}$ feet in diameter at 6 feet from the base. We are glad to hear that Commissioner Johnston has taken steps for their preservation.

Tree-ferns attain a great size in the damp shady forests of the plateau, while favoured nooks showed a gorgeous display of white and yellow helichrysums, purple and blue orchids and irises, and snow-white anemones. Several thousand specimens were collected and dried.

The fauna of the plateau and mountain-top, and of the surrounding plains, proved as interesting as the flora. It was, of course, more difficult to obtain representative collections of animals, birds, &c., in so short a time. The birds found in the "terai" of the mountain were largely identical with the species of the Zomba district (Zomba, in the Shiré Highlands, is the place of residence of the Commissioner); but those collected on and above the plateau differed widely, and many quite new forms were met with. The almost total absence of raptorial birds on the mountain was very noticeable, but Mr. Whyte suggests an explanation in the comparative paucity of animal life at so great an elevation, and the abundance of prey on the surrounding extensive plains. The birds on the plateau are, with very few exceptions, arboreal in habit, finding a safe retreat amid the dense foliage of the forest. Very few were found on the grass-lands—a small, dark brown quail, a pipit, two grass warblers, a snipe (rare), and the ubiqui-

tous great-billed raven. Thrushes, bulbuls, flycatchers, warblers, finches, and honeybirds made the woods lively with their varied notes. Altogether about two hundred bird-skins were collected, some of which may be new to science or of interest in connection with geographical distribution.

Comparatively few mammals were found. Of beasts of prey, the leopard, spotted hyena, serval, and an ichneumon were seen, while four species of rat were caught on the "terai," and three on the plateau. An interesting little antelope—probably a *Neotragus*—is confined to the rocky ridges of the higher range; it proved too shy and wary for capture. In the distance "it looked about the size of a half-grown goat, of a dark brown colour, with a white patch under the tail." Lions, which are common on the plains, do not apparently frequent the plateau. Snakes, of a good many species, were found both on the plains and plateau.

The soil of the grass-lands, owing to the impoverishing effects of periodical fires, is unsuitable for cultivation, but admirably fitted for sheep or cattle runs, the prevailing grasses being more tender and nutritious than the strong, rank herbage of the plains. The climate and the well-watered valleys also favour stock-rearing. As regards the soil around the base, Mr. Whyte considers it well suited for cultivation of coffee, while cacao would do well in many of the sheltered ravines along the river courses. The land, generally, is very productive; some gardens were seen from which the natives had reaped three crops of Indian corn and sorghum within twelve months. The rainfall is probably only from 60 to 70 inches, but apparently better distributed than at Zomba and other ranges in the Shiré highlands, for during one year's experience there had not been a single month without rain. Tobacco and cotton could be grown to an unlimited extent on the plains.

Viewed as a sanatorium, Milanji, according to Mr. Whyte contrasts favourably with some of the Indian and Ceylon hill-stations. The year is fairly equally divided into wet and dry months. During the former—November to May—it must be as miserable as all tropical mountain sanatoria. The other six months are fine, clear, and bracing, the thermometer sometimes falling at night below freezing point in May, June, and July. Of course, the great drawback in this respect is the practical impossibility of reaching it, but Mr. Whyte considers that a road of fairly good gradient might be made *viâ* the Lutshenya Valley; and this would also serve for transport of the valuable cypress timber now rotting in the forests.

Naturalists will look forward with interest to the detailed report on the collections obtained by the expedition, now being investigated by the officers of the British Museum. The Botanical results will be communicated next session to the Linnean Society, and a Zoological Report will be contributed to the Zoological Society's Proceedings.

The Age of the Earth.

About the time when Sir Archibald Geikie's Presidential Address to the British Association was exciting interest in the ever-recurring question of the Age of the Earth, the Physical Society of Glasgow University issued an admirable address by Professor John Young on the same subject. Both authors approached the problem from the point of view of the Naturalist as opposed to that of the Physicist; and both alluded to the circumstance that, whereas the "mathematical mill" will satisfactorily "grind whatever is put into it," no certainty can be reached so long as the initial data are merely plausible assumptions. While, however, Sir Archibald Geikie finds it impossible to accept Lord Kelvin's low estimate of geological time, the Physicist's colleague at Glasgow points out that there are many possibilities of grave error in the biological and geological arguments which may, after all, account for the difference of the result based upon the two lines of reasoning. The enormous period of time required for the evolution of the present world of life according to current views of Biology is, Professor Young thinks, a hopeless exaggeration due to our ignorance of the production of new organisms by some mysterious sudden change of the old ones. Following Eimer, he especially cites the case of the existing Axolotl, which remains a gill-breather so long as it can live in water, but suddenly changes into the lung-breathing *Amblystoma* as soon as water is inaccessible. He also alludes to the crustacean *Artemia*, a denizen of brine springs, which undergoes specific, even generic variation, according to the salinity of the water. Indeed, the Professor goes still further and declares himself in favour of the heresy, that the same type of animal may be simultaneously evolved from a distinct set of ancestors in two remote regions, thus doing away with the necessity of assuming great periods of time for the accomplishment of certain migrations. The oft-quoted arguments of the geologist also fare badly in Professor Young's hands. He points out that the rate of denudation of a country depends so much upon varying conditions as to render even plausible results impossible; and when a geologist speaks of strata miles in thickness, the Professor adds a word of caution that we fear has not been sufficiently attended to by those who attempt to estimate the length of geological periods. Professor Young indicates the great difficulty of determining the contemporaneity of deposits formed under different conditions, and he thinks that the estimated thickness of many geological formations has been greatly exaggerated by piling contemporaneous lake deposits, shore deposits, and deep-water deposits one upon the other. Finally, we are warned that some of our cherished views as to the antiquity of man may be the unfortunate outcome of the independence of the several lines of research, which have never been carefully correlated: it may be that "the net result is the expression not of conviction but of mutual courtesy."

No doubt there is much truth in all these considerations, and it is well that a Naturalist who can speak with authority should occasionally challenge the dogmas that are prone otherwise, by frequent repetition, to gain currency as well-ascertained facts. With regard to the age of the earth in terms of years, we are bound to admit that the solution of the problem is almost as far off as ever; and if a solution should eventually be found, we are convinced that the Astronomer, and not the Geologist or Biologist, will have the satisfaction of its discovery.

The Study of Variations in Animals.

Serious warnings that our philosophy may be attempting to advance too rapidly are not only conspicuous in the latest discussions of such wide problems as those to which we have just alluded. Recent literature contains many new facts that are likely to revolutionise current views on several of the minor questions of organic evolution; and if Mr. W. Bateson's researches on the variation of multiple parts in animals prove to yield something more than a phantom as a result, Biologists must cease to believe in some of their most fundamental tenets. It may be a satisfaction to learn that the five-fingered limb of a lung-breather is not a direct modification of the primitive fin of a gill-breather, but has merely arisen by the independent segmentation of the same kind of tissue that gave rise to the fins. It may also be a satisfaction to the student of teeth in the mammalia to know that the various modifications really cannot be regarded as all derived from one fundamental fixed type. It is somewhat startling, however, to read Mr. Bateson's preliminary announcement of his conclusions (*Proc. Zool. Soc.*, 1892, p. 114), which are shortly to be elaborated in a special treatise, and the following paragraph is especially noteworthy:—

"The received view of homology supposes that a varying form is derived from the normal much as a man might make a wax model of the variety from a wax model of the type, by small additions to, and subtractions from, the several parts. This may, to our imaginations, seem perhaps the readiest way by which to make the varying form if we were asked to do it; but the natural process differs in one great essential from this. For in nature the body of the varying form has never *been* the body of its parent and is not formed by a plastic operation from it; but in each case the body of the offspring is made again from the beginning, just as if the wax model had gone back into the melting pot before the new model was begun."

Our present methods of Comparative Anatomy, according to Mr. Bateson, "though they are ingenious, logical, and orderly, are orderly because they are made without regard to the ways of Variation, which is arbitrary and capricious and follows no order that we have yet devised."

The Evolution of the Mammalia.

Comparative Anatomy, however, when tempered with the results of Embryology and Palæontology, cannot be misleading to any great extent; and, notwithstanding Mr. Bateson's conclusions, Naturalists will still retain confidence in such researches as those of the Professor of Phylogeny at Jena, to whose discoveries in the evolution of the Mammalia we have often alluded. Dr. Kükenthal has just issued (*Biol. Centralblatt*, vol. xii., pp. 400–413, 1892) another of his striking addresses, which, though partly based upon published papers, contains much that is well-expressed and novel. The dentition, as usual, is mainly employed to determine the stage of evolution; and it is pointed out that in the three groups of Theromorous Reptilia, Marsupials, and Placental Mammals, the teeth become independently specialised in the same directions. In conclusion, the Professor believes that none of the known mammals can have been directly descended from any of the known Theromorous reptiles; he considers, on the other hand, that the ancestors of both these groups must be sought in the Palæozoic period.

Such a conclusion is interesting and perhaps justified, but at any rate it is provoking. The "ancestors" of certain well-marked groups of animals recede further and further into remote antiquity, just in proportion to the amount of research devoted to them. The reputed "ancestors" of one author become the "parallel line" of the next; and the geologist, who is supposed to unearth the desired pedigree, begins to despair of following a kind of will-o'-the-wisp. We cannot help thinking that someday a serious misapprehension will be discovered in the prevailing ideas of animal pedigrees. The "imperfection of the geological record" has its limits, and will not account for everything.

The Evolution of Man.

However unsuccessful the geologist may have been in unearthing the very earliest mammals, he has certainly done well in discovering the successive stages through which many groups have recently passed; and these processes of evolution have operated so rapidly in such modern times, that it is difficult to believe they have now entirely ceased. In many cases it is difficult to trace the directions in which they are advancing, on account of the paucity of material examined; but in the case of man, the innumerable variations in anatomy that are constantly being recorded afford a mass of very tangible evidence.

This evidence has lately been collected by Professor H. F. Osborn in the Cartwright Lectures for 1892, delivered before the Alumni of the College of Physicians and Surgeons of New York, and now printed in the *American Naturalist*, commencing June, 1892. It is certainly very striking when treated in Professor Osborn's philosophical

manner; and few will doubt that the human frame is still undergoing marked evolution.

In the first place, the Professor well points out that, physically speaking, man is far from being a pre-eminent creature:—

"From the typical mammalian standpoint, man is a degenerate animal; his senses are inferior in acuteness; his upright position, while giving him a superior aspect, entails many disadvantages, as recently enumerated by Clevenger, for the body is not fully adapted to it; his feet are not superior to those of many Lower Eocene plantigrades; his teeth are mechanically far inferior to those of the domestic cat. In fact, if an unbiased comparative anatomist should reach this planet from Mars he could only pass favourable comment upon the perfection of the hand and the massive brain! Holding these trumps, man has been and now is discarding many useful structures. I refer especially to civilised man, who is more prodigal with his inheritance than the savage. By virtue of the hand and brain he is, nevertheless, the best adapted and most cosmopolitan vertebrate."

Professor Osborn's review must be carefully read in detail to be appreciated. The lower jaw of man, we are told, is distinctly becoming reduced in size; the "wisdom tooth" is disappearing, and the life of civilisation is tending to a degeneracy of all the teeth. There is a frequent reduction in the number of the ribs, resulting in a shortening of the chest, probably correlated with man's upright position. The arm is becoming more adapted to freedom of motion. Owing to our present habits of walking, the great toe of the foot is increasing, while the little toe is diminishing; and in many human skeletons the two end-joints of the little toe are fused together. In many of the muscles, too, there is evidence of change; and there is no doubt that the man of the near future will differ much in his frame from the civilised man of the present.

The Ear.

Another welcome contribution reaches us from America in the form of an elaborate memoir on the organ of hearing in vertebrate animals (H. Ayers, *Journ. Morphology*, vol. vi., pp. 1–360, pls. i.–xii.). The subject is treated from the point of view both of Comparative Anatomy and Embryology; and if Dr. Ayers' results are confirmed, they will make an important advance in our knowledge of the meaning of the various parts of the ear.

So many new features are discussed, that it is impossible here to indicate more than the principal outcome of the investigation. It is now generally admitted that the ear originates in the lowest vertebrates by a modification of the superficial nervous slime-canal system, which gives rise to the conspicuous and familiar "lateral line" in fishes. Dr. Ayers, on the present occasion, elaborates this idea. It is well-demonstrated that the ear is supplied by the same nerves that are connected with the superficial sense-organs of the "lateral

line" in fishes. Not only so, but it is pointed out that the superficial slime-canal itself actually remains in distorted fragments in the perfected ear. The semi-circular canals, according to Dr. Ayers, are "simply remnants of the canal system of the surface," and "have no active part in the auditory function. They serve merely as chambers to hold the liquid necessary to float the auditory sensory hairs." The "otoliths in the ear chambers are to be considered as essentially foreign bodies . . . the result of the secretive action of the ectoderm cells, which in ancestral forms produce the surface scales. The higher we rise in the vertebrate series, and also the greater the auditory sensitiveness, the fewer the otoliths found in the ear. . . ." "The functions of the ampullar sense organs and their connected canals can hardly be different from the sense organs of the lateral line, except in refinement of function; *i.e.*, the difference can only be one of degree, and not of kind." Moreover, "in Elasmobranchs, the structural connection between the ear organs and the surface canal organs is for a long time maintained."

In conclusion, Dr. Ayers maintains that the organ of hearing in Invertebrata is totally distinct from that of the Vertebrata, and he combats the views of Retzius, who supposes that there is a fundamental difference between the ear of a lamprey and that of an ordinary fish. Dr. Beard is also much criticised.

Vanilla Disease in the Seychelles.

The large and widely-distributed family of Orchids, though much in favour with horticulturists, is one of the most useless from an economic point of view. One genus forms an exception—practically the only exception; it has given its name to the product for which it is known. The genus *Vanilla* contains about twenty species widely distributed throughout the tropics. The plants are terrestrial and herbaceous, but tall, sturdy climbers, with leathery or fleshy leaves, and bear racemes or spikes of large though usually dull-coloured flowers. The fruit or capsule, which is often elongated and fleshy, with the appearance of a pod, is very generally used for flavouring chocolate, liqueurs, &c. The best product is obtained from a Mexican species, *Vanilla planifolia*, which is extensively cultivated in other parts of the tropics.

In recent years, the cultivation of *Vanilla* has been taken up in the Seychelles, and a considerable industry established. Unfortunately, however, this has been threatened by the appearance of a disease which, in some areas, has led to much loss, as it affects the pods, causing them to turn black, and fall before ripening. In 1887, material sent from the islands to Kew was examined by Mr. George Massee, but owing to bad packing proved useless for investigation. In February, 1890, a second consignment was received representing every stage of the disease, and conclusively showing that it is caused

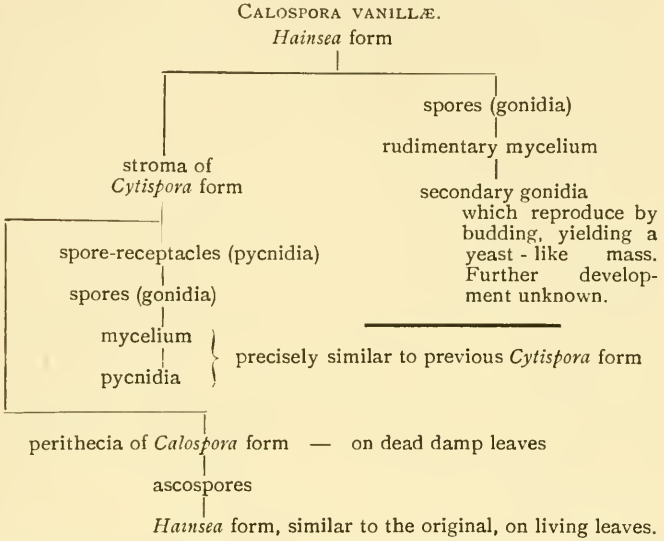
by a microscopic fungus which Mr. Massee calls *Calospora vanillæ*. In the *Kew Bulletin* for May and June last he gives an account of his examination and the results obtained. The latter are also interesting from a purely scientific point of view in elucidating the life-history of a fungus, only one stage of which was previously known. The study of the life-history also suggests a remedy as far as this can be possible without a thorough investigation of all the circumstances conducted on the spot.

Living leaves attacked by the disease show minute amber-coloured pustules springing in small groups from slightly discoloured patches, which are most abundant on the upper leaf-surface. These pustules consist of masses of spores or *gonidia*, evidently belonging to the genus *Hainsea*, borne on a mycelium which is rapidly destroying the leaf-tissue; the fungus proves to be identical with the *Glœosporium vanillæ* received in 1886 on specimens of the same species of Vanilla from Antigua, and described by Messrs. Cooke and Massee, who did not recognise the connection between the several forms, though all were present. These gonidia germinated when sown in a nutrient solution producing a mycelium and secondary gonidia; the latter when sown reproduced enormously by budding, yielding a mass of yeast-like cells, but were never seen to emit a germ-tube. No result was obtained when living Vanilla leaves were infected with them, and it is unknown to what they eventually give rise. When the *Hainsea* fungus is fully developed the leaf has become yellow as the result of its ravages, and is evidently dying. At this stage very minute clusters of black points can be seen beneath the cuticle. These on examination were recognised as black-walled spore-receptacles or *pycnidia* belonging to the form-genus *Cytispora*.

The pycnidia were partly sunk in a common cushion or stroma formed from a mycelium quite indistinguishable from that of the *Hainsea*, which preceded it. Their gonidia ooze out through the ruptured cuticle like "mucilaginous tendrils" forming pale yellow waxy masses on the leaf-surface. When allowed to germinate on the moist epidermis of an uninjured Vanilla leaf they did not enter the leaf, the germ-tube being apparently unable either to pierce the epidermis or get in by the stomata; but when placed on a *dying* leaf after commencement of disintegration they penetrated and spread through it, producing pycnidia in abundance. This shows that the *Cytispora* is a saprophyte and will not reproduce itself on the uninjured living leaf. By the time the *Cytispora* has completed its development the leaf has become dry and shrivelled, the whole of the sap and protoplasm having disappeared. No further development occurs as long as it remains dry, but if kept damp the stromata produce *perithecia* from the central portion. Though their development was carefully watched, no trace of an ascogonium or "Woronin's hypha" was seen indicative of a sexual process. The perithecia contain *asci*, each with eight *ascospores*, and exactly resemble those

of the genus *Calospora*. When placed on the uninjured upper surface of the leaves the ascospores germinate, the germ-tube enters the leaf through a stoma, and the mycelium develops in the tissues, producing a *Hainsea* form with amber-coloured gonidia pustules, in every way resembling the original.

We may represent the life-history in tabular form thus :—


CALOSPORA VANILLÆ.
Hainsea form
spores (gonidia)
rudimentary mycelium
secondary gonidia which reproduce by budding, yielding a yeast-like mass. Further development unknown.
stroma of Cytispora form
spore-receptacles (pycnidia)
spores (gonidia)
mycelium
pycnidia
precisely similar to previous Cytispora form
perithecia of Calospora form — on dead damp leaves
ascospores
Hainsea form, similar to the original, on living leaves.

The death of the plant is caused by the earliest or *Hainsea* form of this pleomorphic fungus; the two other forms appear only on the dead or dying leaf. Destruction of the leaf necessarily affects the whole plant, and symptoms of the disease may appear in the blackening tips of the "pods" before the leaf itself is visibly affected, the disorganisation of its internal tissues through which the mycelium is spreading preventing the proper performance of its functions and seriously affecting the food-supply to all parts, and especially the rapidly-developing fruits. A possible remedy, Mr. Massee suggests, may be found in the fact, brought out in this examination, that the *Hainsea* form is apparently unable to reproduce itself directly on other living leaves, but is only developed from *Calospora* spores which are themselves produced *only on damp dead leaves*. Hence it follows that the continuance of the disease depends on the presence of diseased dead leaves, and "if all fading and dead leaves are destroyed by burning, the fungus pest would soon disappear, or at least be so reduced as to produce no appreciable damage." It is probable that impaired vitality of the plants would favour the spread of the disease, as weakly specimens would almost certainly be more susceptible to attack. This idea is supported by the fact that the disease was most prevalent in flat damp places with insufficient drainage.

It is interesting to read that since the investigation just described, all the stages of the disease have been followed on cultivated orchids of the genera *Oncidium* and *Dendrobium*.

THE habits of the Sand Wasp (*Ammophila subulosa*) form the subject of a paper by Dr. P. Marchal in the *Archives de Zoologie Expèrimentale et Génèrale* (vol. x., p. 23). The female, as has long been known, captures caterpillars which, after paralysing with her sting, she carries to the nest to furnish food for the grubs. The details have been carefully observed by Dr. Marchal, who states that the wasp inserts her sting at intervals along the median ventral region of the caterpillar, so as to paralyse the ganglia. He proved, by subcutaneous injection, that *Ammophila* does not bite her victim with her mandibles, as *Cerceris* and *Philanthus* do to the bees and other insects on which they prey, feeding themselves on the juices of the body and the contents of the alimentary canal. However, *Ammophila* was seen to bathe her tongue in the "salad" disgorged by the caterpillar, and, therefore, her instinct cannot be regarded as entirely due to a disinterested care for her offspring. In wasps which do not so carefully pick out the nervous centre for attack, the poison of the sting is stronger than in *Ammophila*, and in others with that instinct; it appears, therefore, that the venom becomes weakened as the instinct is perfected. Dr. Marchal has no doubt that the very highly perfected method of attack practised by *Ammophila* is developed from such a comparatively clumsy mode of dispatch as is adopted by *Philanthus* and *Cerceris*.

FURTHER light has been thrown on the question of the primitive insect-larva, discussed in an article in NATURAL SCIENCE for June. Professor A. S. Packard (*Zool. Anz.*, vol. xv., p. 229) calls attention to the caterpillars of the American moths, *Lagoa* and *Chrysopyga*, which, alone among the Lepidoptera, have rudimentary pro-legs on the second and seventh abdominal segments. The homology of these with the normal pro-legs seems indisputable, and, according to Professor Packard, shows that this larva "is a survivor of an ancient and very generalised type, and represents, as no other known caterpillar, the polypodous ancestor of all Lepidoptera."

A REMARKABLE habit of certain grasshoppers is recorded by Dr. F. Werner (*Zool. Anz.*, vol. xv., p. 58). Some when captured, others if kept for any length of time in captivity, bit off and sometimes devoured portions of their legs. The insects observed were of the genera *Ephippigera*, *Barbitistes*, *Saga*, and *Locusta*.

IN the last number of the *Annals and Magazine Nat. Hist.* (ser. 6, vol. x., pp. 121–128, pl. xii., 1892), Mr. Edgar A. Smith has an interesting paper on the shells of Victoria Nyanza. These mollusca closely resemble those of the basin of the Nile, and no such remarkable forms as occur in Lake Tanganyika have yet been met with. Victoria Nyanza does not possess a specialised molluscan fauna like

that lake; and of the 28 species enumerated by Mr. Smith, 12 have been recorded from the Nile or one or other of the great lakes, while of the remaining 16 so-called species very close representatives occur in other lakes and rivers of Central Africa.

Mr. R. L. Garner, whose popular work on *The Speech of Monkeys* has just appeared, left England last month for Western Africa, where he hopes to preserve by phonograph the "speech" of the apes in their native haunts. As most readers are doubtless aware, Mr. Garner proposes to establish himself in a steel cage in the forest and attempt not merely to record the vocal sounds but also to photograph any of the quadrumana he may be able to attract to his place of observation. A large audience assembled in the Anthropological Section of the British Association at Edinburgh to hear Mr. Garner's promised account of his researches; but through some misunderstanding with the officers—not altogether creditable, according to our information—the Association's distinguished guest was unable to appear.

In a lecture delivered recently before the Geologists' Association, Mr. Rudler gave an interesting account of the pioneers of British Geology. He pointed out, as was done years ago by Buckland and Conybeare, that to a Welshman is due the credit of first showing that strata succeed one another in a regular manner through large tracts of country. This "Patriarch" of Geology, George Owen of Henllys, Lord of Kemes, was born in 1552 and died in 1613; he left a valuable manuscript work in which he "traced with much accuracy the direction and extent of the strata of coal and the limestone which accompanies them through the whole of South Wales, and pointed out the connections of this tract with similar districts in Gloucestershire and Somersetshire." Owen's original work, "The Description of Penbrokeshire" (so spelt), has lately been published by the Society of Cymmrodorion.

About two years ago a "Coal Search Committee" was founded with much *éclat* for the "systematic investigation of the deep-seated geology of the South-East of England." An imposing list of names was appended to the original circular, which was signed by Mr. James T. Day, and a meeting was presided over (according to the circular) by the Right Hon. the Lord Mayor of the period, supported by the "Coal Search Committee." It would be interesting to know what has been done in the matter, or what has been done with the funds which were subscribed by the over-cautious in the early days of the boom. Perhaps some of our readers can enlighten us?

In the *Annals of Botany* for July, Professor J. R. Green gives an account of his investigations on the digestive ferment of the Kachree

gourd, *Cucumis utilissimus*. Ferments capable of digesting proteid matter have already been shown to exist in various parts of plants—the insectivorous leaves of *Drosera*, the pitchers of *Nepenthes*, the germinating seeds of the Lupin, and the Castor Oil plant, the fruit of the Papau, and the latex of the Fig. The Kachree gourd has, in India, the reputation of possessing the same properties as the two last-mentioned plants, and Professor Green shows conclusively that it contains a ferment which is very similar to papäin, the vegetable trypsin of the Papau. The expressed juice of the central pulp, and a watery extract of the pulp just beneath the rind, were both found capable of dissolving coagulated egg albumen. As the disappearance of the albumen was much more rapid when a 3 per cent. salt solution was used, it is concluded that the ferment is either a globulin in nature, or, more probably, associated with a globulin constituent in the plant cells. The digestive power of the watery extract would be explained by the presence of inorganic salts in the plant, which would assist solution on addition of water. The ferment resembles papäin in working best in a slightly alkaline solution, less readily in a neutral one, and least of all in the presence of an acid. Like papäin, too, it is allied to the trypsin rather than the pepsin of the animal organism, as it effects a very complete decomposition of the albumen, giving rise to peptone and later to leucin.

According to the *Kew Bulletin* for May and June, the consumption of Paraguay tea or *Maté* in South America, rivals that of the Eastern herb in the Old World. It is extensively used by the whole population, "upwards of five million pounds" being exported from Paraguay alone. Its preparation is apparently somewhat rougher than the Chinese or Indian method; the leaves are scorched and dried on the branches brought in by the collectors, and then beaten, separated, coarsely ground by rude mills, and packed in skins and leather bags. In use, the leaves are infused in small tea-pots, and the liquid sucked up through a tube with wire network or perforations at the bottom. The active principle in the leaves is caffein, an alkaloid identical with that found in the ordinary tea and coffee. There is, moreover, a large amount of tannic acid, according to Dr. T. C. Charles, who also tells us in a note in the *British Medical Journal* of July, 1890, that *Maté* is closely allied to coffee, but is slower in yielding up its nutritious principles to boiling water. To the first-mentioned periodical Mr. N. E. Brown supplies a note on the botany of Paraguay tea, which is apparently obtained from several species of *Ilex*—a genus known to us in the common holly (*Ilex aquifolium*)—and a species of *Symplocus* and *Elæodendron*. The most important is the *Ilex paraguariensis* of St. Hilaire, of which there are several varieties, probably yielding different qualities of tea.

I.

The Foundations of Science.

IT is most reasonable that a publication devoted to "Natural Science" should accord some of its space to the consideration of the foundation upon which the whole of that science necessarily reposes. I therefore hail with pleasure Mr. Ryle's defence of Professor Pearson's "Grammar," although I regard that defence as mistaken and exaggerated, and the work it defends (which I have carefully read) a singularly unreasonable one.

Everyone knows that "Idealism" is a metaphysical system, and Mr. Ryle tells us expressly that "Professor Pearson adopts a philosophical position which may be fairly described as thoroughly idealistic." This is true. I have elsewhere said[1] that "he is an idealist of a kind." Nevertheless, if there is one thing Professor Pearson repudiates and blames again and again, it is philosophy. Any defence must be a mistaken one which speaks eulogistically of an author being that which the man himself emphatically denies that he is.

My object, however, in this short paper, is not to engage in a triangular duel, but simply to point out what is that basis upon which all "Natural Science" necessarily reposes.

Professors Ryle and Pearson, faithful to the traditions of their school, declare that we can know nothing but feelings—"sense-impressions and sense-impresses"—received, associated, remembered, &c., and so issuing in "concepts." The ultimate appeal of this school is to "the senses," and it is therefore justly spoken of as "*Sensism*."

To me it is clear that not only every conception, but also every human perception, contains what is altogether beyond sense,[2] and that our ultimate appeal is not to the senses but the intellect. This system may, therefore, rightly be termed "*Intellectualism*."

It is time that the real basis of science should be generally understood; that men should no longer be bound by the fetters of tradition and called upon to bow down "lowly and reverently" to those who are represented to them as "their betters" by the voice of "authority." It is time for them to use freely their own reason, and while unflinchingly and fearlessly recognising what they do not and cannot know,

[1] See *Nature*, July 21, 1892, p. 267.

[2] See *The Origin of Human Reason*, p. 280.

to also recognise what they do know, what they must know if they know anything, and what is that solid foundation on which alone they can securely rear any self-consistent and stable fabric of knowledge.

It would occupy too much space were I to enter upon any defence of the evident and fundamental truths of science, nor is it needful, as I have done so elsewhere both briefly[3] and also more at length.[4]

There are four essential truths concerning what we know :—

1. We can know not only our actions, sensations, imaginations, reminiscences, emotions, perceptions, and conceptions, but also our own substantial and continuous personal existence.[5]

2. We know, through memory, real existences external to all our present experience, *i.e.*, we have the faculty of knowing what, *pace* Mr. Ryle, is objective truth.[6]

3. We know that there are truths which are true not only here and now, but which must be true ever and always—as that " nothing can at the same time both be and not be."[7] We know this to be not merely a law or condition of our own mind, but that it is true of all " things-in-themselves " apart from the existence of any or every mind.

4. We know that, if certain principles be true, then whatever logically follows from them must be true likewise.[8]

Unless we know these things science is logically impossible, and any scientific man who denies them either deceives himself or seeks to deceive others. Without them it is impossible to have—what alone should satisfy a man of science—a system of knowledge complete and harmonious, satisfactory and stable, from its deepest foundation to its loftiest pinnacle.

If these truths are *really* doubted or denied by anyone, he must fall into a state of mental paralysis and intellectual inanition; though, of course, men may really hold and act on them, while supposing or pretending they do nothing of the kind,[9] there being so much both of stupidity and dishonesty in the world.

Really, however, to believe that any appeal lies to sensation from that which the intellect declares to be evidently true, is a self-stultifying belief. How, save by the intellect, can we know that we possess a sensitive faculty, or that we have " sense-impressions " and " sense-impresses " at all ?

But if we know so much, *i.e.*, if we know the four truths above enumerated—what difficulty is there in recognising that we may know and do know yet more ?

[3] See *Nature*, Nov. 19, 1891, pp. 60–62; and Nov. 26, pp. 82–85; also Supplementary Notes in the numbers for Jan. 7, 1892, p. 222, and Feb. 11, p. 343.

[4] See *On Truth, a Systematic Inquiry*, Kegan Paul, Trench & Co., 1889.

[5] See *On Truth*, pp. 3–28.

[6] *Op. cit.*, pp. 29–37.

[7] *Op. cit.*, pp. 39–46.

[8] *Op. cit.*, pp. 53–59.

[9] *Op. cit.*, pp. 134, 135.

For myself, I am absolutely convinced (after wandering for years in the mazes of Idealism) that we do know vastly more. The "sense-impressions" and "sense-impresses" arouse the intellect to much more than their own recognition, association, &c., &c.; they arouse it to the direct perception of external objects—of things-in-themselves—though not, of course, to an exhaustive knowledge of them. It is evident to me that, in Mr. Ryle's words, "they have a 'reality' not conditioned by the nature of my faculties for knowing them."

That the knowledge of the numerical relation of things—that the facts concerning their number (*e.g.*, that oranges on a plate are three and not five) are real and absolute, and not conditioned by my faculties, is, for me, unquestionable. I am also convinced that I have an intuition of the extension of extended things. Dr. Johnson's often-ridiculed kick at a stone by way of refuting Idealism, was most probably a mute affirmation of his consciousness of this intuition, and his recognition that a synthesis of "sensations" and "sense-impresses" is quite inadequate to account for our perception of an external body or of our own body.[10]

But though I am convinced I know the before-enumerated four truths, and the existence, figure, and numerical relations of extended things, I do not know how I know such things. This, however, is an ignorance of small consequence, and an analogous ignorance is absolutely inevitable, whatever theory of knowledge or philosophical system we may adopt.

Messrs. Ryle and Pearson, and their masters and pastors—*i.e.*, the "sensists"—are in the very same difficulty with the Intellectualists. How it comes about that their organisation is endowed with the power of having "sense-impressions" and "sense-impresses"—how the stimulation of a nerve and brain can result in a feeling—it is absolutely impossible for them to say. They do not, however, on this account doubt their possession of them, or the truth of their persuasion to that effect. Neither does the inability of the Intellectualist to say how it comes about that on the occurrence of the necessary conditions he perceives a material body, a thought, or an abstract necessary truth, in the least cause him to doubt a thing which is to him evidently true. Our feelings are but the *means*, not the *object* of perception.[11]

However great our knowledge, we must, unless we have recourse to a *regressus ad infinitum*, always arrive at ultimate cognitions—that is, at facts we cannot explain; and it is no greater difficulty or absurdity to recognise that we have "intellectual perceptions" which are ultimate and inexplicable, than that we have "feelings" which are ultimate and inexplicable; moreover, there cannot possibly be any further test of truths so self-evident.[12] Ultimate truths must contain their own evidence.

[10] As to this consult *On Truth*, pp. 90–96.

[11] *Op. cit.*, pp. 90 and 91.

[12] *Op. cit.*, pp. 10–14.

Mr. Ryle, in speaking of Space and Time as represented in the Grammar, says that what Mr. Pearson "has disputed is the conventional, or uncritical view, that takes space and time for 'things,' instead of taking them for an order of things." But, in fact, they are neither the one nor the other. One "order of things" is Extension, that is, many things are extended, and extension is an abstract idea and term denoting the extended nature of such things, which nature is a reality and real condition of the things referred to. Space, however, is not extension, but is altogether ideal, an abstraction from abstractions, and is nothing but a conception of the extension of all extended things. It is the abstract idea of their common extension and mutual exclusion.[13] Similarly, succession is another "order of things," *i.e.*, many things succeed one after the other. Succession is an abstract idea and term denoting the succession of things which succeed. It is, therefore, real as a quality of succeeding things. But Time is not succession—it is altogether ideal, and again (like Time) is an abstraction from abstractions, and nothing but a conception of the succession of all succeeding things.[14]

This comprehension of the meaning of the terms time and space, accompanied with a recognition that we can intue succession as well as extension, and that the four truths earlier enumerated are true, furnishes us with a secure basis upon which the whole series of sciences, from Mathematics to Philosophy, can progress in harmonious self-consistency.

But there is another truth, the clear perception of which has a special relation to that department of "Natural Science" which we distinguish as Biology. For we know most intimately by and in our own consciousness, something—our own intellect—which exists continuously, which is conscious of successive objects and events, and which, itself transcending them, can recognise them as forming a series which it can contemplate as a whole or in parts, and in different orders, according as may be desired. This power or principle it also knows with perfect certainty can and does know itself, is aware of the kinds and directions of its activities, and can regard them as a whole, or in groups, or singly. It can, it well knows, perceive its own states, both passive and active, and also external objects and events, and can compare the relations between them, returning upon itself at will along different lines of thought, and also setting forth in various directions at will. Such a power, aware of all these things, and present to them all, cannot itself be multitudinous, but must be as much a unity as anything we can think of. It is also evident and certain, therefore, that this principle is neither a material substance nor a physical force, but presents the greatest contrast to both, and the greatest contrast to what is material is what is immaterial. Also, since each man knows that it

[13] *Op. cit.*, pp. 407–409. [14] *Op. cit.*, pp. 409–410.

is he who not only thinks but also feels, he knows, if he reflects upon it, that his body and this thinking principle are, to his experience, one unity.

Therefore, each of us is a unity with two sets of faculties, material and mechanical on the one hand, immaterial and non-mechanical on the other. No certainty we can attain to about any other object can be nearly so certain as this certainty we have about our own being, above all our immaterial being;[15] first its dynamic, immaterial aspect, and secondly its material, mechanical aspect. That each man is a material, definitely organised substance in one unity, with a dynamic, immaterial principle of individuation revealed in consciousness, is the first truth of physical science. It is emphatically the fundamental truth of Biology; for biology deals with living things, and of no living thing can we have so complete a knowledge as of ourself, because it is only to the investigation of our own being and activities that the most direct kind of knowledge—our conscious perceptions—can be directly and immediately applied.

This truth, which a careful examination of our own being reveals to us, has very wide and important results when the phenomena of life, especially animal life, are studied in relation to it. By it, and by it alone, can we obtain any intelligent apprehension of a multitude of physiological phenomena. On this question, however, I do not here propose to dwell, I have only referred to it as a further —as a culminating—instance of the effect which the study of our own intellectual activity must have on our comprehension of the world around us. In the historical order of knowledge—the knowledge of the individual as well as of the race—the study of what is external to us does come, and must come, first. But as we emerge from, and put away, the things of childhood, we have more and more to reverse this process. For any satisfactory comprehension of natural phenomena, and especially for those of living nature, the first and most pressing need now, is an adequate acquaintance with our own higher mental processes. Such investigation is much less easy than is the study of what is external to us, but it is more important because most fruitful. The certain, though gradual, effect of its faithful prosecution will most certainly be the overthrow of "*sensism*," and its replacement by the only system which is satisfactory in its application on all sides, the system which distinctly recognises that the basis of all science must consist of truths recognised by thought as self-evident and necessary—the system of "*Intellectualism*."

St. George Mivart.

[15] *Op. cit.*, pp. 390–391.

II.

Biological Theories.

I.—THE NATURE OF HEREDITY.

THE numerous extant theories of heredity all begin with the assumption that some characters are transmitted from parent to offspring, while they differ as to the "transmissibility" of certain others. In the case of characters which they all assume to be transmitted, they differ largely as to the mechanism of the transmission. Haeckel holds, or at any rate did hold, that the medium of transmission, the carrier of the influence which produced new characters like the old ones, was a kind of molecular movement. Darwin imagined certain particles derived from all parts of the body to serve the same function. Both agreed as to the transmission of some "acquired" characters. Weismann opposes both these views and relies upon a supposed continuity of germ-plasm for the explanation of the transmission of all except "acquired" characters, and of these last he denies the transmissibility. The other theories which are held by modern biologists are all more or less similar to one or other of these two last—some rely upon continuity of something or other, the rest upon the transmission of material particles derived from various parts of parental and ancestral bodies.

I do not propose to weigh afresh the relative claims of these two schools of theorists, nor to ally myself with either school by producing a new theory in conformity with its fundamental principles. Both schools appear to me to be equally wrong, and to have set out with equally false notions as to the nature of the phenomenon they have attempted to explain.

The phenomenon I propose to explain is the relative constancy of specific form in several generations of any species of animal or plant, *i.e.*, the reappearance of certain characters in many individuals belonging to several generations. It is not a transmission of characters from parent to offspring alone, not only the reproduction in the offspring of characters possessed by the parents or more remote ancestors; and this will be shown by the first example I shall bring forward in illustration of my view.

In each successive generation of termites or "white ants," are found certain individuals, called "soldiers," which are blind and wingless, and which are incapable of any reproductive function. They have enormous heads, much larger than any of the other

individuals have, and their jaws are more powerful. The soldiers of one generation are indistinguishable from those of any other; their specific characters are constant. This constancy of specific characters, this reproduction, generation after generation, of individuals indistinguishable from one another, this likeness among individuals of different generations, is a phenomenon belonging to the class I have referred to. Whether it is "heredity" or not all depends upon the definition of the term. If "heredity" means the transmission to the offspring of characters possessed by the parent, then this is not heredity, for the soldier termite is very unlike its parents, and also unlike all its remoter ancestors. All its ancestors, so far as we know, for thousands of generations, have possessed wings and eyes, and the power of reproduction, while none of them have possessed the enormous head and powerful jaws which form a most striking character of the soldier. The soldier is exactly like millions of other individuals from which it is not descended, and which cannot, therefore, have transmitted their characters to it; it is unlike all those which might have been supposed to transmit these characters, but for the fact that they have never possessed them.

This example is not given to disprove any other theory, and I do not think that it would be altogether impossible to explain it on the assumption of pangenesis or of continuous germ-plasm, as satisfactorily as those assumptions explain more commonplace forms of heredity. I give the example merely to aid in the exposition of my own view.

If we examine into the factors which by their co-operation have determined the form and structure and consequent functions of the soldier termite and of its parts, we may recognise among them several classes. Whether the soldier develops from a fertilised egg or not I do not know; we must, therefore, to be on the safe side, assume that it at least may do so. The factors may then be classed as follows:—

1. Those determining the constitution of the ovum at the time of fertilisation.
2. Those determining the constitution of the spermatozoon at the time of fertilisation.
3. Those acting upon the fertilised ovum and the developing soldier from without.

Among the factors determining the constitution of the ovum are to be included the constitution of the female parent. Its structure and consequent functionality—for I include chemical as well as physical constitution in "structure"—have been determined by factors which might be classed in the same way as we have classed those determining the structure of the soldier. The origin of these factors I shall have to discuss in a future paper; for the present, we may take the structure of the parent as our starting point, and similarly also with the male parent. One result of that structure and consequent functionality of the mother is the production of ova. Though all were formed in one ovary, it would not follow that all should be alike,

any more than it follows that all cells of the body are alike because all have arisen from one egg. There may be many kinds of ova produced, and the development of these into insects of at least six different kinds may possibly be due to some initial difference in the ova. The same is true of spermatozoa (*mutatis mutandis*).

Again, the ova may be fertilised at different stages of maturation. There is a difference between a mature and an immature ovum, and there are stages in maturity. The ovum is a living cell, and its constitution, chemical and physical, may vary with its age. The age at which it is fertilised may, therefore, have something to do with its future development. So also with the spermatozoon, and some have held that differences of this kind determine the sex of the offspring in various animals, and especially in mammals.

If, however, two females are exactly alike in every detail—physiologically as well as structurally—then, however many kinds of ova A produces, B will of necessity produce kinds of ova exactly like them: and a similar likeness will also exist between the spermatozoa of two males, M and N, however many kinds there may be of them. If two like ova, derived from A and B respectively, be fertilised by two like spermatozoa, derived from M and N respectively, then the two fertilised eggs so produced will be alike in every minutest detail, physically and chemically, and, therefore, also physiologically.

Of the influences brought to bear upon the fertilised and developing egg, the most important would seem to be temperature, supply of oxygen and of moisture, and quantity and quality of food. We know that the quantity and quality of food supplied to the larva developing from the fertilised egg of a hive bee determine whether that larva shall become a queen or a worker. We do not know this of the termite larva, but it is none the less safe to say that if every influence brought to bear upon each of the two like eggs just spoken of be precisely the same in the two cases then those two like eggs will undergo like changes. Whatever the form and structure of the full-grown insect developing from one of those eggs, that developing from the other must of necessity be like it. The causes are *ex hypothesi* exactly alike, the effects must of necessity be alike also. If one of these two insects be a soldier termite, so must the other be. If one be a butterfly, so must the other be.

The likeness of the influences determining not only the production, but also the *formation*, the determination of every detail of structure, in any two cases, sufficiently explains the likeness of the two individuals. And this is true even when the two individuals are only distantly related to each other, and when neither of them bears any close resemblance either to its parents, or to any of its ancestors.

A particular soldier has no eyes and no wings, because the influences which determined the structure of that particular soldier were not such as to lead to the production of either wings or eyes. A second soldier is also without eyes and

without wings, and for the same reason. Not a single ancestor of either of them was, so far as we know, devoid of either wings or eyes. Winglessness and blindness are constant characters among soldier termites, not only in one species, but in several; not only on one continent, but on several; and there is reason, therefore, to believe that winglessness and blindness were already constant characters, even before the existing species were differentiated from one another—were even constant characters before any existing species was evolved from the common ancestral type from which they have descended.

In the lungs of a common frog are frequently found several parasitic worms belonging to the species *Rhabdonema nigro-venosa*. Each individual *Rhabdonema* is male when young, and later in life becomes female—in other words, it is a protandrous hermaphrodite. The old ones (that is the females) produce ova, which, after being fertilised by the spermatozoa of the younger ones (that is of the males), are carried from the lung to the mouth of the frog, and are then swallowed and pass with the food through its intestine, developing, as they go, into *Rhabdites*. These are ejected with the excrement of the frog, and develop, in moist earth, into sexually-mature worms (*Rhabdites*), of which some are males and others females. They are never hermaphrodite, and they are, in other respects, unlike their parents. The few ova produced by a female are fertilised by the spermatozoa of a male, and, after feeding for a time upon the tissues of the mother's body, escape, and find their way into the lungs of frogs, there developing into *Rhabdonemata*, like their grandparents and unlike their parents, and passing through a condition in which all are male to a later condition in which they are female.

I fail utterly to see how this resemblance of individuals to their grandparents, and unlikeness to their parents, this regular alternation of generations of hermaphrodite *Rhabdonemata* with generations of male and female *Rhabdites*, is to be "explained" by the continuity of germ-plasm, and I see no necessity for assuming the transmission of two sets of pangenetic gemmules which take it in turns to determine the structure of the successive generations. It is very easy to believe that the influences at work producing ova and spermatozoa in one pair of *Rhabdites* are very like those doing similar work in another pair, and seeing that this likeness in itself will explain the likeness of the *Rhabdonemata* which develop from these ova and spermatozoa in one case, to the *Rhabdonemata* produced in the other case, there is no need for any assumption beyond that. So long as causes are alike the effects are alike, and the assumption of the existence of pangenetic gemmules, or of an unproven continuity of a special germ-plasm appears to be uncalled for.

I am almost afraid to refer to the fact that external influences have some share in determining the form, not only of the developing individual, but also of its offspring, because, whenever I have done

so in the past undue importance has been attached to the part I have supposed them to play. To guard against this misunderstanding as far as possible, I will refer to a case in which it must be obvious that I do not suppose that the particular influence I shall refer to has anything to do with heredity in any accepted sense of the word. I regard the likeness of one individual to another as arising from a likeness in the causes which determined the characters of the two individuals respectively. Into those causes some external influences have entered as factors, direct or indirect, but the external influences dwindle into insignificance when compared with the internal ones.

Professor Weismann records an experiment of his, in which he cut off the tails of many very young mice of half-a-dozen successive generations. All those mice, when fully developed, were tailless mice. They resembled each other in being mice by virtue of the action of internal influences, which were approximately alike in all cases. They resembled each other in being tailless by virtue of the action of one particular external influence, which was alike in all cases. So long as that external influence remained constant, taillessness of the full-grown mice was a constant character, *and no longer*. When the influence to which that taillessness was due was removed, the taillessness ceased to recur.

If some change in the conditions of existence should so alter the constitution of mice that the fertilised eggs they produce should have such structure as would lead to their development into tailless mice, then there is reason in believing that the maintenance of the changed conditions would convert the mouse into a tailless species, and that the taillessness of the species would remain a constant character so long as no further change in the conditions of existence occurred, *and no longer*. Though the specific characters are determined by the joint operation of internal and external influences, the internal influences are infinitely more effective than the external, as they are also infinitely more complex. But, on the other hand, the internal influences depend both directly and indirectly upon the external ones. And, further, none but slight changes in the external influences are compatible with the continued existence of the species.

The direct, though perhaps remote, influence of external influences upon the internal constitution, and hence more remotely upon the constitution of the ova and spermatozoa, and hence again more remotely still upon the form and structure of succeeding generations, is well seen in the transformation of a stock of *Artemia salina* into *A. muelhauseni*, and *vice versâ* by Schmankewitsch through alteration of the strength of the salt-water in which the animals were living. This result is so interesting that it is surprising that nobody with the means at command has yet been found to verify it by further experiments. The transformation of a stock of *Artemia* into *Branchipus* is so astounding, that though it might strengthen my argument I dare not assume it to be true till it has been verified.

Semper's experiments upon *Limnea*, though similar, are less striking examples of the direct effect of external conditions upon the development of each individual. They show the size of a Limnea to be as much dependent upon external conditions as was the taillessness of Weismann's mice.

The life-history of Phylloxera or of Aphis is an excellent example of the direct effect of the environment, including food-supply and temperature, upon the form of the offspring. That is, upon the form and structure of the generation as yet unborn, through its effect upon the internal influences determining the structure of the germs in process of formation. In Phylloxera, four successive generations, all different from one another, intervene between each two generations, capable of amphigonous reproduction. More generations may intervene—that depends upon external conditions—but these four, all different from one another, are tolerably constant, and the individuals of generation A in one year are like the individuals of generation A in any other year, though unlike the ancestors of any of the four next preceding generations. To the question "Why?" my answer is the same as in the case of the *Rhabdonemata*.

One more example will suffice. John Jones resembles his father so closely that it is impossible to say which of two photographs taken at similar ages is the portrait of John and which is that of his father. This I hold to be due, not to the continuity of the spermatic portion of the germ-plasm of two successive generations, but to the reproduction, in two generations, of a complex set of influences which, by their interaction and co-operation, have determined the form of the father in one generation and of the son in the next, and these influences I take to be both paternal and maternal, both internal and, to a very slight degree, external also.

All the cases which I have given as illustrations have one thing in common. In every case I have referred to the occurrence of some individuals like some other individuals belonging to an antecedent generation, though the individuals resembled are in some cases parents, in others grandparents or ancestors more remote, and in the case of the termite soldier the individuals resembled are not ancestors at all. In every case I hold the likeness between the two individuals to be due to the likeness of the causes which have determined the formation of those individuals. B is like A because its form and structure have been determined by influences like those which have determined the form and structure of A. When B is the direct descendant of A, it is customary to call the phenomenon "heredity." Rightly or wrongly, I hold the phenomenon to be of the same nature in all cases, and to be simply an illustration of the general law: "Like causes produce like effects."

How the like causes are produced, I shall endeavour to explain in a future paper, as also the phenomena of atavism and variation.

C. Herbert Hurst.

III.

The Evolution of Oceans and Continents.

NATURAL Science is indebted to Dr. Wallace for clearing away some misconceptions which have surrounded the question of the permanence of Oceans, and for imparting some freshness to the controversy by adducing several new arguments in its favour. Hitherto the question has been discussed rather as if it were necessary to hold one of two extreme views; the one view being that the great continental plateaux and oceanic depressions were formed at a very early period of the earth's history, and have occupied their present positions ever since; the other view being that the movements of the earth's crust have been so frequent and so great that no part of it has remained either in the state of sea or land through many periods of geological time.

I will not stop to enquire whether Lyell believed in the possibility of such frequent interchange when he wrote that "continents, although permanent for whole geological epochs, shift their positions entirely in the course of ages." The extreme view may have been held at one time when little was known regarding the depths of the ocean, but few geologists of any repute would maintain it at the present time. On the other hand, the idea of the fixity of the oceanic and continental areas is supposed to have received strong confirmation from the investigations of the "Challenger" Expedition. It was argued that modern oceanic deposits differ so much from any rocks which form part of the continental plateaux that none of these rocks could be of oceanic origin, and consequently that oceans and continents could never have changed places. It was confidently stated that *all* ancient marine formations had been formed in comparatively shallow water, and it was even predicted "that representatives of the abysmal deposits of the central oceans are not likely to be met with among the geological formations of past times." [1]

The supposed absence of oceanic deposits from continental areas was regarded as proving that the interchange of sea and land was confined to areas within 200 or 300 miles of the continental plateaux where the water was less than 1,000 fathoms in depth. In this way 92 per cent. of the whole oceanic area was excluded from the regions of possible interchange. Against this view I have protested as being

[1] Sir A. Geikie. "Text Book of Geology," 1st edition, 1882, p. 608.

merely the other extreme of the swinging pendulum of scientific imagination.

Recent discoveries of oceanic deposits, such as must apparently have been formed in depths of from 1,500 to 2,000 fathoms, and including representatives both of "red clay" and radiolarian ooze, but now raised high above the level of the sea within the limits of a continental plateau, have shown the danger of depending on negative evidence. Again, a more accurate knowledge of submarine contours and the exigencies of explaining the distribution of animals have obliged Dr. Wallace to include much greater depths within the regions of possible interchange. He now says: "All that is necessary to maintain therefore is, that existing continents with their included seas and their surrounding oceanic waters as far as the 1,500-fathom, or in some extreme cases the 2,000-fathom line, mark out the areas within which the continental lands of the globe have been built up; while the oceanic areas beyond the 2,000-fathom line, constituting, according to Mr. Murray's data, 71 per cent. of the whole ocean, have almost certainly been ocean throughout all known geological time."

This is a great concession, and the "almost certainly" may foreshadow further admissions; 71 per cent. is very different from 92 per cent., and the extension of interchangeable areas to regions within the 2,000-fathom line enables us at once to speculate on the possibility of a former antarctic continent with such extensions toward New Zealand, South Africa, and South America as will explain the curious points of similarity in the modern faunas of these countries. The existence of such a continent has already been suggested by Dr. W. T. Blanford.[2] Dr. Wallace's present view also enables us to assume the former existence of a large continent in the Western Pacific with extensions southward to New Zealand and westward to Australasia. It also gives us great possibilities of change in the Atlantic Ocean, though hardly sufficient to account for all the palæontological evidence which has been or can be adduced in favour of old connections between the more-tropical parts of Africa and America.

So far, therefore, Dr. Wallace has departed from the extreme views which have been held by the writers whom he quotes with approval, and which he upheld even in the second edition of his "Island Life," published only a few months ago. He therein argues against the deep-sea origin of chalk, and repeats the very erroneous statement that "deposits uniform in character and more than 150 or 200 miles wide were rarely, if ever, formed at the same time." Why does he vainly endeavour to deny the close similarity between chalk and foraminiferal ooze when in a later chapter he contemplates submergences of more than 1,000 fathoms in extent?

[2] Pres. Address to Geol. Soc., 1890, p. 104.

He now advances certain arguments against the wholesale interchange of oceanic and continental areas, but the force of his reasoning is greatly weakened by a misconception which appears to underlie the whole of it. He seems to think that those who oppose the theory of the permanence of oceans must believe in the possibility of a brand new continent rising whole and complete from the ocean depths while an older continent was compelled to vanish beneath the waters in order to preserve the terrestrial balance. I am not aware that anyone has ever propounded such an audacious hypothesis. I have never seen it seriously suggested that an entire continent has been contemporaneously submerged, nor has any modern geologist contemplated "the building up of a continent the size of Africa from the mean depth of the ocean." Certainly Dr. Wallace is not warranted in imputing such crude ideas either to Dr. Blanford or to myself, and every geologist will admit that "the remarkable parallelism and completeness of the series of geological formations in all the best known continents indicates that none of them have risen from the ocean floor during any portion of known geological history."

Those who oppose the doctrine of permanence say that the present continents are the outcome of a long series of geographical mutations, and I would add that each phase was an episode in a long process of geographical evolution. There is good reason to believe that even in Pliocene time the outlines of the continents were very different from the present, some areas now below the sea being then above it, while other tracts then beneath oceanic waters have since been raised into dry land. We know that Miocene geography differed still more greatly from that of to-day, and it is not therefore unreasonable to suppose that in the Cretaceous period large parts of the modern oceans were land, and large parts of the modern continents were portions of the ocean, the continental connections being totally different from what they are now. In short, the interchange we believe in is the frequent interchange of small portions of oceans and continents, till, in the course of time, the accumulated changes have accomplished great geographical mutations.

Against this view of interchange it does not seem to me that either the first or the third of the considerations mentioned by Dr. Wallace are very powerful objections. With respect to the second argument, that there are no irregularities on the ocean floor corresponding to those on land, I would point out (1) that it applies most completely to the deeper and consequently to the oldest portions of the ocean floor, (2) that we really as yet know very little about the details of sub-oceanic contours, (3) that long-continued deposition must tend to obliterate any pre-existent irregularities, (4) that such inequalities are likely to have been much smaller than those shown in continental areas, because the continents are probably those

portions of the earth's crust which have been subjected to the greatest pressures and disturbances; while the deeper parts of the ocean are probably those where the crust has long been in a state of tension and comparative quiescence. It is true that the tension might, and probably has, led to the production of faults; but the faults developed at any one epoch would be small, and if they formed a system of trough-faults the surface features produced would be quite insignificant. (5) Lastly, if there be any truth in Mr. Fisher's idea of the frequency of sub-oceanic outpourings of lava, such extravasations would largely account for the comparative smoothness of the ocean floor.

Here, so far as Dr. Wallace's arguments are concerned, I might leave the subject, but I wish to make a further suggestion, and, following Dr. Wallace's example, to advance another *à priori* consideration.

All who have hitherto discussed the question of the permanence of oceans have done so on the basis of the present physical conditions of the globe, and have assumed that the volume of ocean water, and consequently the mean depth of the ocean, have always been the same. But are we justified in making this assumption?

The Rev. Osmond Fisher has done a great service to the science of Geology in showing that the contraction theory is totally inadequate to account for the inequalities of the earth's surface, and in placing the hypothesis of a thin crust with a liquid substratum upon a sound basis by his suggestion that water in the state of gaseous matter is associated with the liquid magma. In this way he explains the presence of water among volcanic products, which has always been a physical difficulty. But, if his theory be true, it follows that every eruption which has taken place on the globe during the long ages of past time has contributed a certain amount of water to swell the volume of the ocean. Hence the oceans have been gradually increasing in depth and extent throughout all geological time, and the area of the land has been as gradually diminished.

If, therefore, we accept this hypothesis (and it affords by far the most satisfactory basis for the explanation of terrestrial physics that has yet been imagined), we must look back to a time when the oceans were very much smaller and shallower than they are now. Further, if we accept Mr. Fisher's view of the manner in which continents and mountain chains have been ridged up by the pressure of the horizontal connection-currents in the substratum, it seems probable that there has been a tendency toward the condensation of land masses into continental areas, and we may look back to a time when the continental plateaux did not rise so high above the mean level of the ocean floor as they do now.

It would appear, then, that a geologist is justified in supposing the total area of land in early Palæozoic time to have been equal to or greater than that of the sea, both being more equally distributed,

so that there were neither great oceans nor isolated continents in our modern sense of the terms. From this condition of geographical equality, it seems to me there has been a gradual tendency to the development of high continental plateaux, while the deeper sea-troughs have been merged into oceans, and the volume of the surface waters has been continually increased by the extrusion of the water-substance from the interior of the earth.

On this hypothesis we can at once explain the rarity of extensive oceanic deposits of the modern type among the great series of stratified rocks, for in the comparatively narrow and shallow seas of Palæozoic time no such deposits could be formed. Now, as a matter of fact, while deep-water muds are common among Palæozoic rocks, no rock which is likely to have been analogous to a modern abyssal ooze has yet been found, for the radiolarian chert of Ayrshire can hardly be claimed as such, either in character, thickness, or extent. Coming down to Mesozoic time, we find the Cretaceous Chalk, which does in all three respects bear great resemblance to a modern calcareous ooze, but it is not associated with abyssal deposits of the red clay and radiolarian ooze types. When, however, we reach Tertiary times the West Indian deposits furnish proof that the oceans were deep enough for the formation of all the modern types of oceanic deposits.

On this hypothesis of the gradual evolution of oceans and continents we can imagine the former existence of continental land where the ocean now rolls, without depressing any modern continent below the ocean to produce it. In short, we may believe that the places of our continents have *nearly* always been occupied by land or by shallow seas without assuming that the land areas have always been restricted to the present continental plateaux. We may likewise believe in the great antiquity of the deeper parts of the oceans, without assuming that these oceans have always been either as deep or as extensive as they now are.

In his Appendix (p. 9), Mr. Fisher thinks it probable that his conclusions as to the structure of the sub-oceanic crust do not apply to the borders of the oceans, and that the one type of crust changes gradually into the other type, "so that there is an intermediate belt of which we cannot assert that it belongs exclusively either to the oceanic or to the continental type." This allows us a certain amount of latitude; but I do not think he goes far enough, and believe his own conclusions must eventually lead him into the very theory of evolution which I have just suggested. For instance, he accounts for the greater density of the upper layer of the typical sub-oceanic crust by supposing it to consist of extravasated basic lavas; but the extravasation of these lavas must have gone on *pari passu* with the compression of the continents, and hence the very construction of a special type of sub-oceanic crust must have been a process of gradual development. In dealing with geological time, Evolution, as distinct from

Restorative or Catastrophic Change, must be our principle of interpretation.

Here, however, it may be objected that Mr. Fisher has supported the theory of the permanence of oceans, and that he assigns to the sub-oceanic crust a structure which differs from that of the continental crust. In his Appendix, however, he has partially altered his conception of the structure of the sub-oceanic crust, and he has recently read a paper before the Cambridge Philosophical Society in which he arrives at results which tell strongly against his speculation about the origin of ocean basins; in this I much rejoice, for that chapter in his book had never commended itself to me.

It must be remembered, too, that Mr. Fisher has not attempted to enter into the history of the sub-oceanic crust. His position is simply this; his calculations show that, if his hypothesis be true, there are large tracts of crust which must have the special structure he ascribes to them; he does not assert that they have always had that structure, but he does see a difficulty in supposing that, having acquired that structure, they can ever again become continental land.

A. J. Jukes-Browne.

IV.

Recent Advances in Knowledge of the Ichthyosaurian Reptiles.

FROM the circumstance that nearly all their skeletons found in the English Lias have a dislocation in the vertebræ of the tail, Sir Richard Owen was led many years ago to the conclusion that the extinct marine reptiles, known as Ichthyosaurs, were furnished with an expanded fin at the end of the tail, and that the weight of this fin caused the fracture in question. Years passed on without affording any evidence one way or the other in relation to this bold theory; but at length, in the present year, there has been discovered in the Lias of Württemberg the skeleton of one of these reptiles, in which the outline of the fleshy parts is completely preserved, and which proves the existence of a caudal fin of still larger dimensions than Owen supposed to be the case. This interesting specimen is described by Dr. E. Fraas in the paper standing fourth in our list, from which the accompanying figures are reproduced. We already knew that in the paddles the fleshy part was extended much behind the bony skeleton; but the new specimen shows us that, in addition to the tail-fin, the Ichthyosaurs had a triangular fin on the middle of the back, behind which was a crest of horny excrescences compared to those of the crested newt. The tail-fin is vertical and nearly symmetrical externally, although the backbone runs downward to terminate in its lower lobe. In this respect the fin has the same general aspect as in the Sharks, except that in the latter the backbone runs into the upper lobe. It shows, indeed, as Dr. Fraas remarks, how closely analogous is the form of Ichthyosaurs to fishes; and it thus further justifies the title of "fish-lizards" which has been applied to these reptiles.

An exhaustive comparison of the skeletons of the Ichthyosaurs from the Liassic deposits of the Continent with those from the corresponding horizons of England, has long been a desideratum. This want has, however, now been remedied by Dr. Fraas, in the magnificent work mentioned second in the following list. By no means the least important features in that work are the beauty, number, and large size of the plates with which it is illustrated; and with these by his side the English Palæontologist ought no longer to have much

doubt as to the nature of the relationships of the Continental to the British Ichthyosaurians.

It need hardly be mentioned that the first Ichthyosaurs specifically described were obtained from the Lower Lias of our own country, and there is consequently no sort of doubt as to the names by which they should severally be designated. Very different, however, is the case with the species rom the Upper Lias ; names having been applied to specimens in this country and on the Continent, many of which there has long been reason to regard as specifically identical, although there has hitherto been great difficulty in obtaining decisive evidence on these points. Dr. Fras has, however, now enabled us to judge for ourselves on these difficult questions, and has brought

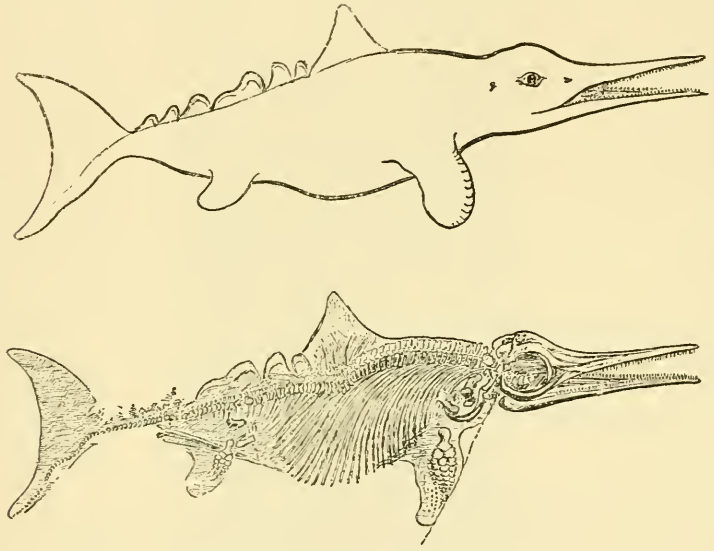


FIG. 1.—Skeleton of an Ichthyosaur, showing the outline of the soft parts, together with a restoration of the same. (*From Fraas.*)

forward evidence to show that there is every probability of the identity of the majority of the British and Continental species.

Dr. Fraas commences his memoir with an interesting survey of the history of the gradual acquisition of our knowledge of the structure of the Ichthyosaurian skeleton, from the date of Sir Everard Home's first memoir published as far back as 1814. We may observe, however, that when stating that the name *Ichthyosaurus* is antedated by *Proteosaurus* of Home, the author appears to have been misled by a similar statement in the British Museum Catalogue of Fossil Reptiles ; the real fact being that the former name is three years the earlier of the two.

The historical portion of the memoir is succeeded by a dissertation on the general structure and affinities of the Ichthyosaurs. Here the author heartily endorses the views of Dr. Baur as to the close

affinities of these extinct reptiles with the primitive Rhynchocephalians (of which the sole remaining representative is the Tuatera Lizard—*Sphenodon*—of New Zealand). Dr. Fraas is, however, by no means thereby prepared to accept the view that Ichthyosaurs, Rhynchocephalians, and Lizards should be regarded as constituting one great branch of the reptilian tree. Seeing, however, that he admits the probable origin of these reptiles from a primitive Rhynchocephalian, the cause of his hesitation to adopt this view is not very apparent. That the Ichthyosaurs have sprung from a stock which was originally terrestrial, and hence occupy a position among reptiles precisely similar to that held by the Whales among mammals, Dr. Fraas fully endorses.

In regard to the author's *résumé* of the structure of the Ichthyosaurian skeleton, we have but one point to mention. This relates to the bones forming the palate, in regard to the interpretation of which there has been some difference of opinion. While adopting the view taken in the British Museum Catalogue of Fossil Reptiles, that the pair of bones occupying the middle of the palate in advance of the pterygoids are the vomers, Dr. Fraas, as shown in plate ii. of his memoir, considers that the bony bars flanking the two latter severally consist of an anterior moiety identified with the palatine, and a posterior moiety corresponding with the transpalatine or transverse bone. It is true that in the skull figured by Dr. Fraas from Sir R. Owen's plate there appears to be a division in this bar, but we strongly suspect this is due to fracture; and there is none in the type skull of *I. zetlandicus* figured by Professor Seeley. Moreover, the position of the palatines entirely in front of the posterior nostrils would be quite abnormal. We have accordingly no doubt but that the whole of these bony bars are the palatines.

Many of the considerations relating to the probable mode of life of the Ichthyosaurs referred to by our author cannot fail to command attention; but the supreme interest attaches to the evidence in favour of the viviparous reproduction of at least some of these reptiles. It has been long known that certain large skeletons of Ichthyosaurs from the Upper Lias of Holzmaden, in Württemberg, and elsewhere, are found with the skeletons of one or more much smaller individuals enclosed partly or entirely within the cavity of the ribs as shown in our figure. Of such skeletons there are four in the museum at Stuttgart, two in that of Tübingen, one at Munich, and others in Gent and Paris (Fig. 2). Of these, two in Stuttgart, as well as the two in Tübingen, contain but a single young skeleton, while one of those at Stuttgart has four, the Munich specimen five, and the remaining Stuttgart example upwards of seven young. Some of these young and, presumably, fœtal Ichthyosaurs have the head turned towards the tail of the parent, while in others it is directed the other way. That these young have not been swallowed by the larger individuals within whose ribs they are found, is pretty

evident from several considerations. In the first place their skeletons are always perfect. Then they never exceed one particular size, and always belong to the same species as the parent. Moreover, it would appear to be a physical impossibility for one Ichthyosaur of the size

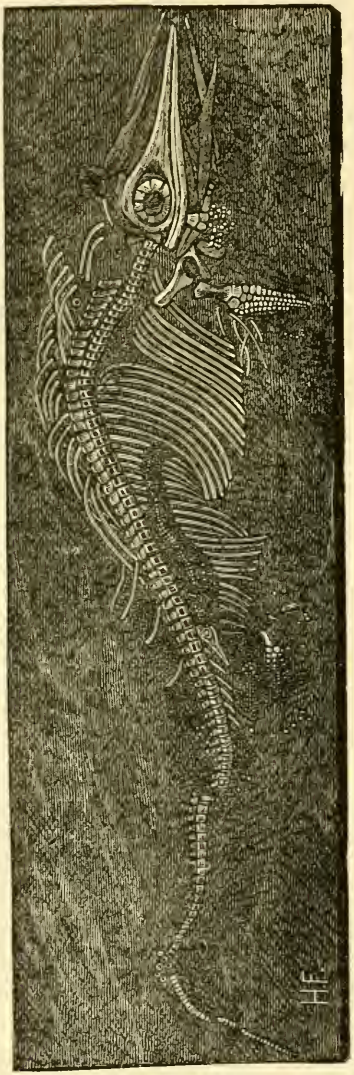


FIG. 2.—Skeleton of *Ichthyosaurus quadriscissus*, with fœtus enclosed in the body-cavity. $\frac{1}{14}$ natural size. (*After Gaudry.*)

of the Stuttgart specimen to have had seven smaller ones of such dimensions in its stomach at one and the same time. We may accordingly take it for granted that these imprisoned skeletons were those of fœtuses. It is, however, very remarkable that, so far as we

are aware, all the skeletons with fœtuses belong to one single species; thus suggesting that this particular species was alone viviparous. Dr. Fraas considers that the retention of the ova within the body of the parent until developed into fœtuses of considerable size, is to be accounted for by the purely marine mode of life of these reptiles, which he believes were not in the habit of visiting the shore. If it should prove that viviparous reproduction was confined to the species with narrow paddles, like the one represented in our figure, it may be suggested that the species with broad paddles, like *I. communis* of the Lower Lias, were accustomed to visit the land to deposit their eggs, and thus had no need of special arrangements in regard to their reproduction.

Coming to the systematic portion of the memoir, we find, as might have been expected, that the small Ichthyosaur from the German Trias, described as *Ichthyosaurus atavus*, is transferred by Dr. Fraas to the genus *Mixosaurus*, originally founded upon the evidence of specimens from the Italian Trias. This genus, it may be observed, in the structure of its limbs, tends to bridge the gap between the true Ichthyosaurs of the Lias and more normal reptiles; and it is regarded by Dr. Fraas as the proximate ancestor of the two great groups of Ichthyosaurs, namely, those with narrow and those with broad paddles. Of the Ichthyosaurs of the Lower Lias, Dr. Fraas has but little to say, since they are but poorly represented in Germany. We may observe, however, that it has been shown in the "Catalogue of the Dublin Museum," standing sixth on our list, that the long-snouted forms described as *I. tenuirostris* and *I. latifrons*, really belong to but a single species, for which the former name must be employed. From our own studies we also fail to find satisfactory means of distinguishing the long-snouted Ichthyosaurs of the Upper Lias from the Lower Liassic species, and we are, therefore, fain to regard the three species, *I. tenuirostris*, *latifrons*, and *longirostris*, admitted by Dr. Fraas, as founded upon one and the same form. Our readers desirous of further information on this species may refer to the paper standing last on our list.

The great bulk of the descriptive portion of Dr. Fraas' memoir relates to the Ichthyosaurs of the Upper Lias, the skeletons of which are found so beautifully preserved in the shales of Holzmaden. For the common German species (the one represented in our figures) the name *Ichthyosaurus quadriscissus* (in allusion to the slits in the edges of the bones of the front of the paddles) is adopted; and, on the whole, we think this is the best course, seeing that our author has shown that *I. acutirostris*, with which this species had been identified, is a totally distinct form. This species is represented in the Upper Lias of England as well as Germany; and Dr. Fraas is disposed to consider that the huge English Ichthyosaur, described as *I. zetlandicus*, together with an identical form from Normandy, figured as

I. longifrons, is only a variety of this species, an opinion with which we are much disposed to concur.

Much important information is furnished by Dr. Fraas with regard to certain Ichthyosaurs of the Continental Upper Lias, described by Theodori in 1854, which it has hitherto been impossible for English writers to refer to their proper systematic position. The first of these is *Ichthyosaurus crassicostatus*, which is now shown to be allied to *I. tenuirostris*, although having a much shorter snout. It has, therefore, the same relation to the latter as is presented by the so-called *I. brevirostris* to *I. communis* of the English Lower Lias. We are, however, by no means assured that such short-snouted forms—which, be it observed, are of extreme rarity—are anything more than abnormal examples of the longer-snouted species to which they are severally allied, similar abnormalities being well-known to occur among the Cetacea. Then, again, we have the still more rare *I. macrophthalmus*, which appears to differ from the type of *I. crassicostatus* only by its larger orbits, smaller teeth, thinner ribs, and the absence of slits in the bones of the paddles. In respect to its teeth, this form serves, indeed, to connect *I. crassicostatus* with *I. quadriscissus*, and it thus appears to us to require much stronger evidence than any yet published to prove that these three forms are really specifically distinct.

The third of these hitherto imperfectly known Upper Liassic forms is *I. ingens*, which is now definitely referred to the Tenuirostrine group, and appears to be mainly characterised by its gigantic size. In this respect it, however, scarcely exceeds the type of *I. zetlandicus*; and we see no reason why it should not be specifically the same, in which case Theodori's name would have to be retained. Moreover, if this form be identical with *I. zetlandicus*, and the latter with *I. quadriscissus*, the name *I. ingens* antedates both the others; the same being the case with regard to the names *I. crassicaudatus* and *I. macrophthalmus*.

Before leaving the Upper Liassic Ichthyosaurs of the Tenuirostrine group, we must briefly refer to the paper by Professor Gaudry, quoted at the end of this review. This refers to a large Ichthyosaur discovered in the Upper Lias of the Yonne, and provisionally named *I. burgundiæ*. It is described as having a long snout, with a pectoral limb of the type obtaining in the Tenuirostrine group, and the teeth fluted. The pelvic limb is, however, peculiar, in that it has two rows of bones arising from the intermedium, or median bone of the tarsus, after the fashion obtaining in *I. communis*; and it appears to be this character which led Professor Gaudry to consider the specimen as indicating a new species. We would suggest, however, that such a peculiarity may perfectly well be an individual one, in which event we fail to see that the other points mentioned in the description are sufficient to distinguish this skeleton from *I. zetlandicus*. The specimen is, however, of considerable interest, as showing how species

with narrow paddles (*Longipinnate*) may have passed into those in which the paddles were wide (*Latipinnate*).

With *Ichthyosaurus acutirostris* we come to the first of those species in which the teeth, instead of being evenly fluted all round, are smooth, and carry two or three sharp vertical ridges; from which character the members of this group have been separated from the genus *Ichthyosaurus* as *Temnodontosaurus*. Dr. Fraas has rendered good service in showing that *I. acutirostris* is a member of this group, and consequently has nothing to do with *I. quadriscissus*, with which it had been erroneously identified. The only other Upper Liassic species mentioned is the gigantic *I. trigonodon*—the analogue of the still more gigantic *I. platyodon* so commonly found in the Lower Lias of England.

Of the post-Liassic species of the Jura, the materials at Dr. Fraas' command do not appear to have been very abundant. It is, however, considered that the Jurassic *I. leptospondylus* is an ancestral type of the genus *Ophthalmosaurus*, characterised by having three bones articulating with the humerus, and being evidently the extreme modification of the Latipinnate type; a further characteristic being the tendency to a partial or complete loss of the teeth. On the other hand, our author is indisposed to admit the reference of the well-known Cretaceous *I. campylodon* to the Latipinnate group, and rather considers that this powerfully-toothed species has been derived from the Longipinnate group, which is not regarded as the ancestral stock of the Latipinnates. The main ground of this new departure appears to be the alleged resemblance in the structure of the skull of *I. campylodon* to that of the Longipinnates, as displayed in sections. No explanation is, however, offered to account, on this view, for the resemblance of the radius of the Cretaceous species to that of the Latipinnates, and its dissimilarity to the corresponding bone of the Longipinnates. Some day, perhaps, we may have the good fortune to obtain an entire paddle of *Ichthyosaurus campylodon*, by which the question will be decided finally. With a brief reference to Professor Capellini's memoir, recording the occurrence of this widely-spread species in the Cretaceous of Italy, we may appropriately bring to a close our survey of these important series of contributions to our knowledge of the Ichthyosaurs.

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R. LYDEKKER.

V.

Notes on the Development and Structure of Arachnids.

IN a recent contribution to NATURAL SCIENCE (pp. 281, 2) I discussed the correspondence of the appendages of Insects and Arachnids in connection with Cholodkowsky's researches on the embryology of the cockroach. It will be remembered that he maintains the post-oral origin of insect antennæ and their consequent homology with the chelicerae of spiders, scorpions, and their allies. In this he is at variance with most modern writers, who, regarding the antennæ of insects as arising from the pre-oral region of the embryonic head, compare the chelicerae of Arachnids with the mandibles of Insects. The former, however, was the prevalent view some years ago, and is restated quite recently in the elaborate Memoir (1) on the Anatomy of Arachnids, by Gaubert, who, on this point, follows his master, Milne-Edwards.

But Gaubert refers to the other view (that of Balfour), and admits that considerable support has been lately given to it by the researches of Jaworowski (2, 3). The latter observer has studied the embryology of a large "tarentula" or hunting spider (*Trochosa signoriensis*), and he describes and figures a pair of structures which arise behind the mouth and in front of the chelicerae, which never become jointed, and which ultimately sink into cavities in the blastoderm. These he regards as rudimentary antennæ. In front of the segment to which they belong, he describes and figures three other segments, of which only one appears to be pre-oral. By adding these four segments to the six which bear the appendages which persist in the adult spider, we see that the cephalothorax is formed by the fusion of at least ten segments. Though no previous observer has described antennæ in an embryo Arachnid, Jaworowski points out the similarity between the rudiments figured by him and a pair of structures, which are shown in exactly the corresponding position in the figure of the embryo of *Agelena labyrinthica* in Balfour's "Treatise on Comparative Embryology" (vol. i., fig. 200*d*). A comparison between the two figures[1] makes it highly probable that

[1] Jaworowski's figures were reproduced in *Nature*, vol. xlvi., 1892, pp. 272, 3, to illustrate Professor Sollas' review of Simroth's "Die Entstehung der Landtiere."

the structures are homologous. Now in the "Treatise" they have no reference-letters, and it might be inferred that Balfour, though he saw and figured rudimentary antennæ, failed to recognise their importance. But if we turn to his original paper (4), from which the figure in question was taken, we find that he describes these structures, without doubt, as the ectodermal thickenings from which are developed the ganglia innervating the chelicerae. Is it possible, then, that Jaworowski has mistaken these thickenings for appendages, and that the supposed antennæ are in reality only incipient nervous centres? The fact that they sink into cavities seems to lend support to such a view, but their pointed appearance (they are more pointed than in Balfour's figure) suggests appendages. Probably we shall be correct in assuming that the antennæ of the ancestors of the Arachnids arose in the region where the ganglia were being developed, and that in some forms, as *Trochosa*, the pointed rudiments indicate this, though their subsequent fate is to form part of the nervous centre.

But, whatever may be the exact meaning of these rudiments, the presence of three segments between the chelicerae and the mouth seems certain. And so, while Jaworowski supports Cholodkowsky's view of the post-oral origin of insect-antennæ, it is impossible to regard them as equivalent to the chelicerae of spiders; for the antennæ are said to arise from the first post-oral segment, while the chelicerae are developed from the fourth. The presence of certain transitory appendages in front of the mandibles in insect-embryos, taken in connection with the segments observed by Jaworowski in front of the chelicerae in the embryo of *Trochosa*, confirms the view that the mandibles in the one class correspond with the chelicerae in the other; and working backwards along the appendages, we identify the maxillæ of Arachnids with those of Insects, the first pair of legs in Arachnids with the second pair of maxillæ, which form the lower lip of Insects, and the second, third, and fourth pairs of legs in Arachnids with the three pairs of insect-limbs. We conceive that the two classes have diverged from a common stock; in the Arachnids the antennæ have been lost, the head has undergone fusion with the thorax, and two pairs of limbs only have the function of jaws; while in the Insects the head has been highly specialised, the antennæ preserved, and three pairs of limbs pressed into the service of the mouth. The passage between a walking-limb and a jaw is well shown in the harvestmen (Phalangidea), in which the first two pairs of legs have at their bases small masticatory plates. It is highly interesting to note how, in various groups of Arachnids, the place of antennæ is supplied by other limbs; in the spiders by the maxillary palps or by either of the front two pairs of legs, in the spider-scorpions (Pedipalpi) by the wonderfully attenuated first pair of legs.

When we consider that the insect and arachnid types were already developed in the Silurian period, we can hardly expect that

palæontology will ever throw much light on the ancestry of the two classes; it is to the structure and development of living forms that we must look for our information.

In the embryo of *Trochosa* the limbs, which afterwards become so differentiated as the cheliceræ, and also the rudimentary limbs on the abdominal segments, which afterwards disappear altogether, show indications of jointing like the legs; this is strong evidence of the former ambulatory function of all the limbs. Another observation of great interest made by Jaworowski is that in the cephalothoracic appendages of the embryo *Trochosa* (except the cheliceræ) there are parts which correspond to the coxopodite, endopodite, exopodite, and epipodite of a crustacean limb. The epipodite is more obscure in the hinder segments, and, in all cases, disappears. From the fact that the epipodite of a crustacean often bears a gill, and that the spider's epipodite is a transitory embryonic structure, Jaworowski infers the descent of the air-breathing Arachnids from water-breathing ancestors. The epipodite of *Trochosa* is only mentioned in his most recent paper (3). It is rather amusing that Simroth in his book, "Die Entstehung der Landtiere," should have used Jaworowski's first paper (2) with the description of the endo- and exopodite of *Trochosa* in support of the exactly opposite conclusion: that water-breathing crustaceans have branched off from ancestors that lived on the land!

The aquatic representative of the Arachnids, however, is the King Crab (*Limulus*), whose gills, borne on the hinder faces of the flattened abdominal limbs, have been repeatedly compared with the lung-books of spiders and scorpions. Lankester (5) and Kingsley (6) suggested the formation of the arachnid lung-book by the invagination of an appendage intermediate in form between the gill of *Limulus* and the pecten of a scorpion. MacLeod (7) proposed that the flattened appendage of *Limulus* might become fused by its edges with the body-wall, so as to enclose the gill-lamellæ. Laurie in a recent paper (8) inclines to the latter view. He finds that in the embryo of *Scorpio fulvipes* the lamellæ lie horizontal, and parallel to the ventral surface, directed forwardly and outwardly from the stigmatic opening. Such an arrangement would require the invagination of an appendage bearing the lamellæ on its front face, whereas the appendages of *Limulus* bear them on the hinder face, and so Lankester's theory would require the complete reversal of the gills. No such difficulty is raised by MacLeod's view. It is noteworthy that in the adult *Scorpio* the lamellæ take up a different position from what they have in the embryo, lying vertically and parallel to the axis of the body. In the adult spider they retain the horizontal position, so that in this respect the spiders show the more primitive arrangement.

In most Arachnids a pair of coxal glands is found near the base of one or more of the pairs of legs; these structures, which open to the exterior only in the embryo and become ductless in the adult,

are generally regarded as the surviving representatives of a series of nephridia, which formerly occurred in nearly all the segments, as in many worms and in *Peripatus*. Lebedinsky has recently (9) described the development of these glands in the common harvestman (*Phalangium opilio*) in the embryo of which they open behind the first pair of legs. Each consists of a coiled tube with thin walls opening to the exterior by a straight duct with thicker walls. This duct, as well as the glandular portion, is of mesodermal origin, being formed by evagination, not, as in nephridia generally, by invagination of the ectoderm. At present, Lebedinsky prefers to call these coxal glands of Arachnids, and the shell- and green-glands of Crustaceans "homodynamous" rather than "homologous."

Passing from embryology, we have, in the work of Gaubert (1) already referred to, an important contribution to the minute anatomy of adult Arachnids of various orders. A comparative review is given of the chitinous integument, chitinogenous layer, glands, sense organs, appendages, locomotion, and influence of turgescence on the movements of joints. Gaubert has only observed two layers in the cuticle, and consequently doubts the existence of the third (intermediate) thin layer described by Schimkewitsch. He has made experiments on the cause of the rupture of the cuticle when it is cast, and has produced artificial ecdysis by plunging spirit-specimens of spiders into boiling water, concluding from this that the rupture is simply due to mechanical pressure caused by the increased size of the animal.

Elongated cells with large nuclei, occurring in the hypoderm of the jaws, and believed by some former observers to be nervous, are stated by Gaubert to be glandular, and to secrete a sticky fluid which mixes with that from the maxillary glands; similar cells occur on the lower lip, and in the pharynx. Glandular cells also form the hypoderm at the base of the femora in *Phalangium*. The two layers of the cuticle and the hypoderm are continued inwardly into the tendons, and outwardly into the hairs, spines, and claws; so that all the three latter have a similar structure. This fact should prove consoling to beginners in Arachnid classification, who may be often puzzled to decide whether a certain excrescence is a "hair" or a "spine."

Gaubert devotes much space to the "lyriform organs" on the cephalothorax and limbs of Arachnids. These consist of series of fine slits in the cuticle with central circular enlargements, filled by thin bands. A number of these slits side by side give somewhat the appearance of a stringed instrument, hence their name. They were discovered on spiders by Bertkau, and now Gaubert has found them also on spider-scorpions, harvestmen, and pseudoscorpions. He gives very full descriptions of their numbers and position on the numerous forms he has examined. From the enlargements of the thin bands nerve fibres pass inwards, which lead to large nerve-cells, whence fibres pass on to the nerve-trunk. The function of the lyriform organs was believed by Bertkau and others to be auditory.

Gaubert rejects this view, on the ground that there is no satisfactory evidence that Arachnids can hear at all. He covered the regions where the organs occur in several spiders with a light coat of varnish, and put these, together with uninjured specimens, into one end of a bottle, which he heated; the spiders which had been varnished did not try to escape from the heat as soon as the others. From this experiment, he concludes that the function of the lyriform organs is that of appreciating differences in temperature, "et peut-être aussi d'autres sensations générales." This result can hardly be regarded as conclusive; the closing of the slits by a coat of varnish would, of course, diminish the creature's power of feeling heat, but this does not prove that the organs have not another and more specific use. The thin bands certainly suggest the reception of vibrations as their function. The experiments of the Rev. O. P. Cambridge ("Spiders of Dorset," p. 582) on *Epeira diadematа* seem conclusively to prove that that species, at least, can hear. Moreover, the presence of stridulating organs in certain male spiders points decisively to a power of hearing in their females.

Much attention has been paid by Gaubert to the pecten of scorpions; he describes in detail these organs in *Buthus*. They are worked by a complicated system of muscles, turning the whole appendage, and also moving the teeth. The cuticle of the teeth is raised into numerous papillæ, each having a pore filled by a nerve-ending. The function of the pecten seems to be undoubtedly tactile, with special reference to copulation. The sense-organs on the palps and first pair of legs of *Galeodes*, described by Bertkau (*see* NATURAL SCIENCE, No. 1, p. 53), are independently described by Gaubert, who regards them as possibly auditory. The varying functions ascribed to such organs by different observers show how much we still have to learn of the life of Arthropods.

The structure of the appendages and their muscles in the various orders of Arachnids is described in great detail, and the homology of their joints carefully worked out; the attachment of the muscles is of great value in this respect. According to Gaubert, the limb of an Arachnid has three main divisions: a proximal part, directed upwards, and consisting normally of coxo-, basi-, and meropodite (coxa, trochanter, and femur), a horizontal part, sometimes of a single joint, sometimes divided into two, carpo- and propodite (genua and tibia), and a distal downwardly-directed part, the dactylopodite, which is also sometimes divided into two (meta-tarsus and tarsus). An extra division may be developed in any of these three parts of the limb; in the spiders and harvestmen the horizontal part is divided into genua and tibia, permitting lateral motion; in the Solfugidea and pseudo-scorpions there is an extra joint between the trochanter and the femur; while in the scorpions the distal part of the limb is divided into three joints, the horizontal part remaining undivided. It thus appears that, although the limbs of a spider and a scorpion each have

seven joints, only the first three in each are homologous. The palp of a female spider or a harvestman, in which the horizontal and distal parts are alike undivided, represents the primitive Arachnid limb.

In walking, an Arachnid moves nearly together the first and third legs on one side with the second and fourth on the other, according to the observations of Gaubert. Several interesting contributions to our knowledge of the locomotion of the Arthropoda have recently been made, but a consideration of them must be reserved for a future occasion.

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VI.

Death in the Forest.

THE tropical forest is the scene of an intense struggle for life. Undisturbed by man, the trees, climbers, and epiphytes are always fighting for their share of sunlight. The weakest, of course, succumb, making a little more room for the others; and the close observer is painfully impressed by signs of disease and death on every hand. What is most striking is the fact that a majority of the deaths are the result of smothering, blood-sucking, and strangling by other plants. Like men in great cities, each plant is an individual trying his best for himself, no matter what becomes of the others. But there is no one to help the weak or nurse the sick. They must be able to cope with their neighbours or must die. Once weakened, their places are usurped by others, and recovery is impossible.

The loss is greatest in early life. Animals and savages take care of their children in their weakest stage, but not so the forest giant. His seeds are scattered, provided with contrivances to spread them in every direction by means of wind, water, or animals, but beyond this they receive neither care nor attention. Thousands of seeds are produced annually, but rarely does one survive and come to maturity. They germinate in every direction, some growing upwards for a few feet, only to give up the struggle when they find no light is obtainable. In the dull arcades of the forest perpetual twilight reigns, and the seedlings can get neither strength nor colour. They droop and wither, some almost immediately, others in a few months, while hundreds fail after gaining a fair position. They cannot pierce the dense canopy above, and are virtually smothered to death.

Between the tree trunks the ground is littered with fallen leaves, petals, and fruits, crumbling down to that rich brown mould so characteristic of the forest. Except a few voyrias or a saprophytic orchid, not a single herbaceous plant is to be found. Like the tree-seedlings, they cannot exist without sunlight. As in the case of the cave-fish, however, the voyria[1] has managed to live under conditions decidedly unfavourable to others, and even to itself, as may be seen

[1] A small leafless plant, of the natural order Gentianeæ, growing on rotting wood and leaves in the forests of tropical Africa and America.

from its weak, leafless, and almost colourless stem. One species is nothing but a stalk supporting a single flower, like a miniature yellow crocus, while others, with branching stems and several flowers, are even more insignificant.

When a clearing is made, however, the result is almost marvellous. In a month or two it is green with herbage, and if left alone will soon become as densely packed as the roof of the forest. Various kinds of weeds, of species unknown in the neighbourhood before, now dispute with each other possession of the vacant space. It is almost impossible to conceive how the seeds have been brought, as it may be scores of miles to the next clearing where the vegetation is identical. Some, no doubt, have been left by floods, others dropped by birds, or carried by the wind. One of the first to appear is the elegant silver fern (*Gymnogramme calomelanos*) which particularly affects the sides of ditches and every spot where the soil has been turned up. After the ferns come grasses and sedges, then phytolaccas, prickly solanums, and a host of interlacing scrambling vines, until an impenetrable jungle extends over the whole clearing. The struggle that goes on here is intense, and the weakest are soon smothered. The first to give up are the ferns, then follow the grasses, while the prickly solanums dispute every inch with the smothering multitude, scratching and tearing the leaves and stems of the scramblers as if determined not to give way. But all their efforts are useless; the smotherer throws its arching stems over them and on these others lay hold, until a dense mass of foliage shuts out the light from everything below.

Meanwhile seeds of forest trees have found the opening. Without the accident of a clearing they would have gone the way of the myriads that die for want of such an opportunity. But they will have a hard struggle, for already the smotherers are doing their best to prevent anything from growing beneath them. However, one after another they pierce through the tangle, taking advantage of every little crevice, and often growing through those leaves they cannot push aside. Once out in the light they begin to spread and overshadow the choking creepers, which lose their leaves and become less hampering. Here and there one tries to lay hold of a young tree and rise with it, but rarely does it succeed, as there are no branches and the leaves are thrown off one after another as the slender trunk rises upwards. Soon the clearing looks like a young plantation so densely packed as to be almost impassable. The mass of creepers is now nothing but a network of dry stems, which are gradually settling down into the soil.

The young trees now form a crowd so dense that no one has room to spread. The space required by a single tree is occupied by a dozen or more, and something must be done. Then comes the final effort. The slightest advantage of one over another, in strength, height, or expanse of foliage, determines the result, which means

death to the many and conquest to the few. As those which have got ahead stretch themselves over the others the sunlight is excluded and death is only a matter of time. Sometimes the struggle between two trees is a very close one. They are apparently equal in size and height, and as they interlace their branches, there is a continual straining to get ahead of each other. One raises a branch above in one direction, while its antagonist succeeds in covering it in another, and so the fight goes on, neither gaining the advantage or being able to arrive at an assured position.

As may be seen, the difficulties in the way of a seed or young forest tree are enormous. Millions of fruits ripen only to become food for labbas and acouries on the ground, or birds and monkeys in the trees. Most of those in Guiana are light enough to float on the water, and on account of this provision are carried away by floods and deposited at great distances from the parent trees. But the forest is crowded—it cannot find room for more. Unless a vacancy occurs there is no chance for a newcomer. And when this happens, what a struggle for the position! Hundreds of candidates are on the ground, and there is no patronage or favouritism unless man interferes. The one that succeeds may perhaps be the most worthless in the eyes of the woodcutter. A soft-wooded tree, for instance, will grow more quickly and outstrip the others. Various contrivances have been adopted. It seems as if everything has been tried, but rarely have two species chosen the same means to the end. All the trees of the Guiana forest have, however, agreed to avoid branching on the lower part of the stem. If branches are produced in early life, these are very small, and soon drop off so as not to interfere with their upward growth. Every effort is made to reach the common level, while to support the slender trunk and prevent its becoming top-heavy, buttresses are thrown out at the base.

It would seem that if once the forest were occupied, there could never be the slightest chance for another tree, without the interference of man. Such would actually be the case were there no destructive agencies. Once over the drawbacks of infancy, its head on a level with those of its neighbours, the forest giant would have merely to maintain its position. But in reality it has to contend with a number of insidious foes. Here and there in the forest fallen trunks, with no sign on them of axe or other instrument, lie along the ground, and crumble slowly away under the influence of moisture and the attacks of wood-ants and beetle larvæ. Occasionally, the traveller comes upon the scene of a recent downfall. A tree has partly fallen, producing a break in the continuity of the forest, as well as a confused mass of wreckage. In the line of its course, are ragged ends of great limbs, twisted branches, headless palms, and torn bush-ropes. As yet it is partly supported by the mass of foliage, branches, and trunks of other trees which stand in the way, but as these decay it will gradually sink until it becomes a rotting log on the ground.

Rarely does such an accident happen to a healthy tree. When nothing has occurred to weaken it, the branches interlace with those of others in such a manner that a fall is almost impossible. Wood-cutters have often to clear away several trunks before they can fell the one they require. The tree is almost invariably dead before it falls naturally. Perhaps, every branch has become dry and decayed, and then broken away little by little, until nothing remains but a bare stem. The roots also have rotted in the ground and no longer serve as anchors, so that all support is lost at both summit and base. Then comes a heavy rain-fall or flood, and the dead trunk crashes down, carrying away everything in its course.

In paddling up some of the smaller rivers of Guiana dead and dying trees may be seen on every side. In various stages of decay, some straining to keep alive a branch or two, they provide hollows for parrots to build in, and food for the wood-ants, which riddle them with their tunnels, and hang their oval black nests in the forks. Here are the remains of magnificent trunks over a hundred feet high, with bare stems and a few ragged strips of bark, only kept from falling by bush ropes and parasites. Perhaps one has fallen across the creek, seriously impeding its navigation. Festooned with creepers, they do not look so unsightly as might be supposed; while large bushes perched in the topmost forks form, as it were, a flowery pall. Some of the most handsome are the clusias, with their smooth stem and shining leaves, and flowers like single camellias. Some have petals a pure white, others tinted with rose-colour. Yet, with all its beauty, this plant is a deadly foe to the forest tree. In Jamaica it is called the "Scotch attorney," that gentleman being proverbially noted for working plantations to his own profit.

Clusias are not the only stranglers; different kinds of wild figs are quite as destructive. These are also clean and sleek in limb and foliage, but have no showy flowers. Their fruit varies in different species from the size of a pea to that of a cherry, and is greedily devoured by birds. Of a rich dark green, their clumps contrast favourably with the decrepit trees on which they sit, and which they appear to hold together by interlacing aerial roots.

Unable to fight for themselves in the struggle, these stranglers have become established in the forks of the trees, where the birds have dropped their seeds in cracks of the bark. Here, among mosses and in congenial positions, they soon germinate and insinuate their aerial roots into crevices and among the damp epiphytes, adhering very closely to the tree, but deriving no nourishment from it. Slowly they creep down towards the earth, broadening as they lengthen, while branches and leaves are developed above. To all appearance they are quite harmless, apparently taking advantage of their host merely to get a share of the sunlight, and, like the thousands of epiphytic orchids, bromelias, arums, ferns, and mosses already resting securely on their immense limbs, causing no injury. Never-

theless, these climbers are preparing for a struggle in which the apparently weak will overcome the strongest monarch of the forest. If the seed has germinated in a suitable fork, its aerial roots flow down on every side, like narrow streams of green pitch, accommodating themselves to every irregularity, and only stopping when they reach the earth, and push their roots into the rich mould. Then the little plant becomes a large shrub, day by day increasing in strength, until, by its dense shadow, it kills every epiphyte in its neighbourhood. Then the aerial roots enlarge until they resemble monster pythons hanging downwards instead of coiling round the tree, which now begins to feel the power of its enemy. From every clinging root side-roots are developed, which unite until, in some places, the whole trunk is encircled.

In forest trees the seat of life is just inside the bark, where, as is well-known, the growth of a season is shown by a ring of new wood. Such trees can be killed by ringing the bark with an axe, or by constriction at the base, and it is by the latter process that the clusia or fig performs its murderous work. As it becomes more luxuriant and tightens its bands, the flow of sap is checked and the leaves begin to fall off. This is greatly to the destroyer's advantage, as it could not get enough light while every inch was occupied by the canopy of foliage. Unlike many epiphytes, it enjoys the blazing sunlight and has developed all its powers to attain a position where this may be secured. Its leaves are thick in texture, and even the flowers of the clusia do not shrivel in the sun like those of most plants. Few forest trees are so well fitted to endure the intense glare, none look so strong and healthy under the influence of a cloudless sky, and the consequent drought for two or three months.

Leaf after leaf falls, until the tree becomes too weak to ripen seeds and bears but few flowers; soon nothing is left but bare twigs. That it is struggling in the embrace of its enemy may be seen by the way the bark swells between its interlacing bonds. Like an athlete straining to break the cords which bind him, the tree tries to rid itself of the living fetters. All its efforts are, however, as useless as those of an ox in the python's coils. It almost seems as if the strangler feels the powerful strain, as every now and again it draws another band round its victim. Sometimes the whole trunk becomes covered with a living pillar, only broken here and there by a few meshes, but more generally the bonds are not so complete. In the former case, death ensues very quickly; in the latter, the tree may linger for years. At times only a single limb is killed, after that a second, and so on, as its foe increases in strength. If the latter be perched on a branch overhanging a creek, the tree may escape without much harm, as most of the aerial roots will descend straight without touching the trunk.

After the tree becomes bare death soon ensues, and then it begins to break up. Twigs and branches fall by their own weight

or that of the epiphytes upon them. Then the bark begins to peel off and fall in great flakes which carry with them masses of orchids and bromelias. Beetles deposit their larvæ in the now rotting trunk, and wood-peckers make holes to get at them. Water trickles down into the hollows and makes them larger and larger until some day parrots build their nests there. Meanwhile, those destructive little creatures the wood-ants, or termites, are mining everywhere until the wood is permeated with tunnels only separated from each other by paper-like partitions. It takes many years before a large trunk is crumbled down under the influence of heavy rains and wood-ants, and during this time it runs the risk of falling, which means death to the strangler, since, deprived of its support, it cannot rise again.

On the sand-reefs which extend in front of the forests of Guiana, the struggle for life is less intense, as few trees can get over the difficulties attendant on a stretch of white sand heated by the burning sun. Seeds of trees that have been accustomed for generations to germinate in the damp gloom cannot endure this furnace-like heat. Clusias, on the contrary, are well fitted for such conditions, and grow to perfection on the reefs. Accustomed to throw their aerial roots downward, this faculty becomes very useful, as it enables them to reach the cool depths where moisture accumulates and is retained, even in the driest season. Among the half-starved looking bushes which grow in clumps here and there, the various species of the clusia family are conspicuous for their healthy appearance. Here, as elsewhere, they allow no epiphytes or parasites to rest on their smooth limbs.

Beside the stranglers there are those veritable blood-suckers, the many species of *Loranthus* and *Phoradendron*. These are allied to the English mistletoe, and like it parasitic. But the mistletoe works in a very quiet way as compared with its tropical cousins. Here in the cultivated districts of British Guiana thousands of fruit trees are infested with loranths, and many may be seen in the various stages from weakness to death.

Like those of the wild figs, the seeds of the parasites are distributed by birds. In wet weather they may be seen germinating on every fence—little discs with two mistletoe-like seed-leaves. The pulp round the seed is glutinous and keeps it from falling until the sucking disc, which answers to the root, takes a firm hold. Those that germinate elsewhere than on the living tree soon wither, but the others take firm hold and soon extend their aerial roots up and down the branch or twig. These are provided with suckers at intervals of about an inch, by means of which the juices of the tree or shrub are extracted until the loranth begins to rapidly spread in all directions. Its long whip-like branches wave in every breeze and are often carried near other trees or other boughs of that on which it is located, seizing them by its aerial roots which coil spirally round them for a foot or more, until they are tapped by a dozen suckers. In this

manner the octopus-like arms are extended everywhere, and the loranth grows into a great bush.

It is not, however, to the interest of the loranth that the tree should die, but rather live as long as possible. It therefore avoids the great limbs and larger branches, confining itself to the smaller twigs. Nevertheless, a branch is often killed by the continued depletion, and if the loranth has not succeeded in laying hold of others it soon withers. Sometimes every branch is infested and then the whole tree must succumb, to the utter destruction of its destroyer. As in the case of the strangler, flowers are at first produced without fruit, then the tree gets too weak to bloom, and finally leaf after leaf drops until all are gone, and the branches themselves die and fall off. When the tree is covered with foliage the loranth is hidden, but when it is bare the enemy can be seen in all its luxuriance. Where trees are close together, as in the forest, it rarely happens that one is actually killed in this way, as the loranths attach themselves to several individuals. Nevertheless, in such an intense struggle as that of the forest any element of weakness is a dangerous handicap to the individual, and will ultimately cause its defeat.

Where there are no deciduous trees, it follows that one with its twigs entirely bare is a very striking object. Everything around is most luxuriant, while the victim of the strangler or parasite stands like the mummy at a feast. This does not last long, however, as bush ropes soon cover it with garlands of flowers, and in a year or two young trees shut out the sight.

Palms and other endogens escape both clusia and loranth. Sometimes a fig will establish itself in the crown, and even encircle the stem, without the slightest injury to its host. The strangler may encircle and compress it, but the vital part is unaffected, being well in the interior and protected by very hard and tough surroundings. Moreover, the immense fronds or leaves are too hard and dry to be congenial to the loranth. Unfortunately, however, it is not entirely secure, although it folds frond after frond over the tender terminal bud. The borer beetle finds out its weak point, pierces all the wrappings, and deposits its eggs, with the result that the heart is soon devoured by larvæ. Then the fronds drop off and nothing remains but a bare stem like the pillar of some ruined temple lost in the forest.

Accidents cause the death of many a forest tree on the river bank. High floods soften the mud, so that when the trunk leans over and is weighted by a burthen of creepers, it is often borne down and even carried away by the current, if the timber is not too heavy to float. Sometimes, on a creek, the tree will be entangled in the branches of its opposite neighbour, and manage to recover itself by throwing out fresh roots. When the wood is heavier than water and the tree settles down and dies, its trunk remains as an obstruction for a very long time, perhaps centuries. When the creeks are very low, these old trunks or tacoobas are seen lying at the bottom in the utmost confusion. Nothing

remains but the heart-wood, which is very hard, and may last for an indefinite period at the bottom of the stream, as wood-ants and other destructive agencies are unable to finish the work of destruction under such conditions. Some of the tidal creeks are so choked with these tacoobas as to be impassable at low water, while in others great care is required. With a stream like a mill-race, the bateau runs a risk of being bored at any turning, while every few minutes it bumps and grazes the sunken logs. Sometimes large trees float down the river and are carried out to sea.

The forest trees of Guiana seem to have lost the power of shooting up afresh from the roots when injured. It is true, as mentioned above, that a tree rarely breaks off or is torn up by the roots until it is actually dead. Storms are rare, and tornadoes unknown, but it sometimes happens that one of the tallest is struck by lightning. Again, the fall of every rotting trunk kills or seriously injures everything that stands in its way. Great limbs are torn off, leaving unsightly gashes, which sometimes heal, but as often fester, ultimately producing holes which continually grow larger as water finds its way into them. When once the outer bark and sap-wood is seriously injured, beetle larvæ and wood-ants soon carry on their work of destruction, and death must ultimately ensue. At first sight it would appear as if when the tree is in the embrace of the strangler, or bleeding to death under the thousand suckers of the loranth, it would shoot up from below and try to recover its position, but such is not the case. The tree has had to fight its way upward for ages, and all its powers are concentrated for this purpose.

The natural vegetation of a country is well fitted to cope with all the difficulties of its environment. Parasites have been developing their powers for ages, but never succeed in making any serious inroad on the forest. As the enemy increases in strength and malignity, its victim gains more power of resistance. With plants from other countries, however, it is different. Taken from their particular environment to another, against dangers of which they have no protection, they suffer greatly. East Indian fruit trees, notably the Mango, are often diseased, while ornamental shrubs are as frequently infested by loranths. Rarely is a forest tree killed by umbrella-ants, but almost every imported plant is destroyed by them. Some districts in Guiana are so infested by these leaf-cutters, that cultivation is impossible. In early times we read of plantations being abandoned on that account. It was useless to attempt any cultivation, since myriads of these little creatures carried away the young leaves as fast as they grew. Butterfly larvæ are equally destructive to foreigners. In a day or two, a fiddle-wood tree (*Citharexylon*) will be stripped perfectly clean by swarms of these voracious creatures. Rarely does the tree recover from such an attack, although it sometimes succeeds in doing so. The sudden defoliation of a whole tree

is so severe a check to its growth, that even if it should overcome one such calamity, it is weakened for a long time afterwards.

Imported fruit trees, and especially palms, are much infested by scale insects. These cover the under surface of the leaves and suck the juices, causing sickness, and even death. Often they are accompanied by fungoid growths, which hasten the end. The mango is much infested by both scale insects and fungus, and may live in this condition for many years. The under surfaces become spangled with a pretty star-like scale, and as this carries on its work of depletion, the foliage begins to look dirty and unhealthy, while in its later stages the fungus flakes off. At first the tree loses its power of ripening fruit, then no flowers are produced, and finally it may linger for years a centre of infection to others, until the owner chops it down as an unsightly pest. It does its best to get rid of its parasite by dropping leaf after leaf, and developing new ones, but both scale and fungus multiply and attack them almost as soon as they unfold.

Unlike some other countries, Guiana has few herbivorous animals. Tapirs and manatees do not interfere with the forest trees, and even the rodents confine themselves to fruits or water plants. The young seedlings are never bitten off, nor is the bark gnawed. It follows, therefore, that protective measures against the larger animals are unnecessary, and every effort can be used for other purposes. Unhampered by outsiders, the plants only contend among themselves, thus contrasting with European vegetation, where thousands of signs indicate a struggle with outside enemies which in the tropical forest are almost entirely wanting.

James Rodway.

VII.

Some Recent Researches on the Movements of Diatoms.

THE announcement a short time ago of the discovery of supposed pseudopodial processes in Diatoms by Mr. J. G. Grenfell, in the *Quarterly Journal of Microscopical Science* (October, 1891) has excited much criticism from various sides, some of which may be found summed up in a short note on the subject in NATURAL SCIENCE for April, 1892 (p. 94). In the words of Mr. Jabez Hogg, quoted in that notice, the processes in question "are, in no sense of the word, pseudopodia, neither are they, nor can they be regarded as, organs of locomotion; and their discovery—which, by the way, is a very aged one—throws no light whatever on the debatable question: the movements of diatoms."

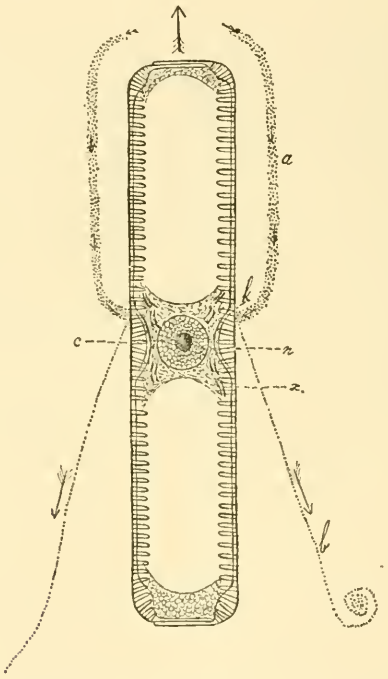
In March of this year a paper was read by Professor Bütschli, of Heidelberg, before the "Naturhistorisch-Medizinischer Verein" of that University, containing a preliminary account of some researches carried on by himself and a student, Herr Lauterborn, upon the movement of a large species of Diatom.[1] The results obtained are so novel and important that, from this reason, as well as from the fact that the journal in which they are published is one not easily accessible to English readers, no further excuse is needed for bringing them before the notice of the general scientific public.

The researches in question were conducted on a large species of *Pinnularia*, which Herr Lauterborn identified as *P. nobilis*. M. Schultze had already shown that in various Diatoms foreign bodies could often be seen gliding along the so-called *raphe* in the middle line of the side of the shell, and concluded from this and other observations that the shell valves could be opened for a longer or shorter distance along the course of the raphe, and that here the protoplasm, which plays a part in the movement, comes to the surface. This observation was frequently repeated, and finally the authors were able to obtain more exact results by placing specimens of *Pinnularia nobilis* into water containing Indian ink. By careful observation of the Diatom in side view, it could be seen that at the ends of the raphe nearest to the so-called *noduli* in the middle of

[1] Verhandl. Naturhist.-Med. Vereins zu Heidelberg, *n.s.*, vol. iv., p. 5.

each shell valve, little clumps of fine granules of Indian ink were formed, which increased gradually by the addition of more granules. After a time it could often be observed that a fine thread shot out from the heap of granules, which ran along the raphe, though not directly upon it, to one end of the diatom.

The investigation of a specimen lying upon the zonal side gave a clearer insight into the real course of the phenomenon (see Figure). From in front of the diatom a stream (*a*) of loose granules, each separately suspended in the liquid, flows along each side in a backward direction towards the nodal point of the raphe (*k*). At this spot



Optical section of a *Pinnularia nobilis* in movement, in zonal view. *n*, Nucleus; *c*, the Centrosome; *x*, the peculiar double threads in the protoplasm; *a*, the granules flowing back to the nodal point (*k*) of the anterior raphe; *b*, the jelly thread shooting out backwards, which has rolled itself up into a tangle on the right side. The arrows show the direction of the movement of the Diatom, the granules, and the thread.

All the details drawn in the figure can be plainly observed in the living Diatom.

they become fastened together by an invisible connecting medium, and then continue to travel backwards united into a thread (*b*), which passes obliquely outwards and backwards, becoming thus further and further removed from the surface of the shell-valves. As the thread is formed the diatom moves forward in the direction of the arrow. It is evident that here we have to do with a thread which is simply made visible by the sticking together of numerous Indian ink granules. This can be recognised (1) by the regular arrangement of the granules in rows, (2) by the cessation of the usually so lively

molecular movements as soon as the granules are united with the thread, and (3) from the circumstance that the end of the thread sometimes winds itself up into a tangle. Moreover, the thread not unfrequently appears interrupted for a stretch, owing to the Indian ink granules being wanting for some cause on this portion of it, it may be on account of less adhesiveness of the thread, or from want of granules at the moment of its formation. Usually the thread is long enough to project back beyond the diatom ; at times, however, the formation was observed of unusually long threads, far exceeding the whole length of the diatom. But after a time the threads break off, together with the clumps formed by rolling up of their ends, and then they remain still while the diatom continues to move forwards. The formation of the thread proceeds by jerks, which harmonises with the peculiar jerking movements of the diatom.

Sometimes, instead of a thread on each side running backwards, as when the diatom is gliding forwards, one thread runs backwards, the other forwards, and the diatom revolves. In some instances it was possible to follow the behaviour of the threads during a reversal of movement. The threads carried at their extremities a clump of ink granules. After the diatom had come to rest, it could be observed that the clump was drawn back to the nodal point of the raphe, apparently by shortening of the thread. In a short time a new thread was developed, pushing out the clump of granules at its extremity, but in the opposite direction to the former thread. The diatom then began to move in a direction opposite to that it formerly took.

From these observations Professor Bütschli draws the following conclusions :—" It can hardly be doubted that the jerking elongation of the threads stands in causal connection with the locomotion of the diatom. Now, since it is known that diatoms, as a rule, only move upon a solid surface, it would be an obvious assumption that the threads fix themselves in some way upon the surface, and by their elongation push the diatom forwards in jerks. On the other hand, however, the rocket-like shooting out of the threads would be quite sufficient, even without fixation of their ends, to explain, by means of the resistance of the water, the jerking progress forwards of the diatom. To me, indeed, the latter explanation appears to have more in its favour, since we never could determine with certainty any fixation of the threads to the underlying surface ; and the above described tangle of their ends, and, further, the pulling back and apparent drawing in of the thread during change in the direction of the movement, speak against such a fixation. There would always be a possibility that the peculiarity of our fresh-water diatoms, of moving only upon a solid surface, is connected with the fact that they cannot as a rule float. We should then have to refer the cause of diatom movement to an abundant production of sticky jelly, which shoots out quickly and with some force at the nodal points of the

raphe in the form of fine threads. Then the movement of diatoms would have a great resemblance to that of Desmidiaceæ, which, according to the investigations of Klebs, similarly move with the help of secreted threads of mucus."

The threads would be completely invisible, owing to their transparency and similar refrangibility to the surrounding water, if it were not for the peculiarity of ink granules sticking to them, and it was not found possible to colour them with methyl blue, Congo red, or other aniline stains. Sometimes, however, the adhesiveness appears to be wanting, since frequently nothing could be seen of the threads in actively motile specimens, and the same was the case in a series of other diatoms examined, *e.g.*, various *Navicula* species, &c. Perhaps, in these cases, the secreted jelly dissolves quickly again, and the formation of the threads observed in *Pinnularia* is suppressed.

The invisibility of the jelly, without application of special methods, is further shown by the fact that, in the investigation in Indian ink of *P. nobilis* and other species of the genus, it could be demonstrated that these forms are, as a rule, covered with a more or less considerable coat of jelly. It is only recognisable by the fact that the Indian ink does not penetrate the region of the jelly envelope, and even with the application of this method no sharp boundaries to the envelope could be determined with the strongest magnifications. In the motile specimens it could at least be seen that the jelly was interrupted at the nodes on the sides of the shell, so that no hindrance was opposed to the exit of the thread. The anterior current of ink granules takes its course to the nodal point of the raphe along the outside of the jelly envelope, as a result of which it always remains at a definite distance from the surface, as shown in the figure, and first sinks into the raphe at the nodal points. From the presence of this envelope the question might arise whether the supposed jelly thread is not really a thread-like tract of jelly which passes along the surface of the shell, and first becomes free posteriorly, but the authors believe this is not the case.

Finally, the authors promise us a continuation of these observations, to which all interested in microscopic animal and plant life will eagerly look forward.

In these researches we have an entirely new light thrown upon what has always been so great a puzzle, the movements of Diatoms, and it is obvious that any form of pseudopodia is quite unnecessary and inadequate to explain it, thus quite justifying the words of Mr. Jabez Hogg quoted at the commencement of this article.

E. A. Minchin.

SOME NEW BOOKS.

Darwin and after Darwin. An Exposition of the Darwinian Theory and a Discussion of Post-Darwinian Questions. By George John Romanes, M.A., LL.D., F.R.S. I.—The Darwinian Theory. 8vo. Pp. 460. London: Longmans, Green & Co., 1892.

The undertaking originally planned by Mr. Romanes was sufficiently extensive. It was nothing less than a complete survey of the doctrine of organic evolution from its beginning in the earliest recorded times down to the present day. He intended to elaborate and expand the material of the course of lectures delivered by him in the Royal Institution in the years 1888 to 1890 into a systematic treatise in three separate volumes. The first part, dealing with pre-Darwinian evolution, is indefinitely postponed, wherefore the title "Darwin and after Darwin" has been substituted for "Before and after Darwin," the title of the series of lectures. The second part is contained in the volume before us, and is confined to the Darwinian theory, while the third part will follow soon under the sub-title Post-Darwinian Questions.

The author, in his preface, proceeds to tell us that the present volume is thus intended to be merely a systematic exposition of what may be termed the Darwinism of Darwin, and that, as on this account it is likely to prove of more service to general readers than to professed naturalists, he has been careful to avoid assuming even the most elementary knowledge of Natural Science on the part of those to whom the exposition is addressed. One would suppose that the Darwinism of Darwin meant Darwin's own argument and his own illustrations, and would prepare to judge the book according to the soundness of the exposition and the force and lucidity with which it appeals to the average intelligence of those who have been unable to study, or unable to understand Darwin's own writings. But on reading the book itself we find that though it may be said to expound the Darwinism of Mr. Romanes, there is much in it which is not derived from Darwin, while much that is most characteristic of Darwin is only conspicuous by its absence.

In fact, in the introduction a different description is given of the purpose and character of the book. The author here states that he wishes to present the whole theory of organic evolution as he believes that it will eventually stand; that his endeavour is to exhibit the general structure of the theory in what he takes to be its strictly logical form, rather than to encumber any of its parts by a lengthy citation of facts. The present volume is concerned with the theory from the appearance of the Origin of Species in 1859 to the death of its author in 1882. It is to be a condensed and critical statement of the main evidences and the main objections which have thus far been published with reference to the distinctively Darwinian theory, the chief novelty being the pictorial illustration of the facts on which the arguments are based.

The book, however, does not correspond to this description much more exactly than to the other. Section I., which is more than half the volume, is devoted to what the author calls "the main evidences of organic evolution considered as a fact." The theory of organic evolution is not the distinctively Darwinian theory.

The view expressed at the commencement of the "Origin" is that, however cogent the evidence that organic forms have been evolved by descent, the demonstration of that truth would be unsatisfactory without an explanation of the manner in which adaptations and perfection of structure had been acquired. Accordingly, nowhere throughout the "Origin of Species" is the evidence for the occurrence of evolution considered apart from the evidence for Natural Selection; and the same thing is true in Darwin's other books. There were evolutionists before Darwin who knew nothing of Natural Selection, and many of his adherents and disciples have shown more interest in the course and the details of evolution than in its causes. The most prominent of these was Haeckel, and the first section of Mr. Romanes' book may with greater truth be described as Haeckelism than as Darwinism. The distinction between the occurrence of evolution and its explanation, is, of course, a most important one, but no one has pointed out more clearly and emphatically than Haeckel that Darwin's title to glory rests on his contributions to the latter, and not on his discovery or establishment of the former. It is astonishing that Mr. Romanes should have drawn so largely from Haeckel's writings, and in particular from his "Schöpfungsgeschichte," and yet have ignored the truth of the following paragraph which occurs in that work:—"Darwin's merit is over-estimated when he is regarded as the founder of the Theory of Descent, or of the whole of the Theory of Development. We have seen from the historical sketch in this and the preceding chapters, that the Theory of Development, as such, is not new; all philosophers who have refused to be led captive by the blind dogma of a supernatural creation have been compelled to assume a natural development. But the Theory of Descent, constituting the specially biological part of the universal Theory of Development, had already been so clearly expressed by Lamarck, and carried out so fully by him to its most important consequences, that we must honour him as the real founder of it. Hence it is only the Theory of Selection, and not that of Descent, which may be called Darwinism."

In expounding the evidence on which the Theory of Descent is based, Mr. Romanes, like Haeckel, also endeavours to disprove the theory of Special Creation. This was natural enough in Haeckel's work, which included a survey of the history of the subject; but Mr. Romanes makes no attempt at historical treatment. He contrasts the logical consequences of the two theories with one another, and with the facts of organic nature; but it is doubtful whether the proof of the evolution theory is in any degree made logically stronger in this way. My own opinion is that it is not. As the creation theory has no strictly logical basis, so it has no strictly logical consequences. Let us take, for instance, one example of Mr. Romanes' method—his discussion of the controversy between Mivart and Darwin concerning the eye of the octopus. He says, when it is proved that the eye of the cephalopod is of quite a different plan of structure from that of the vertebrate, the special creationist can only reply that it may have pleased the Deity to form a certain number of ideal types and never to allow the structures occurring in one type to appear in any of the others. He argues, then, that in that case we should

expect the same typical structures to be always present within the limits of the same type; and, as this is not the case, the hind limbs being absent, for instance, in the whales, the assumption of the creationist must be abandoned. But this is obviously a *non sequitur*. It would be equally true to say that the absence of hind limbs in the whale proves that the animal does not belong to the vertebrate phylum.

Even if we consider the facts of embryology, which are the most striking evidence of the truth of evolution, it is not possible to show that they are absolutely incompatible with the creation doctrine. The creationist may say, if he pleases, that when man was created it was ordained that he should pass through fish-like stages in his embryological development, so that the ideal unity of the vertebrate phylum might be more perfectly maintained. It is impossible to disprove such an assumption. The logical reply to the creation hypothesis is that it assumes a multitude of miraculous occurrences for which there is absolutely no room in Nature, so far as we know it by human observation and reasoning; while at the same time the hypothesis as upheld by naturalists in Darwin's time, including a succession of creative epochs, could lay no claim to support from supernatural revelation.

Mr. Romanes' restatement of the argument for evolution is divided into five chapters, containing the evidences from Classification, Morphology, Embryology, Palæontology and Geographical Distribution respectively. Of these, the chapter on Embryology is by far the worst, being unfortunately crowded with errors and inaccuracies. The first case described of the persistence of ancestral organs in the embryo, is that of the external gills of the fœtus of the Alpine Salamander. This case is mentioned by Darwin on the somewhat unsafe authority of G. H. Lewes, who wrote that the gills of the fœtus had no reference to the future life of the animal, nor any adaptation to its embryonic condition, that they had sole reference to ancestral adaptations, and repeated a phase in the development of its progenitors. Mr. Romanes still supposes that this is a correct account of the matter, although it must be evident to anyone who has studied Fraülein von Chauvin's paper on the subject that the fœtal branchiæ have been specialised and enlarged for the purpose of intra-uterine respiration. It is true that the fœtal branchiæ are capable of aquatic respiration outside the body of the mother, but it is also true, although not mentioned by Mr. Romanes, that the great size and delicacy of these branchiæ were the actual cause of death to all the larvæ with which Fraülein von Chauvin experimented except one, and that one escaped the fate of its brethren only by getting rid of its uterine gills and growing new ones of more serviceable size and character.

A large number of pages are next devoted to a detailed discussion of the relations of the Metazoa to the Protozoa, a discussion which it is very difficult to criticise. It is certainly interesting, but it also certainly belongs in many respects to the post-Darwinian, not the Darwinian period. The discussion is difficult to follow, and I fear will not be thoroughly intelligible to the general reader who is destitute of even the most elementary knowledge of Natural Science, for it requires some acquaintance with Weismann's theories to understand the author's meaning. He says, for instance, that not only the individual development but also the powers of asexual reproduction on the part of multicellular organisms are all ultimately due to the specialised character of their germ-cells. He points to the karyokinesis of the segmenting ovum and the formation of the polar

bodies as evidence of this specialised character, and then proceeds to consider to what extent these peculiar complicated phenomena are represented in the Protozoa. Of the expulsion of polar bodies, he says that nothing resembling it has ever been observed in any of the Protozoa—an unjustifiably dogmatic statement, inasmuch as portions of the nucleus have been observed to be ejected in the conjugation of the Ciliata. One of the general conclusions drawn from the discussion is that no line of real demarcation can be drawn between growth and reproduction, even of the sexual kind. But the course of the argument has by no means succeeded in filling up the chasm of difference between the division of one cell into two, which is growth, and the union of two cells into one, which is sexual reproduction.

The rest of the chapter is devoted to the embryogeny of the Metazoa beginning from the fertilisation of the ovum. In the description of the characters of ova an unfortunate error is made, the pores of the zona radiata in the Mammalian ovum being referred to the same category as the micropyles of other ova.

The majority of the instances of embryological correspondence described are derived from Haeckel's more imaginative writings, but Mr. Romanes is placidly unconscious that doubt has been thrown on the objective reality of any of them. He does not even quote Haeckel's fascinating, though sometimes unfortunate, speculations with strict accuracy. On one page he figures the gastrula of the zoophyte *Gastrophysema*, and on another gives two figures of *Prophysema primordiale*, an extant gastrula-form, all "after Haeckel." As far as I have been able to discover, Haeckel never used the name *Prophysema* at all. The figures described under this name are copied from those of *Halophysema primordiale* in Haeckel's paper "Die Physemarien," and were proved 12 years ago to represent an organism specially created by Haeckel himself, *Halophysema* being really a reticularian Protozoan. *Gastrophysema* was another genus of Haeckel's imaginary extant Gastræa, and I have been unable to discover where among Haeckel's works Mr. Romanes found a reference to the gastrula of a zoophyte called by the same name.

It is stated that probably all the Metazoa pass through the gastrula-stage, which is correct, if by the latter term is merely meant the diploblastic condition. But only one process by which this condition is reached, namely, invagination, is described, and the gastrula of Olynthus, "after Haeckel," is figured without any mention of the fact that it is not produced by invagination. Another figure is described as the "gastrula of an Arthropod (*Nauplius*)," as though *Nauplius* were a genus.

Still more extraordinary, however, is the foot-note on p. 139, which states that in most vertebrated animals the process of gastrulation has been more or less superseded by another process called delamination, but that even in the higher Vertebrata embryologists are pretty well agreed as to delamination having been merely a later development of, or possibly an improvement upon, gastrulation. Perhaps the author has confused Professor Lankester's theory that gastrulation is originally derived from the delamination observed in some Coelenterata, with some faint reminiscence of the modified epibolic gastrulation of fishes and birds. The rest of the chapter is still more strongly permeated with Haeckelism. We have the figure of Haeckel's ideal primitive vertebrate, and two pages of the well-known somewhat ideal figures of embryos of fish, pig, man, etc. On p. 147 we note the peculiar statement that the gill-slits are supported

internally by gill arches, or the blood vessels which convey the blood to be oxygenised in the branchial apparatus. It is really a pity that the author was not more careful to prevent misconception on the part of the general readers for whom his book is intended. Mr. Romanes claims to belong to a class of naturalists whom he calls specialists in Darwinism, and says that the opinion of those who have done good work in other departments may be destitute of value in questions of evolution. However this may be, it is certain that the specialist in Darwinism who undertakes to expound the embryological argument for evolution should not rely almost exclusively on the writings of Haeckel. He would find Balfour's "Comparative Embryology" a safer guide. Mr. Romanes would have been wise to submit this unfortunate chapter to the revision, if not of a specialist in embryology, at any rate of some zoologist with a competent knowledge of that department, for instance to that of his friend Professor Lankester, whom he quotes with great admiration.

The second more truly Darwinian section of the book is much more satisfactory than the first. The theory of Natural Selection is clearly and fairly stated. It is pointed out that in its main elements the theory is merely a statement of observable facts, the fact of the excessive rate of reproduction leading to the constant pressure of numbers in each species, the fact of competition, or struggle for existence and consequent survival, the facts of hereditary transmission and individual variation. We welcome the candid admission that the theory only explains changes in organisms so far as these changes are of use; in other words, that it is a theory of the origin of adaptations, not of species. This admission occurs in an interesting discussion of misconceptions of the Darwinian theory which are current, not among its opponents, but among its supporters. Another of these misconceptions is that the theory can explain all cases of modification, whereas in some cases it is not logically possible that it can apply; others are that it follows deductively from the theory itself that Natural Selection must be the sole means of adaptive modification, and that all hereditary characters are necessarily due to Natural Selection.

In the chapter on the evidences of the Theory of Natural Selection we have three general or main arguments:—The observed fact of the extermination of forms in the struggle for existence; the consideration that we cannot find an instance of a structure or instinct developed for the exclusive benefit of another species; and, thirdly, the facts of domestication. This last class of facts is illustrated by fourteen pages of figures drawn from actual specimens of domestic breeds, illustrations which form the most novel and distinctive feature of the book. These figures, although not very beautiful, are certainly of great practical use, and enable one to appreciate the peculiarities of the several races better than the most elaborate description without figures. As far as we can judge, they are not only vigorous, but accurate.

Then follows a discussion of some of the detailed applications of the theory to the explanation of adaptations, the author having decided to select all his instances from a single class, namely, that which may be generically termed defensive colouring. This choice is, it seems to me, extremely unfortunate at a time when so many popular treatises have just been produced on the same particular subject. As we have so recently had to study Mr. Poulton's discussion of the subject on one side, and Mr. Beddard's on the other, it would have been a relief to find in Mr. Romanes' book the exposition

of some other class of adaptations. Mr. Romanes has little that is new or original to say on this familiar subject, and we find the well-known cases treated in the same familiar way. The resemblance of the butterfly *Kallima* to a leaf is illustrated once more, here in an uncoloured figure, which contrasts to great disadvantage with the splendid chromo-lithograph in Mr. Beddard's book. As usual, no information is given concerning the actual habits of *Kallima* in its natural environment, the particular leaves which it resembles, or the enemies which it deceives. The last sensational discovery by Mr. Sclater, described by Mr. Poulton, of a homopterous insect whose body mimics a leaf-cutting ant, together with the leaf it carries, is here reproduced; but, as in the original description, no attempt is made to show that the mimicry is of any advantage to the insect which exhibits it. Another case given is the imitation of a venomous snake by a non-venomous one, and considering the difficulty, even to a naturalist, of distinguishing at first sight the innocence or otherwise of any snake met with in its native home, it would have been by no means superfluous if this case had been clearly proved to be one of true mimicry.

Chapter IX. is devoted to criticisms which have been put forth of the theory of Natural Selection, and to some at any rate the most interesting part of this chapter is that which deals with the electric organ of skates and rays. Here the author, after giving an interesting and detailed account of the structure and relations of these organs, and discussing very impartially how its evolution can be explained on the theory of Natural Selection, confesses with a candour which does him the greatest credit that in the present state of our knowledge such an explanation is quite impossible. He even goes so far as to say that if a number of such cases could be produced, the theory of Natural Selection would have to be discarded.

The last chapter deals with the theory of Sexual Selection, which the author warmly defends against Mr. Wallace's criticisms. Mr. Wallace's chief objection is that if the secondary sexual characters supposed to be due to Sexual Selection are not necessarily correlated with that fitness which nature selects, then the fit that do not possess them would be selected, while if they are so correlated then it is nature which really selects them and not the opposite sex. Mr. Romanes states what in his judgment would have been Mr. Darwin's reply, and in so doing presumably gives his own. It amounts essentially to this: that secondary sexual characters are of too definite and elaborate a kind to be regarded as the mere concomitant of that exuberant health and vigour which are the result of Natural Selection. It may be admitted, says the author, that a general brilliancy of colour might accompany a general increase of vigour, but it does not follow that the particular disposition of colours in the form of ornamental patterns, and also elaborate special structures, can thus be accounted for by Natural Selection. In this contention we entirely agree. But Mr. Romanes is by no means successful in avoiding the other horn of the dilemma. He merely says that "in the phenomena of decorative colouring (as distinguished from merely brilliant colouring), of melodious song (as distinguished from merely tuneless cries), of enormous arborescent antlers (as distinguished from merely offensive weapons), we have phenomena which cannot possibly be explained by the theory of Natural Selection; and, further, that if they are to be explained at all, this can only be done, so far as we can at present see, by Mr. Darwin's supplementary theory of Sexual Selection." It seems to us that a much more

forcible answer than this can be made, to wit the following: Mr. Wallace has been to some extent misled by the influence of language, Natural Selection and Sexual Selection being unconsciously conceived by him as independent powers or causes. He has forgotten that, however successful an individual male may be in the struggle for existence, he cannot bequeath his superior endowments and faculties to offspring unless some female appreciates him sufficiently to grant him the favours of sexual intercourse. Mr. Wallace argues as though the superior male, *facile princeps* in the competition for a living, could found a line of descendants inheriting his own health and vigour without female assistance, and could afford to scorn and ignore the petty female standards. On the contrary, the fact is that there is competition in love as well as in life. It is an observed fact that males, in very many instances, fight together for the possession of the females, and, in other cases, compete with one another by the display of adornment or the efforts of song for the favour of the opposite sex. It is also an observed fact, in many cases, that where this sexual competition is most conspicuous there the secondary sexual characters are most developed, and that competition is conducted solely in respect of these characters. A male that excels in the struggle for existence is a complete failure, so far as the species is concerned, unless he can succeed also in finding mates. In view of the fact that competition for mates occurs among those males which have already survived the process of Natural Selection, and are all able and eager to beget offspring, it is a certainty that those will leave most offspring to succeed them who are most able, either by force or charm, by weapons or adornments, as the case may be, to constitute themselves the fathers of the next generation. Without going so far as Mr. Romanes, and saying that this is the argument that Darwin would have used, I venture to think it is more consistent with the Darwinian method and doctrine than that employed by Mr. Romanes himself. At the same time, the formulation of the argument is not intended to imply any admission that we regard the actual selection of individuals as the essential and most important result in sexual any more than in "natural" or general competition.

Subsidiary objections of Mr. Wallace's are, that there is no evidence that hen birds, for instance, are charmed by the beauty or voice of the males, and that, if there were, it would be necessary to the theory that the taste of the females should be uniform in all individuals and constant throughout many generations. Mr. Romanes' reply is practically what he condemns in another place as the argument from ignorance. We do not know, he says, what sentiments may be in the mind of a hen, and as for the constancy and uniformity, we know very little about the psychology of the lower animals. Here, again, I would venture to suggest a better reply. We do not require to assume an æsthetic sense in the hen birds. Without going deeply into psychology it is obviously probable that it is simply the sexual desire which is alone concerned in Sexual Selection. This desire is usually not easily excited in the female, and the function of subsidiary sexual organs and their display or exertion is probably enough the excitement of this desire, without which that of the male cannot be gratified. And the explanation on this view of the constancy of the selection in the same species is merely heredity. The sexual desire of the female has a hereditary association with certain sensory stimuli, and the means of furnishing these stimuli are constantly reproduced and improved by inheritance in the males. I will not develop these considerations further. They are, perhaps, too much beyond the

scope of the distinctively Darwinian theory to be relevant to Mr. Romanes' book.

The volume concludes with an appendix and notes, of which the former is a criticism of objections to evolution on palæontological grounds.

After noticing the salient features of the book at such length, it is unnecessary to add much concerning it as a whole. I will, therefore, simply finish with the opinion that while it is scarcely to be recommended to the unscientific reader as an introduction to the general philosophy of biology, it contains, notwithstanding its defects, much that is interesting and suggestive to the biologist.

J. T. CUNNINGHAM.

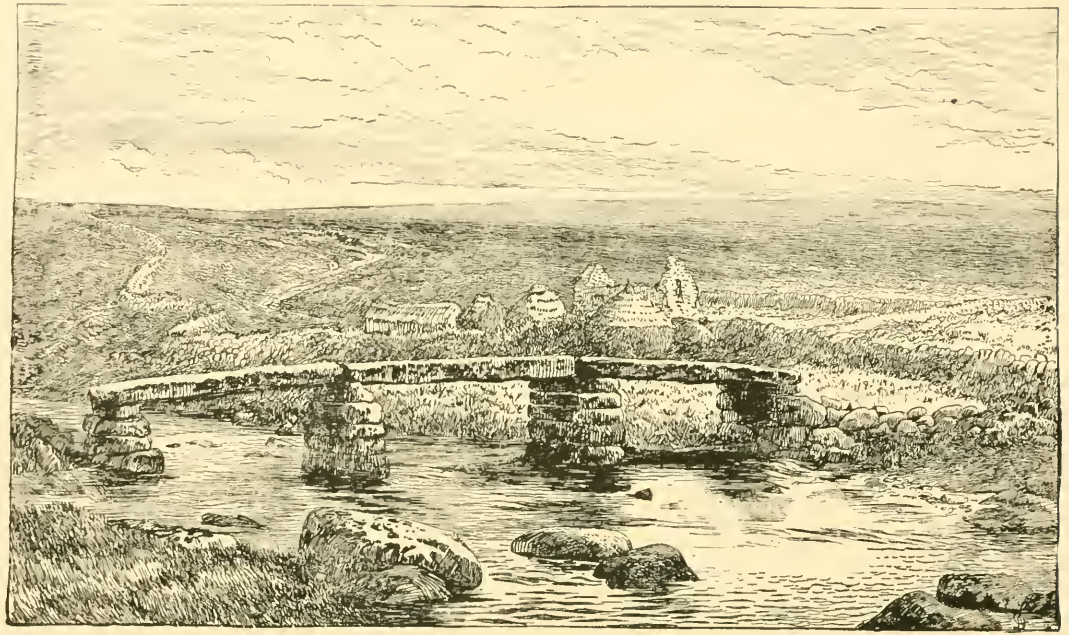
AN EXPLORATION OF DARTMOOR AND ITS ANTIQUITIES. By John Lloyd W. Page. Third Edition. Pp. xvii. and 316. With Map and Illustrations. London: Seeley and Co., 1892. Price 7s. 6d.

CONSIDERING its proximity to one of the most tourist-haunted areas in England, and the ease of access to it both by road and rail, it is surprising that Dartmoor remains one of the least-known of British moorlands. Doubtless this is largely due to the villainous reputation of its weather, originally established by the rhymes of local poets and the lamentations of French prisoners, and still maintained by the Princetown rain-gauge. This, however, cannot be a sufficient explanation while the Lake District continues to be visited by hordes of rain-soaked tourists, and the lack of popular literature has probably more to answer for. While the geology of Wales, the scenery of the Yorkshire moors, the literary associations of the Lakes, and the antiquities of the Wiltshire Downs have received constant description and discussion, the bibliography of Dartmoor has remained comparatively limited. Since Rowe's "Perambulation" in 1848, there has been no general work of merit on the area, except the admirable article by R. J. King in Murray's Guide, or the same author's "Dartmoor Forest and its Borders," until the publication of the present work; the issue of three editions in two years shows that it was needed.

The chief merit of the work is one which is its own reward, being sure to bring success; it consists not only in the author's knowledge of Dartmoor, but in his love for it. He is full of the stories and traditions of the moors, and of the legends of the demons that dwell among its granite tors, or of the pixies that skip about the boggiest of its bogs; he knows well the peculiar manners and customs that till recently survived in the seclusion of its dales; and he goes into raptures over the scenery, alike of the bare rounded hills of the central moorland, of the weird rocks along its margin, and of the picturesque valleys on its outskirts. His genuine enthusiasm for his subject in all its moods and aspects is contagious, and does much to disarm criticism.

The book is divided into five sections, one dealing with the general features of the moor, and the others treating particularly of the north, south, east, and west divisions. The especial sections are obviously based in the main on the author's journal, as he takes us along the lines of his different visits, pleasantly telling us of what he saw. The directions given, however, are quite insufficient for anyone to find the way by them, and the book thus combines the disadvantages of a topographical instead of a subject classification, without

TO FACE P. 549.



ANCIENT BRIDGE, POSTBRIDGE, DARTMOOR.

Drawn by ALFRED DAWSON.

its advantages. A guide-book account was not so much wanted, as we have those of King and Baddeley, and the work would probably have been more useful if it had followed the lines of a general treatment of the moor and its antiquities. It must, however, be admitted that the general and critical parts of Mr. Page's work are the least satisfactory. Let us, for example, take his remarks on the geology. His fond and frequent use of the word "trap" is symptomatic. We are not, therefore, so surprised when we read, on p. 13, that the granite is often porphyritic "from the presence of felspar." He quotes Mr. Ussher's hopeless theory that Dartmoor is a laccolite, without telling us that its author has abandoned that view. Another point which shows that the author has not fully grasped the significance of recent results, is that he quotes the theory that the schorlaceous and porphyritic granites and the elvans form a threefold series, without any hint that this has been rendered quite untenable by the acceptance of schorl as a decomposition product. We much wish that on p. 14 he would have given us some definite clue as to the exact locality "where no distinction could be drawn between granite and ordinary slaty material"; we always thought that the most famous feature of the contact-metamorphism around Dartmoor was the comparatively slight alteration the neighbouring rocks had undergone.

The Archæology is, as a rule, more reliable; the author's views are generally marked by common sense, except when, as on pp. 136, 137, he treats Ferguson's notions seriously. He is profoundly agnostic on all archæological problems, which is no doubt in the main justified by our ignorance of the facts that ought to be determinable. A comparison of Mr. Page's crude maps of the Merivale antiquities—great advance though they be on those of Sir Gardner Wilkinson—with such surveys as Petrie's of Stonehenge, shows how far our knowledge of these lags behind that of the Wiltshire monuments. Nevertheless, the author entertains little doubt of the pre-Roman age of the stone avenues and circles, the hut circles, the trilithons and the "clapper" bridges, the last of which are the chief of the unique features of Dartmoor. The bronze weapons and amber ornaments of the few barrows that have been well opened remove all doubt as to the occupation of the area in the Bronze age, and of the stream washing for tin that must then have been carried on. The general resemblance of the stone circles to those of Stonehenge, Karnac, and North Africa serve also to demonstrate that these are of approximately the same age as the Wiltshire series, which certainly date from the Bronze age. It does, however, seem difficult at first to believe that the clapper bridges have the same enormous antiquity; but their foundations are, as a rule, more substantial than those of the Stonehenge trilithons, and as most of these have survived, there seems no reason why the bridges should not also have done so. The rudeness of workmanship, the massiveness of the stones used, and the association of the bridges with the ancient British trackway, and groups of hut circles, all agree with the early age that is generally assigned to the bridges. The accompanying view of a typical example at Postbridge, which we owe to the courtesy of the publishers, is a good specimen of Mr. Alfred Dawson's beautiful drawings that adorn the volume.

In regard to the rock basins, we are glad to find Mr. Page admitting that these owe their existence to natural causes, dismissing the view of their artificial origin, which has been so strongly urged by Sir Gardner Wilkinson. The argument of this author—that they

must be artificial because they occur on other kinds of rock beside granite—would be all very well if he could find them on slate or limestone; but as he can elsewhere only quote them from the coarse grits of the Peak and similar rocks, which weather in exactly the same way as granite, they really support the view of their natural origin. The same explanation no doubt applies to the "ringed stones," for which the author adopts the term "tolmens"; and so many authors accept this word as synonymous with "dolmens," as does the Ordnance Survey in its map of Dartmoor, that a few words of explanation might well have been inserted.

The easy demolition of the "Druids' basins" encourages Mr. Page to run a tilt against the whole order of those holy men: he repeatedly denies all evidence for the existence of Druids anywhere on Dartmoor. If the author merely wishes to throw discredit on the fables of the burning wicker cages, which were probably invented for the edification of the ladies in Rome, and were no more reliable than the missionary stories of a later age, no one is likely to quarrel with him; but he seems to mean more than this. It is quite clear that the Bronze age people had a firm belief in immortality and some sort of religious ritual; this inevitably meant priests and collections. So long as one calls these priests "Druids" there seems to be little objection to the use of the term. To be consistent, Mr. Page must also deny the existence of Druids in Wiltshire, for the Drewsteignton Spinster's Stone is as fine a trilithon as the Fyfield Devil's Den; in that case he might as well deny the existence of Druids anywhere and invent a new name for the ancient British priests.

In spite of a constant tendency to feel dissatisfied with our author's treatment of the scientific part of his subject, we must congratulate him on having produced by far the best general sketch of Dartmoor. The book is well printed, and is illustrated by a series of etchings and photographic reproductions, several of which are the work of Mr. Alfred Dawson. The map, however, is very inadequate, and is of interest mainly as a sad example of amateur map-making. This will, we hope, be improved in the next edition, which should also be more carefully revised than the present. A very sanguine man might have been excused, in 1889, for saying that the prospects of the Postbridge Mine were promising: Mr. Page was certainly not a shareholder or he would have followed its later history more closely: nor is the Vitifer tin mine still working, as he tells us it is on p. 26. The work of the Devonshire Barrow Committee is also not mentioned. There is, in fact, little evidence in the book of any close acquaintance with the work of the younger school of archæologists, whose methods have not been as yet much adopted on Dartmoor. So far the excavations there have been conducted in a most casual manner, and not until the theodolite, chain, and spade have been applied with modern precision and thoroughness can we hope to make the stone avenues, rock circles, clapper bridges, and trilithons of Dartmoor yield their contributions to the prehistoric history of our land.

THE ANATOMY, PHYSIOLOGY, MORPHOLOGY, AND DEVELOPMENT OF THE BLOW-FLY (*Calliphora erythro-cephala*): A Study in the Comparative Anatomy and Morphology of Insects. By B. Thompson Lowne, F.R.C.S., F.L.S. Vol. i., pp. x. and 350. With 21 plates and 47 figures in the text. London: R. H. Porter (for the author), 1890–2. Price £1 10s.

IN 1870 Mr. Lowne published a small book on the Anatomy and Physiology of the Blow-fly. The present volume is by no means a

second edition, but an entirely new work, as is necessary from the great advances in our knowledge of the structure and development of insects during the last twenty years. This first volume deals with the anatomy of the larva, the development of the embryo and of the imago, the external anatomy of the imago, and its histology. There are also chapters on the general life-history of the blow-fly, the general anatomy and morphology of insects, and the characters of the Diptera. The volume has appeared in three parts, the first issued in October, 1890, the third in April, 1892. In the second volume, which has yet to appear, we are promised "a detailed description of the various internal organs, their development, and physiology."

The study of insect anatomy has been neglected in this country, and the later literature of the subject has mostly come from the laboratories of Germany and Russia. We have, however, Miall and Denny's excellent monograph of the Cockroach, one of the most generalised of insects. Mr. Lowne's work is much larger than this, his type being a highly specialised insect.

It is impossible not to admire the thoroughness with which Mr. Lowne has carried out his task, and the laboriousness of his investigations deserves every acknowledgment. The plates representing the parts of the exoskeleton of the thorax are striking examples of this, no less than fifty-three sclerites being figured. In his description of the structure of the feet, he confirms the oldest suggestion as to the method of walking on plane surfaces: that the hairs on the pads secrete drops of viscid fluid which temporarily attach them to the surface, instead of the later theories that the hairs act as hooks or the pads as suckers. The mechanism at the base of the wing consists of several sclerites, and their action in producing the motion of the wing-tip is described. The curve described by the tip is of complicated form, and closely agrees with that arrived at for dragon-flies by Lendenfeld by means of instantaneous photographs.

In his interpretation of the proboscis, Mr. Lowne differs from most insect-anatomists. This organ in the fly has generally been regarded as almost entirely formed by the second pair of maxillæ (labium or lower lip) as it undoubtedly is in the Hymenoptera. Mr. Lowne, however, considers that he has evidence, from its structure and development in the blow-fly, that it really represents the first pair of maxillæ, and that we must consequently compare it with the sucker of a moth, not with that of a bee. He would derive the rostrum of the Hemiptera from the same source. The labium of the fly he supposes to be quite rudimentary. The piercing stylets which are found in many Diptera, and have always been regarded as the mandibles and the first pair of maxillæ, he considers, as well as the sucker, to represent parts of the first pair of maxillæ only. The similar stylets in the Hemiptera would probably be explained in the same way. These views will certainly not be accepted without further evidence, but they deserve careful consideration, and will stimulate research.

An embryological point on which Mr. Lowne is at variance with most other observers is the nature of an invagination in the dorsal region of the blastoderm (dorsal organ of Kowalevsky). This structure, regarded by Graber as the proctodæum, is thought by Mr. Lowne to be an archenteron, and, therefore, to show a typical gastrulation. The Malpighian tubes are developed as sacculi from this cavity; hence if it is an archenteron, they are coelomic in origin, and show more affinity with the excretory organs of other invertebrates

than has hitherto been supposed to exist. Here, again, there must be considerable sifting of evidence before a definite conclusion can be reached. Most naturalists will, however, be startled by Mr. Lowne's comparison of this invagination with that forming the neural canal in vertebrates, a comparison induced by the speculations of Gaskell and Patten that this canal represents an old alimentary tract. The near relationship between arthropods and vertebrates, required by this view, will need far more evidence than has yet been produced in its support; and most students will still prefer to regard a fly and a man as extreme specialisations in very different directions.

In the Diptera insect-metamorphosis reaches a high degree of specialisation. The organs of the larva are not transformed directly into those of the nymph and imago, but the larval tissues are dissolved within the pupa, while special invaginations (the imaginal discs) which had been present in the larva, form the pro-nymph. From this the nymph is developed, its external organs being evaginated, and it is in turn transformed into the imago. It follows from this that "not one single organ is common to the larva and the imago;" so "complete" is the metamorphosis, that some observers have regarded it as a case of alternation of generations, considering the nymph as a new embryo. To this somewhat wild suggestion, Mr. Lowne lends no support, but rightly explains it as an extreme case of ecdysis. He refers to the development of the Nemerteans in which the primitive ectoderm is ultimately cast, while the adult skin is formed by invagination from it, as an analogous case. It will be noted that Mr. Lowne speaks of the precursor of the imago as a "nymph," while he uses the term "pupa" for the dried up larva-skin within which the "nymph" is developed. This sense of the words is different from that adopted by entomologists generally.

The account given of the development of the legs in the nymph deserves special notice. At first there is a basal portion with two processes; these three parts are compared to the coxopodite, endopodite, and exopodite of a crustacean limb. The endopodite becomes five-jointed, and ultimately forms the tarsus. The exopodite lengthens and thickens, finally splitting longitudinally into a proximal (femur), and distal portion (tibia). The tarsus is, after the split, of course, attached to the tibia. This interpretation is ingenious, but it seems doubtful if the development can have the morphological meaning attached to it by Mr. Lowne. It is remarkable, however, that he and Jaworowski should almost simultaneously have compared respectively the limbs of flies and spiders with those of crustaceans.

Many of Mr. Lowne's theories will be combated; some will probably be refuted. He is careful, however, to state clearly the various opinions already put forward on disputed points, and to distinguish between the facts he describes and the hypotheses he builds upon them. A very complete bibliography adds greatly to the value of the work, and future investigators owe a debt of gratitude to our author for his full descriptions of his methods of study which will save them many fruitless experiments. G. H. C.

BIRDS: The Elements of Ornithology. By St. George Mivart, F.R.S. Pp. 306, figs. 170. London: R. H. Porter, and Dulau & Co., 1892. Price 6s.

"THIS small volume," we are told, "is put forward in the hope of supplying a want which its author has himself felt keenly. It is intended to supply, in a small compass, a general view of the Class of

Birds; together with such a knowledge of their structure, activities, geological and geographical relations, and classification, as may fit the student to enter upon a serious study of Ornithology." Dr. Mivart has set himself to accomplish this task in 306 small 8vo pages, which are illustrated by 170 woodcuts. The first half of the book contains a brief account, copiously illustrated—no less than 140 of the woodcuts are to be found in this "Introduction"—of the principal kinds of birds, and these illustrations, although rough, are effective enough and accurate. There are, perhaps, rather too many of the passerine birds, which often do not present salient differences that can be duly emphasised without the assistance of colour.

The anatomical part of the book is, in our opinion, not full enough for the beginner to whom Dr. Mivart addresses himself; it would have been better, we think, to give a more elementary account of a bird's structure at greater length, and with abundant illustration; or, on the other hand, the author might have contented himself with a description of those structures which vary most among birds, and which are therefore made use of for classificatory purposes. A person with a knowledge of comparative anatomy does not want such an abridged account as that which Dr. Mivart gives, for he has the excellent and elaborate treatises of Professor Fürbringer and Dr. Gadow; and for a beginner, with presumably no knowledge of anatomy, the chapter on structure would be decidedly too stiff. A very useful feature to ornithologists in the book before us is the list of structural characters that is given with each group in the classification which concludes the volume.

Les Plantes Alexitères de l'Amerique. By H. Bocquillon-Limousin. 8vo. Pp. 120. Paris, 1892.

Serpents, with their deadly bites, form one of the greatest scourges of the New World as well as Asia. In some parts the mortality is very great, as, for example, in Martinique, Mexico, Ecuador, Brazil, and the Argentine Republic. Happily for humanity, the flora of the country affords an antidote, and the object of this little book is to give an account of those tropical and sub-tropical American plants which experience has shown to be thus useful. The author evidently believes in a somewhat subtle relation between the flora and fauna, for, in concluding, he states his belief in the principle that the bite of a serpent must be counteracted by the action of a plant of the same country. As, however, he likewise affirms that the poison is nullified only by the adverse physiological action of the plant itself, not by that of the definite chemical principle contained, and also suggests elsewhere that the action of dried plants is very different from that of freshly-gathered plants, it is obvious why the serpent and the antidote must flourish in the same neighbourhood; otherwise, his statement seems based only upon the fact that the Condor of Ecuador always has recourse, when bitten, to the *Gonolobus condurango*, and similarly the Guaco heron of the Magdalena Valley to the *Mikania guaco*; while, according to authors, the latter plant is powerless against the *Trigonocephalus* of Martinique.

Under the botanical name of the plant is given the synonymy including native names. Then follow the habitat, a botanical description, and the therapeutic properties. In the more important cases, the account is a full one, including the minute anatomy of the stem, with sometimes illustrations of transverse

sections, a chemical analysis of the efficacious portion, its physiological action, the way to use it, and the dose. In several cases glucosides and alkaloids have been for the first time isolated, or thoroughly studied, *e.g.*, mikanine from *Mikania guaco*, condurangine from *Gonolobus condurango*, dorsténine, and others.

The physiological action of plants, or the chemical compounds produced in the course of their metabolism is a subject always deserving of study, both from a medical and scientific point of view, and it may be of use to have collected in one volume such a class as those treated of in "Les Plantes Alexitères de l'Amerique."

MINERALOGY. By F. H. Hatch, Ph.D., F.G.S. Pp. 124, figs. 115. London: Whittaker & Co., 1892. Price 3s. 6d.

THIS little volume, which belongs to Whittaker's Library of Popular Science, should prove useful as an elementary introduction to the scientific study of minerals, and in our opinion it differs from other English or American text-books of Mineralogy in precisely those features which make a book suitable for beginners.

The author refrains from describing many species or from entering into much detail, and the minerals selected for description are grouped according to those properties from which they derive their interest or importance. A brief introduction concerning the Characters of Minerals is succeeded by four short chapters on (1) Rock-forming Minerals, (2) Ores and Veinstones, (3) Salts and Useful Minerals, (4) Gems. This arrangement is evidently more attractive, and, we venture to think, more useful to a beginner than the customary method of describing one species after another in systematic order.

The book is of a quite elementary and popular character, and is written in the simple and lucid style which distinguished Dr. Hatch's "Introduction to Petrology." We can find nothing in this volume which should not be easily intelligible, except the explanation of cleavage on p. 36, of which the meaning is by no means apparent. Some of the figures are reproductions from photographs of specimens.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

British Association.

The recent meeting of the British Association held in Edinburgh was well-attended, the total number of tickets issued being 2,070. There were many distinguished foreign guests, and the number of associates would doubtless have been greater had not so many of the residents of Edinburgh been spending their vacation elsewhere at the time. In Natural Science, Geology was an especially prominent feature, Sir Archibald Geikie's Presidential Address dealing with the Doctrine of Uniformity and the Antiquity of the Earth in a popular manner, while the Sectional Addresses of Professors Lapworth and James Geikie were devoted to what we elsewhere (p. 481) term "The New Geology." Sir Archibald Geikie contended, from geological evidence, that the Earth must be much older than supposed by Lord Kelvin and other physicists.

Apart from the brilliant new ideas in Professor Lapworth's Address, the Geological Section advanced little beyond minor matters of detail. Mr. J. Lomas' demonstration of the scattering of fragments of rock from Ailsa Craig (in the middle of the Firth of Forth) over the Isle of Man, North Wales, and parts of Cheshire, was of great interest, the rock being so characteristic as to be always readily recognised. Messrs. Peach and Horne exhibited a supposed deep-sea ooze with radiolaria from the Silurian of the south of Scotland, and showed how the chert in contact with granite became completely recrystallised, consisting of large grains of quartz with inclusion of some garnets and mica. Mr. Clement Reid described seeds of some truly Arctic plants from the lowest deposit of certain silted-up lochs near Edinburgh; and Mr. E. T. Newton contributed a preliminary notice of the Elgin Dicynodonts and other reptiles, which have at last been rescued by the Geological Survey from the obscurity in which they have lain for six years at the Edinburgh Museum. The Committees of the Geological Section are not so active as they might be, and some of the Reports this year were far from satisfactory.

The success of the Biological Section was very marked in each of the three subdivisions. It is estimated that the aggregate number of zoological, botanical, and physiological papers was higher than that of all the other papers presented to the Association. Professor Rutherford's Address was an elaborate exposition and criticism of current views on the physiology of the colour-sense. A discussion on the possibility of uniting all the organisations devoted to Sea Fisheries in the British Isles may lead to important legislation. The Committees for the exploration of the West Indies and the Sandwich Isles reported good progress; and Mr. H. O. Forbes exhibited recently-discovered bird-remains from New Zealand. Dr. McCook gave an address on spiders, in which he described them as solitary animals; and in General Biology Dr. Beard discussed the relation of larval to adult animals. In the Botanical Department, foreign visitors came conspicuously to the front. Professor Goebel, of Munich, contributed a very important paper "on the simplest form of Mosses"—viz., *Buxbaumia*, long known to Bryologists as an extreme form, by reason of its remarkable sporogonium, and inconspicuous vegetative characters. Professor Goebel now announces that its antheridia are borne on the protonema. There is room for a difference of opinion as to the precise significance of this astonishing discovery, but the botanists present appeared to agree, on the whole, in

regarding it as a "degraded type." Side by side with this paper was read one by Professor Bower, in which, with admirable lucidity, he brought forward not only "modern instances" in support of his well-known views on the evolution of the Vascular Cryptogams, but drew support from a minute study, recently made, of fossil remains in the Botanical Department of the British Museum. Dr. Scott and Mr. Brebner made a solid contribution to plant anatomy, viz., an examination of cases of secondary thickening of the stems of certain Monocotyledons. This paper, indeed, like Mr. George Murray's "on a comparison of the marine floras of the warm Atlantic and Indian Ocean," seemed a little too solid to be easily digestible by a British Association audience. A paper was read by Miss Nina Layard on the arrangement of the buds in *Lemna*. Nothing very revolutionary came of it, and since the Secretaries of the Section did not emulate the zeal of their geographical colleagues when a lady "explorer" appears on the scene, no profound sensation was caused by the modest presentation of an unpretentious paper. On the whole, the botanical subsection showed no approach, even for a moment, to the giddiness associated in the public mind with these meetings.

Besides Professor James Geikie's Address on coast-lines, several other geological matters were submitted to the Geographical Section. Mr. John Thomson vividly described the eroding action of rain and streams in Formosa, and Mr. J. Y. Buchanan opened a discussion on the winding of rivers. There was a long discussion with the chemists on Oceanography; and the Prince of Monaco advocated the establishment of meteorological stations on the Azores and other Atlantic islands.

In his address to the Anthropological Section, Professor Macalister critically reviewed recent advances, and lamented the want of precision in anthropological works. The Committee for the Investigation of the Canadian Indians presented a good report; and there were important discussions on Criminal Anthropology, and on a proposed Ethnographic Survey of the British Isles. Dr. Garson's report on measurements of members of the Association at Cardiff was ungratefully received, being described as meaningless; and the exhibitor of some supposed pre-palæolithic flint implements from the North Downs of Kent met with little encouragement.

The General Committee of the Association voted the total sum of £1,000 to the various committees engaged in research. The grants to the Geological Section were small in amount, applied to the subjects of Erratic Blocks, Underground Waters, Geological Photographs, and some Palæontological Investigations. In Biology, the sum of £100 is granted for the maintenance of a table at the Naples Zoological Station, and £30 for another at Plymouth; £20 is devoted to the expenses of Mr. Sherborn's Index of the Genera and Species of Animals, and a similar amount for the study of Asphyxia; £100 is granted for the exploration of the Sandwich Islands, half that amount for further enquiries into the Zoology and Botany of the West India Islands, and £30 for dredging in the Irish Sea. In Geography, the Committee for the exploration of the Karakorum Mountains receives £50, and that for the investigation of the Climatology and Hydrography of Tropical Africa a similar sum; £10 is granted for the study of Scottish Place Names. In Anthropology the Committees on Ancient Remains in Abyssinia, the Tribes of N.W. Canada, and the Natives of India receive respectively £25, £100, and £10. The Corresponding Societies' Committee has £30, and the Anthropometric Laboratory £5.

THE organisation of the new University of Chicago is progressing well, and the first session will begin on October 1 next. The Director of the Biological Institute is Dr. Charles O. Whitman, who also holds the Professorship of Animal Morphology. Most of the Director's colleagues lately in the Clark University at Worcester (Mass.), accompany him in the new institution. The Professorship of Biology still remains vacant, but the Chairs of Anatomy and Comparative Neurology are held respectively by Dr. Franklin P. Mall and Dr. H. H. Donaldson. Dr. Georg Baur occupies the position of Assistant-Professor of Comparative Osteology and Palæontology.

In the August number of the *American Naturalist* there is an interesting account of the success of the Biological Department of the Leland Stanford Junior University, California. The number of students has been much in excess of anticipation, and the original equipment of the various laboratories proved far short of requirements. The new Marine Biological Station at Pacific Grove, Monterey Bay, is named the Hopkins Seaside Laboratory, the general furnishing having been provided by the liberality of Mr. Timothy Hopkins. The building was presented to the University by the Pacific Improvement Company and the inhabitants of Pacific Grove, and affords accommodation for about fifty students. Teachers and ordinary students pay a small fee, but original investigators are admitted to the laboratory free of charge.

The first part of the *Kansas University Quarterly*, dated July, 1892, has lately been received. The journal will be devoted to the results of original scientific researches undertaken by members of this Western University. The part to hand contains an illustrated account of the Cretaceous Pterodactyles and Mosasaurs of Kansas by Professor S. W. Williston, and some technical descriptions of insects.

Dr. J. S. Kingsley has been appointed Professor of Biology in Luft's College, College Hill, Mass., U.S.A.

Dr. Carl Berg has been appointed Director of the Museum of Buenos Ayres, in succession to the late Dr. H. Burmeister.

The latest publication of the Australian Museum, Sydney, is the second part of Mr. John Brazier's "Catalogue of the Marine Shells of Australia and Tasmania," comprising the Pteropoda. It is remarked that no mollusca of this class have yet been discovered off Tasmania and South or West Australia.

The American Museum of Natural History, New York, has lately acquired a series of mammalian teeth from the Laramie Formation. We are pleased to learn from the *American Naturalist* for August that the monopoly in collecting Laramie fossils attempted to be held by certain officers of the U.S. Geological Survey, is now to be destroyed. An expedition, under the direction of Dr. J. L. Wortman, will shortly be despatched by the American Museum to the Laramie region, for the collection of the Horned Dinosauria and associated fossils.

The Annual Report of the Trustees of the South African Museum, Cape Town, for 1891, has just reached England. In addition to their ordinary routine duties, the Curator (Mr. Roland Trimen, F.R.S.) and Assistant-Curator (Mr. L. Péringuey) are occupied with important researches in South African Entomology. The specimens acquired during the year are numerous and varied; but much difficulty is found in preserving the skins of the larger animals on account of insect pests, and the Museum has had the misfortune to lose some valuable diamonds and nuggets by theft. The extension of the premises, long contemplated, has hitherto been delayed by the unfavourable condition of the revenue, and the prospect that the new Museum might form part of the projected University Buildings. A further effort, however, is being made to erect an independent institution at an early date. At the end of the present year, the Museum will receive another important acquisition, in the form of a plaster cast of the skeleton of the great extinct reptile, *Pariasaurus baini*, from the South African Karoo, discovered by Professor H. G. Seeley, and now in the British Museum. This unique specimen was despatched last month by the Trustees of the British Museum to the Kimberley Exhibition, where it will be shown prior to its removal to Cape Town.

We are pleased to learn that the Director of the Natural History Departments of the British Museum, Professor Flower, has received the honour of knighthood, attaining the rank of K.C.B. Professor Huxley has also been honoured by election to the Privy Council.

The Museums Association meets next year in London, under the Presidency of Sir William Flower, when the possibility of establishing some closer connection between the British Museum and the Provincial Museums will doubtless be discussed. At its recent meeting in Manchester, the Association conferred with a deputation from the Libraries Association, in reference to the proposed emendation of the Acts under which Public Libraries and Museums are at present governed. An increase in the appropriation of public funds for the support of these institutions is desired.

In 1878, the Council of the Hastings and St. Leonards Philosophical Society published a small pamphlet on "The Natural History of Hastings and St. Leonards and the Vicinity," containing a complete list of the known Fauna and Flora. In 1883 and 1888, supplementary lists were issued, chiefly through the instrumentality of the Rev. E. N. Bloomfield, of Guestling. In 1889, the naturalists of the district held a meeting with some of the prominent citizens to discuss the possibility of founding a museum to illustrate both the Natural History and Antiquities of the town and neighbourhood. Subsequently the scheme received much support, becoming gradually realised under the auspices of the "Hastings Museum Association," and on August 16 last the nucleus of a Museum was formally opened in the upper rooms of the Brassey Institute. The Mayor of Hastings presided at the ceremony, and speeches were made by Mr. W. V. Crake (Hon. Sec. of the Association). Mr. Wilson Noble, M.P., Mr. Felix Joseph, and Mr. Smith Woodward (of the British Museum). According to the prospectus, the Natural History and Antiquarian Departments of the Museum are to be devoted exclusively to local specimens, arranged, as far as possible, for educational purposes; and a good beginning has certainly been made. The Committee, however, are already embarrassed with the inevitable pagan idols, foreign curios, and furniture blessed by contact with some famous man of the past; and unless they can devise some means of removing such "matter in the wrong place," their admirable intentions will soon be frustrated. The Art Exhibits also encroach much upon space that might be more profitably devoted to objects having local associations. A small Zoological Laboratory is to be added to the Museum, while courses of lectures and demonstrations are contemplated.

The Annual Report of the Conservator of the Museum of the Royal College of Surgeons of England, lately presented to the Museum Committee, records the completion of the two additional rooms that have so long been required for the expansion of the collection. Much progress has already been made in the re-arrangement necessitated by the addition. In the Department of Human and Comparative Anatomy the most important additions during the year are thirteen skeletons and twelve skulls of ancient Egyptians, probably dating back to 4000 B.C., presented by Dr. Flinders Petrie; and a large series of various skeletons and skulls of Vertebrata from the collection of the late Sir Victor Brooke, Bart., presented by his son.

An appendix to the volume of the Proceedings of the Royal Society of London just completed (vol. l.) is devoted to an interesting summary of the Society's second and third Charters and a historical notice of the Statutes. There is also a catalogue of the portraits, busts, and medals in the possession of the Society.

THE Imperial Academy of Sciences of Vienna has granted a subvention to Professor L. von Graaff, for the purpose of defraying the expenses of an expedition next Spring to the tropics, to collect material for the completion of the second volume of his Monograph of the Turbellaria.

THE Royal Scottish Geographical Society proceeds next month to the election of a new Secretary. Geographers will regret to learn of the retirement of Mr. Arthur Silva White on account of ill-health.

THE twenty-first anniversary of the Chester Society of Natural Science will be celebrated early this month. His Grace the Duke of Westminster is expected to preside, and several of the Society's honorary members will attend.

THE Annual Report of the Yorkshire Philosophical Society for 1891 records progress in the work of the Museum, but contributes little to original research. Except a brief record of two borings for water, the report contains only a statistical account of meteorological observations. We understand that the late Mr. William Reed bequeathed the sum of £600 to the Society, the interest to be devoted to acquisitions for the Museum.

THE Manchester Geological Society has just completed vol. xxi. of its *Transactions*. In the last part Mr. G. C. Greenwell discusses the possibility of discovering coal to the south of the Mendip Hills, and Mr. C. Roeder publishes some observations on marine shells in the Glacial Drift near Manchester.

MR. EDWARD F. PITTMAN, Chief Mining Surveyor, has been appointed Government Geologist of New South Wales in succession to the late Mr. C. S. Wilkinson. The annual report of the Geological Survey for 1891, lately received, is devoted almost exclusively to economic questions.

GEOLOGISTS will regret to learn of the death of Dr. Otomar Novàk, Professor of Geology in the Bohemian University of Prague, which took place on July 29, at the early age of 42 years. The Professor was occupied with the continuation of Barrande's great work on the Silurian fossils of Bohemia, specially investigating the corals.

OBSERVATIONS AND CORRESPONDENCE.

Dr. Weismann's Theory of Heredity.

I have received the following interesting observations from Dr. E. Sickenberger, Professor of Chemistry, Botany, and Materia Medica in the School of Medicine, Cairo, which readers interested in the subject may be glad to see:—"I have read with great interest your publication upon Dr. Weismann's theory, and I am very glad to see that your conclusions are entirely in accordance with my own observations. I believe, if Weismann had had more special knowledge of botanical matters, he would not have undertaken to apply his theory to plants. You have completely refuted by your explanations his assertion that a continuity of germ-plasm exists in a certain series of somatic cells only. I cannot admit any essential difference of characters between species and varieties, such being only due to a greater or less degree of qualities acquired by the influence of external conditions during a longer or shorter period. Hybridisation comes in, in a secondary way, mixing those acquired qualities with another series. Hereditary persistence is, I believe, nothing else than the maintenance of those acquired qualities under the power of inertia (Gesetz der Trägheit). The more any acquired qualites bave changed the anatomical and chemical constitution of a plant, so much the more will those qualities be persistent. If Weismann says 'acquired characters cannot be transmitted at all,' every day in Egypt contradicts this opinion, and the fact that 'species of plants in Egypt have remained unchanged for thousands of years,' only proves that the climatal conditions of Egypt have remained all the time unchanged. On the other hand, seeds of *Gignut* [?], a variety of *Cannabis sativa* from Europe, produce the true *Cannabis sativa* yarn by the third generation. Black mustard, *Brassica* (*Sinapis*) *nigra* is transformed in the second generation into the endemic *B. bracteolata*, Fisch. et Mey. [Plante particulière à l'Egypte (Asch. et Schweinf.)], and the thick-rooted celery assumes in the first year the much foliated form with a thin root-stock, like the summer spontaneous form of Egypt. I have myself had the opportunity of substantiating these facts several times. On the other hand, it must be proved that external influences act in some other way upon the germ-plasm than upon the somato-plasm—all action in nature being based upon the universal physical and chemical forces—so that really the constitution of the pretended germ-plasm is different from that of the somato-plasm."

George Henslow.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

J. Wilson (*Stamford*).—The Conchological Society's List of British Land and Freshwater Mollusca has already appeared: see *Journ. Conchology* for April, 1892.

W. H. Corder (*Lancaster*).—The Darwin collection of Cirripedia is now in the British Museum, but we believe that all the duplicates are to be seen in the Liverpool Museum. The "*Coronulites diadema*" to which you refer is also in the British Museum. The Parkinson sale took place in 1826, and the specimens were widely distributed; this specimen, however, finally reached the national collection through a Bury-St.-Edmunds collector.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 8. Vol. I. OCTOBER, 1892.

NOTES AND COMMENTS.

Field Observations in Natural History.

SOME time ago (*suprà*, p. 84) we had occasion to lament the meagre character and restricted import of most of the original contributions to Natural History published by the British Field Clubs. The issue of several new reports and journals which we record elsewhere still further justifies this lamentation over wasted opportunities; and the fact that even in so widely-studied a subject as botany no member of the societies affiliated with the Midland Union has done any original work during the last three years worthy of the award of the Union's Darwin Medal, emphasises the need for a further systematic organisation of research. On the former occasion, we pointed out the service that might be done by any widely-gifted naturalist who would produce a handbook stating modern problems in such terms that any ordinary student of nature, dwelling at a distance from towns and libraries, could turn his observations to some profitable account. Quite lately we have had the great gratification of receiving the precise *vade mecum*, which ought to be in the hands of all observers who take an interest in the facts and phenomena of organic nature.

This little work [1] is a new volume of Mr. Murray's University Extension Manuals, and has been prepared by one of the most lucid of modern teachers, Mr. J. Arthur Thomson, of the School of Medicine, Edinburgh. It is a book intended for suggestive guidance of the student, "rather than to satisfy that thirst for knowledge which leads many to intellectual insobriety." The matter is divided into

[1] "The Study of Animal Life." By J. Arthur Thomson, M.A., F.R.S.E. 8vo., pp. 375, with woodcuts. London: John Murray, 1892. Price 5s.

four sections, dealing respectively with the everyday life of animals, their internal activities, their forms and structure, and the general theory of animal life. The style is charming, and the language remarkably simple. The appendix urges the reader to turn his attention to Nature itself, gives a carefully selected list of the special works to use, shows him how to commence and proceed. "Begin with domesticated animals and their history," says Mr. Thomson, "enjoy your seaside holiday, . . . form an aquarium, . . . begin a naturalist's year-book, . . . observe the animals you see on your country walks, . . . work out some subject which attracts you." Let the observer thoroughly understand the principles and spirit of investigation expounded in the four sections of the work; and then he will be prepared to follow these injunctions both with pleasure and profit.

Whether employed as the basis of University Extension Lectures or independently read, Mr. Thomson's "Study of Animal Life" ought to inaugurate a new era in Field Natural History in this country; and we hope it will not be long before we are able to chronicle a sensible improvement in the too frequently aimless and pointless "observations" of the local student of the phenomena of living nature.

The Dispersal of Plants and Animals.

While simple, comprehensive Handbooks and University Extension Lectures may tend to remove the reproach to which we have just alluded, concrete examples of the valuable contributions to the Philosophy of Biology within the power of ordinary Field Observers, are perhaps more to the point. It is thus with great satisfaction that we are able to refer to some recent publications by two recognised leaders in such research. This year's Presidential Address by the Rev. Leonard Blomefield to the Bath Natural History and Antiquarian Field Club, lately issued, is worthy the close attention of all botanists who would do something more philosophical than merely record the geographical distribution of the various plants in their district. A paper contributed by Mr. Clement Reid to the new number of the *Transactions of the Norfolk Naturalists' Society*, is still more suggestive both to botanists and zoologists. Mr. Reid, indeed, may almost be said to inaugurate a new line of research, and this will probably lead to interesting and striking results. He treats of the Natural History of isolated ponds, confining himself to those of artificial origin, usually of quite recent date; and it is astonishing to find how rapidly these small pools of water become stocked with a varied fauna and flora. Certain aquatic animals and plants seem to be transported with the greatest ease; and the problem is to determine, not only the mode and means of dispersal, but also the effect of changed environment on the organisms concerned. So far as Mr. Reid's observations have extended, it appears that the main

transporting agency is that afforded by the feet and feathers of wading and bathing birds; for the commonest plants in isolated ponds are the brittle species with finely-cut leaves, such as collapse and cling when lifted out of the water. The associated molluscs are usually the species that live among, or attach their eggs to, these plants.

With regard to the changes produced by environment, we may also allude to some remarks by Mr. Robert Holland in the August number of *Nature Notes* (vol. iii., pp. 147–152) which have reference to similar observations previously published by Mr. Collingwood Hope. It appears, as might be expected, that fishes placed in stagnant ponds soon become stunted, rarely growing to their normal size, but it is interesting to have proved that even after several generations, this dwarfing in the case of tench is not permanent, the small fishes attaining their usual dimensions as soon as they are removed to a shallow pond with a plentiful supply of fresh water.

Economic Science.

Such observations, of course, have at present merely a philosophical interest, and some of the lukewarmness in this line of research may be due to the preference of many workers for subjects that have an immediately economic bearing. In this connection it is of interest to refer to the exhortation lately delivered by Professor Herbert McLeod to one of the most economically-minded bodies in the world of science—that of the chemists. In his recent Presidential Address to the Chemical Section of the British Association, the Professor refers to Faraday's well-known production of benzene from oil gas in 1825, when this investigator had not the slightest idea that it could ever have any practical application; and he then quotes from Professor A. W. von Hofmann's lecture on Mauve and Magenta in 1862, when the purely scientific discovery had become the basis of a great industry. He quotes von Hofmann's peroration, and it will bear reprinting again:—

"Need I say any more? The moral of Mauve and Magenta is transparent enough; I read it in your eyes. We understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compounds, you will not cool his noble ardour by asking him that most terrible of all questions, 'What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?' Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat, perform his task. Let him indulge in the pursuit of truth—of truth pure and simple—of truth not for the sake of Mauve, not for the sake of Magenta, let him pursue truth for the sake of truth."

"This," remarks Professor McLeod, "seems to be the true spirit of the scientific investigator"; and when as much is known of the fundamental principles of life as is already known about the Atomic and

Molecular Constitution of bodies, Chemical Affinity, and the Conservation of Energy, there is little doubt that this knowledge will have as important a bearing upon everyday affairs as the great industries that have been founded on our knowledge of non-living matter.

Colour Blindness.

Biologists are already called upon to give advice in many matters of ordinary life, and among the latest subjects submitted for their consideration is that of colour vision. A Committee of the Royal Society of London, composed both of Biologists and Physicists, has presented a report on the subject to Parliament, and this is now issued as a Blue-book. It appears that nearly four per cent. of the males in civilised countries exhibit some defect in colour-perception ; and most of these persons are practically blind to one of the primary colours of the spectrum. Blindness to red appears to be most frequent, while blindness to green is not uncommon, and there is rarely failure to perceive blue. Some people are blind to two colours, and instances are known in which no difference could be recognised between the three primary colours. The industrial importance of such colour-blindness, in relation to marine and railway signalling, is too apparent to require comment ; and the Royal Society's Committee will have done good service if their Report leads to satisfactory legislation.

The Skeleton of the Voice-Organs.

Among the most interesting of recent researches in the development of the animal organism, are those devoted to the elucidation of remnants of embryonic structures in the adult. The preliminary results of these researches sometimes prove to be rash speculations, and we are occasionally inclined to question the value of such journals as the *Zoologischer Anzeiger* and the *Anatomischer Anzeiger*, in which they chiefly appear. In the last-named journal of August 27, however, there is a noteworthy publication by Dr. Harris G. Wilder on the cartilages of the larynx in the Amphibia, in which some striking, novel views are advanced for examination. It is well-known that at least four of the pairs of bars that support the gills of the young tadpole become fused together to form the hyoid plate supporting the tongue of the adult frog. The question now arises as to what happens to the fifth (or hinder) pair of gill-arches when the time arrives for their disappearance or modification. Dr. Wilder believes his researches will show that these elements are transferred to the voice-apparatus, and become the arytænoid cartilages of the larynx. Du Bois has already determined that the thyroid cartilage of the larynx is derived from part of the second and third gill-arches ; and Dr. Wilder's new view is further rendered probable by the fact that no animal possesses both the fifth gill-arch and the arytænoid cartilages, exhibiting only one or the other supplied by one and the same nerve.

Hairs and Feathers.

Another remarkable speculation that also necessitates further independent research, appears in the current number of the *Morphologisches Jahrbuch* (vol. xviii., pp. 717–804, pls. xxiv.–xxvi.). As is well-known, it is the common belief that the hairs of mammals, the feathers of birds, and the scales of reptiles are all epidermal structures of a fundamentally identical character; but after an elaborate study of the growth and development of these several protective coverings, Dr. F. Maurer, of Heidelberg, now arrives at the conclusion that hairs are, in every respect, distinct from feathers and reptilian scales. He considers that they are homologous with the sensory points in the skin of the Amphibia, or, at least, that they are outgrowths from these points as bases. Referring to the fact that the characters of the integument are of importance in classifying the great groups of Vertebrata, Dr. Maurer thus concludes that his researches confirm the supposition that the Mammalia are derived directly from the Amphibia, and have not had any Reptilian ancestors.

The Dawn of Life.

Among recent discoveries in the rise and progress of the world of life as a whole, naturalists will turn with perhaps most interest to Dr. Charles Barrois' announcement of the discovery of Radiolaria in the Archæan rocks of Brittany (*Comptes Rendus*, vol. cxv., pp. 326–328, 1892). The so-called *Eozoon* proving to be most unsatisfactory, another claimant to represent Pre-cambrian Life will be welcomed by those who still hope to decipher the conditions of that remote period; and the evidence has been discovered under circumstances that have long been thought to betoken organic agency. The fossils occur in a fine siliceous matrix (phtanite) associated with fragments of graphite, in the neighbourhood of Lamballe, Côtes-du-Nord. They have been submitted by Dr. Barrois to M. Cayeux, who regards them all as simple Radiolaria, of the family Monosphæridæ; and a detailed description is promised for an early date.

The Chemist as a Stratigraphical Geologist.

While referring to progress in the study of fossils in France, we must not omit to mention a remarkable—some might say fantastic—conclusion recently arrived at by a chemist, M. Adolphe Carnot, in the study of fossil bones by chemical analysis (*Comptes Rendus*, vol. cxv., pp. 337–339). This author believes, in fact, that the relative age of fossil bones can be ascertained by estimating the amount of fluoride of calcium entering into their percentage composition. While admitting that these fossils vary much in composition with the nature of the matrix, M. Carnot concludes that "there exists a sufficiently constant relation between the amounts of fluoride and phosphate of

lime contained in fossil bones of the primary and secondary epochs. There is much less fluoride in those of the tertiary epoch, of the quarternary, and especially of modern times." He further applies his method to determining whether certain human bones found with those of extinct animals are really contemporaneous or are of later origin, and have been introduced into the deposits where they now occur. A human tibia discovered by M. Rivière in association with bones of animals was specially submitted to M. Carnot for examination; and analysis proved that this tibia contained only the same percentage of fluoride as a recent human bone, while the associated animal remains yielded seven to nine times as much fluoride as their recent analogues. Hence, concludes M. Carnot, the human tibia in question belongs to a later period than the other fossils with which it was found—a result in which M. Rivière acquiesces on other grounds.

The Miocene Mammals of France.

It is not a little remarkable that the wonderful discoveries of Tertiary mammals made during the last twenty years in the United States have been paralleled by a great advance during the same period in our knowledge of the fossil mammals of Europe, and more especially France. As our readers may be aware, it is within that space of time that we have become acquainted with the marvellous fauna of the Phosphorite beds of Central France through the labours of Dr. H. Filhol, while our knowledge of the mammals of the Oligocene beds of St. Gérand-le-Puy, and of the Miocene of Sansan in the Gers, has been greatly augmented. It is true, indeed, that the extinct European mammals of these horizons do not include many of the strange forms characterising the equivalent beds on the other side of the Atlantic; but they comprise, on the other hand, many types which are quite unknown there, and their importance to the palæontologist and evolutionist can scarcely be overestimated.

Among the faunas of which our knowledge has advanced with such rapid strides during the last few years is that of the village of La Grive-St.-Alban, in the Isère, belonging to the upper division of the Miocene period. The mammals of this fauna were first brought to notice by Dr. Jourdan in the year 1861, and new forms from the same beds were subsequently described by MM. Filhol, Chantre, and Depéret; but it has been reserved for the writer last mentioned to make us acquainted with the full extent and importance of the fauna in question. This task has been completed (so far as present materials permit) in a memoir just published by Dr. C. Depéret,[1] which treats not only of the mammals of La Grive, but likewise of those from other *gisements* in the Rhone basin; and when we mention

[1] "La Faune de Mammifères Miocènes de La Grive-St.-Alban (Isère) et de quelques autres Localités du Bassin du Rhone." *Archiv. Mus. Lyon*, vol. v., pp. 1–94, pls. i.–iv.

that this memoir has been published in the *Archives* of the Lyon Museum, it goes without saying that, both as regards letterpress and illustrations, it is all that can be desired.

From the La Grive deposits, Dr. Depéret now recognises upwards of 46 species of mammals, the great majority of which are identical with those of Sansan. Indeed, most of the species peculiar to the Isère locality are mammals of small size, the remains of which are naturally liable to escape detection. The most interesting part of the memoir before us relates to the peculiar creature known as *Macrotherium*, and it is to this portion that our few remarks will, in the main, apply.

It may be within the knowledge of many of our readers that the Miocene Mammal known as *Macrotherium* was first described on the evidence of claw-bones of the feet, which apparently indicated an animal more or less closely allied to Edentates, like the living Pangolin and the extinct *Megatherium;* and that, although other bones of the limbs had been obtained, its skull appeared to be still unknown. On the other hand, the mammal which had been previously named *Chalicotherium* was known merely by the skull and teeth, which resembled those of certain Perissodactyle Ungulates. Now, only a few years ago, Dr. Filhol startled the zoological world by the announcement that the presumed Edentate *Macrotherium* and the Ungulate *Chalicotherium* were really one and the same, the limbs of the one and the skull of the other having been found in association at Sansan; and it is not a little remarkable that this discovery should have been so shortly followed by a second at La Grive, where a whole skeleton was likewise brought to light. Unfortunately, however, this specimen was broken up before Dr. Depéret appeared on the spot; and when he arrived it was only possible to rescue the limbs, skull, and a few vertebræ.

The results of the observations of the learned author have been to show that, although *Macrotherium* of the Sansan and La Grive Miocene is closely allied to *Chalicotherium* of the overlying Eppelsheim beds, yet that the two are really distinct, the so-called *Ancylotherium* of the Pikermi beds being identical with the latter. The chief point of difference between the two is to be found in the circumstance that, whereas in *Macrotherium* the fore limb is much longer than the hinder—this being especially shown by the radius being nearly twice the length of the tibia—in *Chalicotherium* the two limbs are approximately equal. There is, moreover, a difference in the structure of the bones of the claws, those of *Chalicotherium* being less deeply cleft than in the other genus; and it is curious to observe that in both these respects the later form is less specialised than the older one.

In regard to their affinities, it may be observed that Dr. Filhol was disposed to regard these remarkable creatures as most nearly allied to the existing Edentates, his opinion being largely based on the circumstance that the skull exhibited the smooth contour in the region

of the brain-case characteristic of these animals. The specimens described by Dr. Depéret show, however, that this absence of ridges on the skull is entirely due to the immaturity of Dr. Filhol's example; and the Edentate theory is, therefore, once for all demolished. On the other hand, from the fact that the molar teeth of *Chalicotherium* and *Macrotherium* are extremely close to those of the extinct Perissodactyle genus *Palæosyops*, while many features in their osteology—especially the transverse elongation of the head of the radius—are likewise of an Ungulate type, Dr. Depéret comes to the conclusion that these creatures were merely Perissodactyle Ungulates specially modified for a fossorial, or possibly an arboreal, life. In this view we are entirely disposed to concur, seeing that all the peculiar features of these animals are just those which might have been produced by adaptation for a particular purpose from an ordinary Perissodactyle Ungulate. Still, however, it must be confessed that to find an Ungulate armed with long curved claws, and furnished with disproportionately elongated fore-limbs, like those of a sloth, comes as somewhat of a shock to our ordinary preconceived ideas of an Ungulate.

The second most interesting point in the memoir before us relates to the gradual approximation to modern Pigs presented by the molars of the later representatives of the extinct Oligocene and Miocene animals known as *Hyotherium*. In the Oligocene representatives of this genus (separated by Dr. Depéret as *Palæochœrus*), the last molar has scarcely any hind talon; but in the different varieties of *H. soemmerringi* of the Miocene the author finds such a gradual increase in the length of this talon as to render it difficult to determine where *Hyotherium* ends and *Sus* begins.

The Quarternary History of the Baltic.

Of late years, so much has been written on the Baltic and its glaciation, that we are glad to welcome a summary of the present state of our knowledge, especially when it is accompanied by a full bibliography.

In a paper of 120 pages, Herr Henr. Munthe deals with the various Quarternary deposits of that region (*Bihang till K. Svenska Vet.-Akad. Handl.*, band xviii., afd. ii., no. 1). Following Professor Nathorst, he divides the Quarternary period into five epochs: Post-glacial, Younger Glacial, Inter-glacial, Older Glacial, and Pre-glacial. Then, taking these epochs in regular order, he describes the deposits and the fauna and flora which characterise each. As regards the so-called "Pre-glacial" strata of the Baltic, we cannot help feeling somewhat doubtful, for at present the list of fossils does not contain a single characteristic "Pre-glacial" species, and it is not clear how the fauna is to be distinguished from that of the "Inter-glacial" deposits. The reindeer and dog (*Canis familiaris*, var. *groenlandica*), both unknown in our Pre-glacial deposits, are the only land mammals of which the

species is given. The elephant and the rhinoceros are recorded, but without specific name. As the common Pre-glacial elephant (*Elephas meridionalis*) and Pre-glacial rhinoceros (*Rhinoceros etruscus*) are quite distinct from the mammoth and the woolly rhinoceros, the determination of these species might help to settle the question.

The Geological Survey of Great Britain.

We are glad to learn from the Report of the Director-General of the Geological Survey that a re-survey of the South Wales coal-field has been commenced on the scale of six inches to the mile. From a practical point of view, no more important work could be undertaken by the Geological Survey, but we fear that some time must elapse before the work is accomplished, as at present only one officer—Mr. Aubrey Strahan—is engaged in the survey of that large area. The original survey was made by De la Beche, Logan, and others, about fifty years ago. As there are other coal-fields, including those of the Forest of Dean, Bristol and Somerset, South Staffordshire, &c., that should be surveyed geologically on the six-inch scale, it would seem necessary to increase the staff if the work is to be carried out during the lifetime of anyone now living.

The survey of the north-west Highlands has been vigorously prosecuted, and the Director-General draws attention to the important scientific results that follow from the determination of the Pre-cambrian age of the Torridon Sandstone. The aid of photography has been appropriately introduced to illustrate the structure of the Archæan Gneiss. Another important piece of work is the determination that the "Calcareous Sericite-schists," which extend as a traceable band through the whole of Perthshire into Forfarshire, are a continuation, in a more metamorphosed condition, of the shales or slates of Ardrishaig.

Archæology in Dorsetshire and Wiltshire.

We have just had the opportunity of seeing the large quarto volume, entitled "Excavations in Bokerly and Wansdyke, Dorset and Wilts," by Lieutenant-General Pitt-Rivers, F.R.S. This forms the third volume of a series of researches which he has been carrying on.

In the two former volumes General Pitt-Rivers described the excavations he had made during a period of ten years in the neighbourhood of Rushmore, in Wiltshire. The particular tract is a part of Cranbourne Chase, that lies along the Wiltshire and Dorsetshire Downs, south-east of Shaftesbury. Evidence was obtained of two Romano-British villages, just outside the park of Rushmore; both villages were alike in their general arrangement, and their chief feature consisted of pits, 3 feet 6 inches to 10 feet in diameter, and 3 feet 6 inches to 9 feet deep. The pits were filled with earth and

refuse, so that no trace of them could be seen at the surface, but upwards of 90 were found in each village, and in both cases they were drained by deep trenches. The excavations showed that one of the chief concerns of the inhabitants in those days, was to carry off the heavy rain, of the prevalence of which there is historical and other evidence. Thus a well 188 feet deep was re-excavated, and the Roman bucket found at the bottom, but no water, suggesting that the water-line or ordinary plane of saturation must have lain somewhat higher in the hill in former days than it does at present. The pits were probably used to contain refuse, and they were subsequently used as places of burial.

Among the animal remains, those of horses, oxen, and sheep were of small size, the horse rarely exceeding the size of the Exmoor pony, while the sheep were of a breed the like of which is only to be found at present in the island of St. Kilda. The pig, but slightly removed from the wild boar, was of large size, and the dog varied from the size of a mastiff to that of a terrier. The people themselves were of small stature.

Of the many interesting remains there were a number of iron styli, which showed that the people were able to read and write; and one decorated tablet of Kimeridge shale appeared to be of the kind used for writing upon with the stylus, by means of a coating of wax spread over the surface.

During the past three years (1888–1891) General Pitt-Rivers has turned his attention to the Bokerly Dyke and Wansdyke, and the evidence upon which the date of the latter has been to some extent determined, has been derived chiefly from the careful record of discoveries made in the two Romano-British villages before described. Both earthworks, at the places where excavated, are Roman or post-Roman, and the theory of the Belgic age of the Wansdyke has been completely overturned.

Bokerly Dyke, the present boundary-line between Dorset and Wilts, is an entrenchment of high relief, nearly four miles in length, running in a north-west and south-east direction, across the old Roman road, which runs from Sarum to Badbury.

The well-known Wansdyke runs, or probably did run, from the fenny country in the neighbourhood of the Severn at Portishead, by Bath, passing to the north of Devizes into Savernake Forest, and on to Chisbury Camp, where it turns and runs southward in the direction of Andover.

Full accounts of the excavations are given, and also of the Pottery, Coins, and implements of Bronze, Iron, and Bone that have been obtained. There are notes on the Human Remains from Woodyates on the Bokerly Dyke, by Dr. J. G. Garson; and notes, for comparison, on human skulls from Humsbury Camp, Northampton, and from the Roman Villa at Llantwit Major near Cardiff. Numerous plates serve to illustrate the characters of these remains and of the other objects

obtained. In fact the work is full of valuable material of interest to the Antiquary and to the student of Prehistoric Archæology, where Geology may be said to merge into History. Referring to the detailed nature of his work, and especially to fragments of pottery, the author says that "tedious as it may appear to some, to dwell on the discovery of odds and ends, that have, no doubt, been thrown away by their owners as rubbish, ... yet it is by the study of such trivial details that Archæology is mainly dependent for determining the dates of earthworks."

Although General Pitt-Rivers holds the appointment of Government Inspector of Ancient Monuments in Britain, the excavations have been conducted entirely at his own expense. In Germany extensive explorations of a similar nature have been carried out by the Government of that country; but, as the author remarks, "it is, perhaps, more in harmony with the recognised custom of our country to leave such works for private enterprise." Fortunately, General Pitt-Rivers has had the three most necessary requirements for original research —time, money, and ability. He acknowledges also the aid of his private staff of four assistants.

The volumes are printed privately, being intended for workers. The author believes there would be no demand on the part of the public for a work of so much detail, for "few persons, even among those who attend archæological meetings, put themselves to the trouble of checking opinions, by sifting the evidence upon which they are based."

The specimens have been placed in a museum erected by the author at Farnham, in Dorset. He is, however, of opinion that, so far as the general public is concerned, museums must be supplemented by other inducements to make them attractive. Within a short distance of his museum he has formed a recreation ground, where his private band plays every Sunday evening in the summer. This ground was attended last year by 16,839 persons, and nearly half of the number visited the museum. General Pitt-Rivers has likewise built a Museum Hotel, so that visitors can find first-class accommodation. He thinks it a mistake to suppose that country towns are the best localities for local museums. He finds the attendance at the Dorchester Museum only 2,826 as against 7,000 at the Farnham Museum; and no doubt the outing is in itself an important accessory in a visit to a country museum, for it draws people out of the towns.

Indexes of Science.

We are glad to see the discussion now in progress in *Nature* about the recording of Zoological literature. It is an old story, and suggestions are many; but those willing to devote the necessary time to recording are few and far between. That something must be done is evident, for over and over again we meet with papers in which

strange ignorance of previous work is patent; and this is not to be wondered at, for did not Sir William Flower, in his recent book on the horse, tell us that more than two thousand papers had been written on that animal alone? We are not surprised, therefore, that authors avoid, or ignore, such masses of literature—life is not long enough as it is, and we cannot spend the whole of it in finding out whether a previous author has counted the spots on a beetle's wing-case, or labelled the hairs on a caterpillar. The suggestion made by one of the correspondents in our contemporary, that there should be a central agency—a Romeike, in fact, for Natural History—is an excellent one; but who is to carry the project into execution? For such an agency must be complete and perfect, else one is no better off than before. There are too many *attempts* at indexes and bibliographies; what is wanted are not attempts, but completed efforts. Such works should be undertaken by the larger institutions, and undertaken on the distinct knowledge that they will be a financial loss. The lamentable indifference of the scientific public, and even of the institutions themselves, is always felt by the compiler or the publisher of one of these time-saving books of reference. The Naturalist will not buy a book; rather than spend his few shillings, he will suffer any amount of inconvenience, and "wait till he goes to the library next time" to see it. The societies grant small financial help to bibliographic work, and when they actually undertake the publication of records, these are often done in such a careless manner as to be almost useless. We have in mind several dreary examples of works which should be of infinite value, but which are so imperfect or unreliable as to be more trouble than relief. In short, the question of providing records for the use of working naturalists has yet to be solved, and the sooner the leading academies of the world take counsel together and arrive at a mutual understanding, the better will philosophy be advanced.

The Nomenclature of Insects.

From the general question of recording, we may turn to the difficulties in the nomenclature of insects. The number of brief notes and papers in which "new species" of insects are described is appalling. We place "new species" in inverted commas advisedly, for an intimate acquaintance with entomological literature impresses us with the belief that it is absolutely impossible for the authors who write on many of the groups to have read a tithe of the papers previously published on the subject. Of men we know, their very years make it impossible, even had they read from their cradles onwards. Moreover, a large majority of these so-called "new species" are shortly described, but *not* figured. Now, it is not reasonable to imagine that any specialist can read, say, one hundred descriptions of butterflies, closely allied, and keep a clear idea from mere text of the minute differences insisted upon. It ought not to be allowable to

publish a description of any animal without some figure, either of the animal itself or of its special feature; and the more so would we urge this point in reference to insects, since Dr. Riley has almost paralysed us with the announcement that there are something like 10,000,000 insects altogether. At present, we can only be thankful that they have not all been discovered, and in their discovery we can well afford to "go slow." It is, of course, essential to describe new varieties or "species" when they occur, but surely what is wanted in entomology is more general monographing, and fewer scrappy papers on two or three disconnected forms.

The Grasses of our Pastures.

The recently-published *Annual Journal of the Bath and West and Southern Counties Society* contains an interesting paper with the above title, by Mr. W. Carruthers, who has in several ways lately contributed towards the diffusion of useful knowledge among those practically interested in agriculture, notably by the preparation of the unique set of wheat diagrams noticed in Natural Science for June. In the present paper the author gives the results of a personal examination "of many of the best pastures of England," and also describes and criticises the observations and views of others.

However rich a natural pasture may be, it will always contain plants of no value as food, which are left untouched by animals when feeding; hence a natural pasture is always capable of improvement by the removal of such useless plants. Grasses form the principal bulk of all pastures. Our flora boasts 107 kinds of grasses, all of which are not available or of equal value for pastures. In fact, when we have deducted those which only occur high up on Scotch mountains, above the limits of cultivation, those found in cornfields, on dry walls, in sandy dunes, the water-grasses, those which are very rare, and those undesirable in pastures, we have left only 47 species which may be regarded as the pasture-grasses of England, any one or all of which, if eaten by stock, would be nutritious.

The feeding value of a grass differs greatly at the different stages of its life; in proof of this a table is given based on data contained in a paper by Mr. David Wilson. Here we have a record of the comparative food-value of twelve grasses at different stages, from which it appears that when in full leaf a grass is worth nearly twice as much as when the seeds are ripe. An amount of cocksfoot, sufficient when in the earlier stage for seventy animals, would, when beginning to bloom, only satisfy fifty, after bloom forty-four, and when the seeds are ripe, only thirty-five. The plant in full leaf is vigorously growing and full of plastic food material, which, as the flowering period is reached, becomes worked up into insoluble fibre, or expended to supply energy for the exhausting process of flowering and subsequent production of fruit. Hence the gradual decrease in feeding-value with increasing maturity.

A second table gives the composition of 100 parts of meadow fescue and cocksfoot at different periods. It shows that the amount of digestible albumenoid in the full-leaf stage is about six times as great as in the seeding stage, and also that the most economical proportion of digestible food—one of albumenoids to about five to seven of carbohydrates—is found only in the leaf stage. It follows, therefore, that "the most nutritious pastures are those in which the plants are not allowed to flower and seed, but are kept in leaf by continual grazing of stock," and on this point botanists, chemists, and practical graziers are all agreed.

Apart from the feeding value of a grass, its palatableness must be considered. Animals, as in the case of man, have their likes and dislikes, and a grass, however nutritious when the only available food, becomes useless in a pasture where it is passed over and left untouched. Hence it is important to know what plants are selected and what are rejected by grazing stock. The identification of the former is not an easy matter, as favourite grasses are so much eaten down that it is hard to recognise the species. But, as the author suggests, by enclosing a few square yards of grass land that has been thus eaten down, the stock will be kept off, and the various species will grow up and flower, and can then be easily identified.

As regards rejection of plants by stock, Mr. Carruthers suggests a very simple method of determination. It is merely to note those grasses which have been allowed to run to seed, and have, therefore, obviously been passed over, as in eating the leaves cattle will also bite off the top of the undeveloped stem, and thus prevent flowering. By this means, the farmer will ascertain not only what grasses he is to reject from mixtures for new pastures, but also what weeds should be got rid of; for the latter, though perfectly innocent in themselves, are occupying the space and consuming the food which would otherwise be at the service of nutritious plants. Yarrow and ragwort are both found in many pastures; the former is palatable to the stock, and is everywhere eaten down, but every plant of ragwort runs up to flower, thereby proving its unpalatable nature.

A short account is given of the "Rye-grass Controversy." Scientific agriculturists are divided in their opinions as to the value of this grass. Mr. De Laune observed that his rams passed it by when selecting food in a mixed pasture, and also attributed the failure of new pastures after three or four years to the fact that rye-grass, which formed the principal bulk of the mixtures employed, is a short-lived grass. Dr. Fream, on the contrary, adduces the universal presence of rye-grass in old grass lands, frequently forming a large percentage of the whole, as evidence of its value, and also asserts that its persistence in an old pasture proves it to be perennial. Its presence is, however, no proof of value as a food material, but argues against it, if the grass is short-lived; for then it could only persist if allowed to seed, and this it could not do if eaten down by stock. A

remark of Sinclair's in his *Hortus Gramineus Woburnensis* is also against Dr. Fream, for "rye-grass," he says, "is but a short-lived plant . . . continued by its property of ripening an abundance of seeds." He also draws attention to the fact that the seed-stalks of rye-grass form a large proportion of the bents in an old grass-land.

As regards other grasses, dog's tail, barley-grass, yellow oat-grass, sweet vernal grass, and the different kinds of agrostis cannot be reckoned among the valuable grasses in a meadow. On the other hand, cocksfoot, fox-tail, meadow fescue, tall fescue, and timothy are eaten down so closely as to be scarcely discernible by an ordinary observer, and wherever one or more of these form the bulk of a pasture it has a high character. The meadow grasses may also be classed with these. In the case of grasses that are equally palatable to stock, the amount of nutritious food produced is an important consideration in estimating their value.

The paper concludes with a table of results obtained by Mr. Wilson bearing on this aspect, from which it appears that the feeding value of the produce of a given area in cocksfoot is twice as great as in rye-grass, and more than three times as great as in sheep's fescue.

Lamarck's doctrines are still in the ascendant in America, and the latest contribution to philosophy of this kind has reference to the mechanical origin of the scales of fishes (*Proc. Acad. Nat. Sci. Philad.*, 1892, pp. 219–224). Professor J. A. Ryder, in treating of this subject, points out that the scales are primitively arranged in direct relation to the muscular segments of the trunk; and he believes that the earliest scales were rhombic, because the connection between the muscle-plates and the lower layer of the skin is such, that the integument would be thrown into rhombic areolæ during contraction.

An interesting case of commensalism is recorded by Mr. A. Alcock, in the September number of the *Annals and Magazine of Natural History*. In 1889 two specimens of a small scorpænoid fish (*Minous inermis*) were dredged off the Godávari Delta, on the Coromandel coast, one of which was covered with a fleshy colony of polypes, since determined as new and named *Stylactis minoi*. The same association of fish and polypes was again observed in a haul off the Malabar coast in November, 1891, and also in January this year, when a single specimen of *Minous* was taken between the Deltas of the Ganges and the Máhánadi. Although numerous other fishes were hauled at the same times, only the *Minous* was found to be infested with the polypes. Mr. Alcock inclines to the belief that this investing growth assists the fish to obtain food by giving it a deceitful resemblance to the incrusted rocks of its environment, while the polypes obtain in return a more abundant supply of food than if stationary or independent.

In an earlier number (*suprà*, p. 101) reference was made to the peculiar group of large horned or hornless Ruminants more or less closely allied to the Giraffe, such as the Helladothere of Greece, the Samothere of Samos and Persia, and the Sivathere and Hydaspithere of India. Hitherto this group has been known only from the countries above-named, but we have now to record that a member of the assemblage has been obtained from the Pliocene strata of Oran, in Algeria. For this new creature Mr. Powel proposes the name of the Libythere (*Libytherium*). This new discovery is of the more interest, since it shows that the ancient Pliocene Giraffes of Europe and Asia were accompanied in their migration into Africa by a member of the Helladothere group, although whether the latter ever reached the true Ethiopian region must await further investigation.

Professor Karl Möbius has recently been studying the hair of the existing elephants, and communicated the result to the Royal Prussian Academy of Sciences. He finds that, in addition to the sparse long hairs, there are most distinct traces of a fine under-fur; and this fact is of great interest in connection with the remarkable development of the wool in the extinct mammoth of Siberia.

An account of "Charles Moore, F.G.S., and his work," has been contributed to the *Proceedings of the Bath Natural History and Antiquarian Field Club*, by the Rev. H. H. Winwood. Moore's life was largely devoted to palæontological studies among the Rhætic, Liassic, and Oolitic rocks, more especially of Somersetshire and Glamorganshire. To him, mainly, we owe the recognition of the Rhætic beds in this country. His chief work was, however, among the fossils, and the large collection he gathered together has brought fame to Bath, in whose museum they were placed. Unfortunately, nearly all the specimens have lately been removed from the room in which they were arranged by Moore, into an upper room, the Saurians affixed to the walls remaining below. The memoir is appropriately accompanied by a list of the fossil types and described specimens in the Bath Museum by Mr. Edward Wilson; and we are informed that Mr. A. Smith Woodward has in preparation a monograph of the Fossil Fishes of the Upper Lias, an exceedingly fine series having been obtained by Moore from Ilminster.

At the recent meeting of the British Association, Dr. Schmitz of Greifswald, made an important communication on the tubercles formed in the fronds of certain red seaweeds of the family Florideæ. These he attributed to the action and growth within the tissues of Bacteria—a remarkable discovery in many ways, since the sea must now reckon its Bacteria, and the plant-world this time furnishes another instance of disease caused by this agency. Mr. George Murray criticised Dr.

Schmitz's position with some severity, pointing out the cases of similar malformations described by Miss Barton, caused by copepoda, nematode worms, &c., and further insisting that since no proof of infection was forthcoming, there was no more than a superficial reason for believing that these Bacteria were *propter hoc*—he strongly inclined to regard them as *post hoc*.

The vines at Pyrgos, on the West Coast of Greece, have been attacked by a disease which has caused great havoc. A letter from Vice-Consul Fauquier, reproduced in the *Kew Bulletin*, states that some localities are literally devastated, others to the extent of 50 to 60 per cent., while those that have suffered less show a damage of 10 to 20 per cent. Unfortunately, the disease shows no sign of abatement; even if it makes no further progress, the damage done in Pyrgos and Olympia probably amounts to a third or half the crop. The Black Corinth or Zante grape-vine, from which the currants of commerce are obtained, is a variety of the common vine, with seedless fruit; it is cultivated chiefly on the mainland of Greece and the Ionian Islands.

The September number of *Grevillea* is "under new management," namely, the editorship of Mr. Massee and the active co-operation of the proprietor, Mr. Batters. Two plates are novelties, and, though very "processy," are better than nothing. The new management would do well to depend more on readable contributions than on pages of lists and diagnoses. The mantle of Dr. Cooke has indeed descended on Mr. Massee, and it has not proved a misfit. A useful part of the new number is that devoted to critical and bibliographical notices, these being good-humoured and impartial in all cases except one (p. 27). The last would have been intelligible under the old *régime*, but it may be doubted if the new management is acting prudently in taking over the *ill*-will of the business.

The series of Swiney lectures on Geology, to be delivered at the Natural History Branch of the British Museum this month, is the only official attempt made during the year to popularise the vast and interesting Natural History collections stored in the National Museum. It is, indeed, difficult to comprehend why the authorities should have even omitted to build a lecture theatre when the grand idea of a Museum of Natural History was being realised. In the original plan, as sketched by Sir Richard Owen, this most important feature was not omitted. At present, the lectures are given in the Geological Library, with the result that numbers of people are unable to obtain admission, while the regular staff and students are seriously inconvenienced in their work and researches. The great popularity enjoyed by Dr. Nicholson has, no doubt, much to do with the success of the past two courses.

I.

Biological Theories.

II.—THE EVOLUTION OF HEREDITY.

IN the previous article it was contended that the phenomenon called "heredity" was merely a likeness of effects due to likeness of the causes producing them; the like effects being two similar individuals belonging to different generations of the same species, the like causes being the influences internal and external which have determined the form, structure, and constitution of those two individuals respectively.

The present paper attempts to account for the recurrence, generation after generation, of those "like causes."

In the search for the efficient cause of this recurrence, that is for the cause of heredity, it is necessary to remember and compare the diverse forms in which heredity is exhibited, and at the same time to take due cognisance of the existence and nature of variation.

The following facts will serve as types of the groups of phenomena which it is especially necessary to bear continuously in mind with reference to heredity:—

1. A man not infrequently resembles his father in some features which are comparatively rare in other men.

2. All *Rhabdonemata* closely resemble their grandparents, while they differ so widely from their parents (*Rhabdites*) as to have been referred to a distinct genus. Both *Rhabdonema* and *Rhabditis* are produced sexually.

3. "Soldiers" among one generation of *Termes* are very like "soldiers" of previous generations, and yet quite unlike their parents, and unlike any ancestor whatever, however remote. The occurrence of "soldiers" among the offspring of several species of *Termes*, which geographically are widely separated, appears to point to their occurrence as a phenomenon of very great antiquity.

4. In *Phylloxera* each generation consists of individuals like each other, except in sex, but unlike any individual of the three or four preceding or succeeding generations. In each year there is a cycle of unlike generations, but the individuals of any one generation closely resemble those of the corresponding generations of previous

cycles. Only one generation in each cycle consists of males and females.

5. Hybrids frequently, though not always, exhibit the characters of both parental types blended together. Not infrequently they bear a closer resemblance to a more remote ancestor than to their immediate ancestors (atavism).

6. Though some cells divide, like *Amœba*, into two *like* halves, cell-division not infrequently results in the production of cells which are *unlike* each other, *e.g.*, *Vorticella*, segmenting ovum (after the first two or three divisions).

With reference to Variation, it must be borne in mind that—

1. Slight variation is almost universal.

2. Conspicuous variation, though less widespread, is very marked in certain species and under certain conditions.

3. Conspicuous variation is in some cases more noticeable in the larva than in the adult. This is especially true of some caterpillars.

4. An individual which differs conspicuously from all its near relatives, not infrequently bears a closer resemblance to remote ancestors (atavism).

5. Variation is often very great under domestication and under the influence of a changed environment.

* * * * *

The schools of Pangenesis and of Continuity alike regard heredity as the original and normal course of events, and variation as a secondary and abnormal occurrence brought about by the fortuitous interference of some extraneous influence. Even modern text-books speak of "heredity" and "variability" as if they were concrete entities capable of producing effects. One would laugh at the philosopher who sought to explain the turning of a weather-cock to the south-east on a particular occasion by referring it to the bursting into activity of a long-latent tendency to turn to the south-east. The "tendency to vary" and the "phylogenetic force" of some writers, the "force of heredity" and "force of variability" of others, are equally absurd. T. J. Parker says[1] "*Heredity* . . . is modified by *Variability*," and earlier writers are not less slip-shod in their treatment of these and allied subjects.

It is, however, not so much this blundering between the abstract and the concrete that I wish to emphasise, as the unanimity with which heredity has been regarded as the primitive, and variation as the modified phenomenon. This aspect of current views may be illustrated by comparing heredity to the regular movement of a planet in its orbit, and variation to a deviation of that planet from its orbit through the attraction of some larger body or bodies passing near it.

I propose to turn all this upside down, and looking upon variation as a phenomenon of greater antiquity than heredity, to consider

[1] "Elementary Biology," 1891, p. 145.

by what means it has been limited, by what means the limitation has been progressively narrowed, and by what means the narrow limitation is now maintained. Heredity, in fact, I propose to treat merely as the narrow limitation of variation, and I hope to be able to show that if no such phenomenon as heredity occurred in early stages of evolution, it would of necessity arise by degrees through the action of well-known influences, and without the intervention of any unknown force or gemmule.

At the outset, I am hampered by a difficulty in terminology. "Natural Selection" is a term so familiar in the Darwinian sense, that were it not that I believe a change in the meaning of the term would add to its usefulness, I should not venture to use it at all. One essential factor of "Natural Selection" (in the Darwinian sense) is heredity; and hence natural selection in that sense cannot rightly be invoked to explain the origin of one of its own factors.

Heredity being established, variation being an observed phenomenon, and the struggle for existence being in progress, then "Natural Selection," in the Darwinian sense, will account for the origin of new species. Such is, I believe, essentially the view held by all Darwinians, but I find it necessary, for present purposes, to eliminate the element of heredity entirely from the meaning of the term, and to use the term, so altered, to express the survival of the fittest, even before heredity existed, and independently of heredity. The change in the meaning consists in what is, I believe, called by logicians a limitation of its intension, and consequent enlargement of its extension: it renders the term applicable to a group of phenomena not previously included within its signification, but in no way affects its application to the phenomenon usually suggested by it, except, perhaps, to give it increased accuracy.

It is to Natural Selection in this wider sense, to the survival of the fit before heredity existed, that I now seek to refer the origin of heredity, to the survival of the fittest that I seek to refer the maintenance of those limitations of variation which collectively constitute heredity.

The terms "offspring," "generation," and "parent," for convenience, may be applied even in the case where multiplication is effected solely by fission. I shall speak of the two *Amœbæ* resulting from the fission of a pre-existing one as the "offspring" of that from which they arose, and the latter I shall speak of as the "parent" and as belonging to the previous "generation."

In the earliest stages of the evolution of living things, whatever those stages may have been, there is no necessity, and, it appears to me, no justification for the assumption that heredity existed or occurred. A living mass might well differ in its various parts, and, if so, the separation of two successive portions from one larger parent mass need not necessarily involve the likeness of those separated portions to each other in every detail of structure and constitution,

nor need it involve the likeness of either of them to the parent mass as a whole. Among the separated portions which, by virtue of their constitutions (chemical and physical), were capable of life and growth, there must have been likeness in all those characters which were, under the conditions then existing, necessary to life and growth. Any separated portions not possessing those characters must have died, and need not be further considered.

After many generations all individuals living must have agreed not only in possessing all the characters necessary to life, but also in having descended from an unbroken series of ancestors similarly endowed; and this, not because these characters were of necessity present in all the offspring of all the ancestors, but because all which did not possess them must have failed to live, and therefore to leave descendants. This weeding out of the unfit individuals independently both of heredity and of competition, and the correlated survival of *all* the fit and not of the fittest only, is the first stage in the evolution of heredity. Those stocks fail to increase in numbers whose constitution does not determine the production, by some means or other, of new individuals capable of life, growth and multiplication. Within those limits, however, variation may be unchecked. Similarity in form, size, colour, etc., need not exist among the individuals. A large proportion of the offspring may indeed be incapable of life, but those stocks which produce the largest numbers of new individuals capable of life and multiplication will, by virtue thereof, gradually become most numerous. The first stage in the evolution of heredity is the weeding out of stocks whose variation in certain directions renders them incapable of numerical increase. Let this not be misunderstood; I do not mean that in some stocks there may be a "hereditary tendency to variation" which leads to failure in the struggle for existence; I do not assume that the offspring are from the first like the parents. I have already denied that the division of a cell into two necessitates the likeness of those two; even at this stage of evolution the vast majority of new individuals may be incapable of life. The favoured stocks are those which, by virtue of their constitution, however widely it may vary, are capable of producing an increase in the number of individuals, the variation among the individuals being thus far limited only with respect to those characters which are essential to life.

The second stage in the evolution is marked by the commencement of the struggle for existence in the ordinary sense of the term, that is, the beginning of competition: it is the stage in which each individual comes to be an adverse element in the environment of others. This is the stage when the fit compete against each other, and the fitter alone survive. The competition between individuals is, however, not of immediate interest to us; it is the struggle between stocks, which is the efficient factor in evolution. As individual competition renders certain characters in the individual advantageous to

the individual, it thereby renders the chemical and physical constitution of the stock, upon which the endowment of the individual with those characters depends, advantageous to the stock. The efficiency of the selection depends entirely upon its giving the advantage to those stocks among the new individuals of which the characters advantageous to the individual are numerically most frequent. All stocks may produce some individuals well-fitted and others ill-fitted for competition in various proportions. Those stocks alone succeed in the competition which produce on the average the greatest proportion of well-fitted, the smallest proportion of ill-fitted. That is, the constitution which determines the production of a large proportion of well-fitted new individuals confers an advantage upon the stock possessing it. A constitution which determines undue frequency of variation in a direction disadvantageous to the individual, is not only a disadvantage to the stock, but the disadvantage increases in its efficiency for elimination at every subsequent stage of evolution, till at last it becomes absolutely fatal, and every stock which is endowed with such a constitution is rooted out. Thus is variation in a stock first definitely limited. Stocks not only *differ* but also *vary* in fitness; every unfit stock, every less fit stock, every new stock varying in the direction of less-fitness is weeded out. As the intensity of the struggle increases, this limitation of variation of the stock in unfit directions becomes more and more definite. Thus only those stocks survive in which the constitution is such as to allow of variation only to a very limited extent in certain directions, while in other directions the variation is almost unlimited.

Such a limitation is not, however, what is ordinarily called heredity; it is not a simultaneous limitation of variations in all directions; there is in this supposed stage of evolution none of that thorough similitude among individuals which the continuity school have without justification assumed to have existed *ab initio*. In any one stock or group of stocks in one definite environment variation is, so far, limited in frequency and extent in certain directions only. The range of variation and its frequency is limited only with reference to certain characters; but as the struggle for existence increases in intensity, not only is this average range of variation progressively narrowed, but the limitation is gradually extended to characters hitherto varying almost unchecked. Characters hitherto unstable in the extreme are gradually reduced to stability. The number of characters thus rendered comparatively constant is gradually increased till it reaches near to infinity. The range of their variation is gradually narrowed till it becomes almost infinitesimal both in extent and in frequency. Genera, if not even species, are now established.

This relative constancy introduces a new factor. The whole constitution upon which so large a number of almost constant characters depends, must itself have been reduced to almost complete constancy. It is difficult to imagine that a constitution limited in its variations

so narrowly and in so many directions that none of the innumerable essential characters which depend upon it shall vary appreciably, can yet itself remain variable to any large extent. The variation in the constitution of a stock must, in fact, be extraordinarily slight if no variation shall be discoverable in any of the innumerable characters which that constitution determines in the new individuals. This constancy of stock-constitution, this constancy of the new individual constitutions itself is the guarantee of that constancy in the recurrence of the "like causes" to which I referred in the previous paper. The efficient cause of heredity which we set out to seek is found. It consists in a stock-constitution whose constancy has been gradually brought near to completeness by the indirect effect of natural selection upon it. The struggle for existence has eliminated all stocks varying beyond certain limits. The variation, whatever it may have been, has at every stage been the necessary outcome of the stock-constitution, and the elimination of stocks whose new individuals varied, has been at the same time an elimination of stocks of unfit constitution, that is, of unstable constitution.

Such a stock-constitution having been once established, it is obvious that, except through accident or other disturbing cause, the struggle for existence might well be suspended, even for many generations, and yet no marked variation might appear. On the other hand, a radical change in the environment capable of affecting the constitution *directly* (*i.e.*, not through natural selection) might in one generation blot out the limits of the species. A species whose specific constitution had, through long selection in marine waters, been rendered constant under constant external conditions, might well undergo a sudden or very rapid change on transference to fresh water. Whether the new constitution would be such as to lead to the production of new individuals capable of life and multiplication or not would be, in a sense, a matter of chance. The new individuals, if capable of living, might be unlike the older ones in form, and their new constitution might be such as to lead to wide variation in succeeding generations. If so, a new process of selection through many generations could alone render it again constant, and the new set of constant characters would be determined largely but indirectly by the new environment. A radical change of diet having altered the chemical composition of the circulatory fluid in the body of one of the higher metazoa, it is doubtful whether this would lead to such a change in the tissues nourishing the ova in process of formation as to involve the production of new ova unlike those of former generations; while we know not whether the new ova would be capable of development at all, and are ignorant of the form and structure of the new individuals if the ova did develop. It would thus be as gratuitous on my part to say dogmatically that increased variability in future generations would be the result, as it is for the continuitatists to dogmatically deny that the germ-plasm would be affected. It at

least appears probable that if such variability did arise, a new selection would again limit the variation, and reduce the species again to comparative constancy, though perhaps the new species so formed might differ radically from the old one. That such influences may have helped in the comparatively rapid transformation of species in nature, if not proven, at any rate appears possible.

Whether such *direct* action of disturbing influences actually has had a share in producing the transformation of some domesticated animals or not, it is difficult to determine; but the *indirect* effect of artificial selection is beyond doubt, and the comparative rarity of the "natural" coloration among rats bred in captivity, and apparently not even subjected to artificial selection, lends some support to my view that no character will remain constant for an indefinite number of generations unless that character be the immediate or remote object of some kind of selection. The phenomena described as resulting from "panmixia" are numerous enough, and sufficiently well known to justify me in stating that what my theory would indicate *à priori* is in accordance with observed facts. If heredity be a limitation of variation by the indirect action of selection, then the suspension of that limitation, as a consequence of the suspension of selection—in other words, increased variation under domestication—is comprehensible.

Variation having increased under domestication, a new selection, either natural or artificial, is found, if carried far enough, to produce new limitations of that variation; that is, to render new groups of characters hereditary, groups of characters which, in the case of domestic "breeds," distinguish each breed from others, as well as from wild congeners. It is thus that we have seen new breeds of poultry produced, breeds which have already become sufficiently constant to entitle them to the distinctive names they have received.

This constancy, however, is only relative: even the most constant of domestic breeds is probably less constant than almost any species evolved under the direction of natural selection. The "heredity" is not only not complete, but it is less so than in natural species. The average frequency of variation in the direction of ancestral natural species has been progressively diminished, but has not been reduced to zero. The stocks which most frequently gave rise to animals possessing the desired characters have been selected: those stocks which reproduced the ancestral characters either more markedly or more frequently have been exterminated, and thus both frequency and distinctness of the obnoxious ancestral characters have been reduced: but the characters do nevertheless still occur, and we call the recurrence of them "atavism." It is, in a certain sense, the progressive elimination of atavism by selection which constitutes the formation of a new species or new race.

Though thus offering an explanation of true atavism, I would not for a moment be understood to regard the great bulk of so-called

"atavistic" monstrosities as being of the same nature as the true atavism, shown for instance in the "reversion" of domestic breeds of pigeons to the type of the blue rock pigeon.

Monstrosities like two-toed horses, human beings with "persistent gill-clefts," and the like, belong to a class of pathological phenomena which are, in my opinion, in no way connected, or at most are only remotely connected with true atavism.

That the blending of two constitutions by hybridism should lead to a partial blending of characters distinctive of those two constitutions, is what might perhaps be expected *à priori;* and the exaltation of characters possessed in common by the two constitutions is similarly to be expected, but it is to be remembered that it is stock-constitutions, not individual-constitutions, to which I refer. In the case of pigeons, the characters common to all breeds are those implied by the name "pigeon." All pigeons alike have certain anatomical characters, but there are other characters which distinguish each breed from the others. There are certain constitutional characters common to all domestic breeds; for instance, all appear to be liable to "atavism," though its occurrence is comparatively rare. Experiment has shown (Darwin) that hybrids do possess some of the characters of the separate breeds from which they are derived, and that they are more liable to atavism than pure-bred birds.

Before proceeding to consider some more complex cases of heredity, it is well to summarise the conclusions reached so far. They are the following:—

(1) *Heredity is essentially a limitation of variation, and* (2) *is therefore of less antiquity than variation.* (3) *It is not an absolute limitation; variation still occurs.* (4) *The limitation has been effected by "natural selection" (using that term in a widened sense) operating upon the stock-constitution.* (5) *The limitation has been progressive, the range of variation having been narrowed step by step; the frequency of variation having been diminished step by step; and the number of characters whose variation has been thus affected having been increased step by step.* (6) *The efficient cause of that limitation, that is, "natural selection," being removed, the range and the frequency of variation increase.* (7) *A new selection leads to a new set of "hereditary characters," and this process is similar to that by which "heredity" has arisen.* (8) *The phenomenon of true atavism is simply the occurrence of those characters in a given individual which selection, natural or artificial, rendered less and less frequent, but the frequency of which has not yet been reduced to zero.* (9) *The blending of two stock-constitutions does in fact lead to the phenomena to be expected; notably to a combination of various features characteristic of the two stocks, and an exaltation of at least one characteristic common to the two constitutions,* i.e., *the occasional production of atavistic offspring.*

So far only the more ordinary types of heredity have been considered, those types and varieties which other theorists have sought to explain, that is, those cases in which the production of individuals fitted for successful competition in the struggle for existence, has been

limited to at most two kinds of individuals—males and females: the case of polymorphic species in which some individuals are incapable of taking any part in reproduction has now to be considered. The cases of alternation of sexual with asexual, and of dicœcious with hermaphrodite or parthenogenetic generations will need no special elucidation, for what has already been said and what has now to be said of the Termites, etc., will, with only slight alterations, apply to them also.

In non-social species such as birds, where the society, if it exists at all, includes at most two adults, the males and females alone do all the work, and any other individuals, if produced at all, would not only be useless competitors for food and doomed to die without offspring, but would also be a disadvantage to the stock. More accurately, that stock which, by virtue of its constitution, produced sterile individuals would thereby be at a disadvantage in the *struggle for perpetuation*. Such individuals are occasionally produced, but natural selection weeds out the stocks producing them frequently in the same way as it weeds out all stocks hampered by any other disadvantage whatever: and hence the percentage of "neuters" in *non-social* species is almost infinitesimal.

Among social species it is otherwise. Where all individuals work for the common good, the production of neuters need not be a disadvantage, and it may even be an advantage. The continuous labour of such members, uninterrupted and unhampered by reproductive functions, is of great value to a community; and the stock which, by virtue of its constitution, produces individuals either varying in reproductive capacity, or liable, under certain conditions as to diet, so to vary, is at an advantage as compared with other *social* stocks whose constitution determines the production of fertile offspring only. Hence natural selection will tend, within due limits, to progressively increase the average percentage of neuters (or potential neuters) among the individuals of successive generations. It is *not the individuals* which are selected, but the *stocks*. The unit, corresponding to the individual or a pair in an ordinary species, is in these species the whole society. The stock which is favoured by natural selection is not that producing the fittest individuals, but that producing the fittest society. The constitutional characters which natural selection favours are those upon which the production of the fittest society depends. What society is the fittest depends on the environment of that society in part, but largely also upon the organisation of the society itself. The environment and social organisation of the Termites, or "white ants," are such, that natural selection has determined a stock-constitution, one peculiarity of which is that, instead of the ordinary two types of offspring (male and female), there are six types. That constitution is kept constant by natural selection, and hence those six types of individuals are found among the offspring, if not of every "queen," at least of every society.

It was the facility for explaining this phenomenon offered by the theory I have set forth which first led me, in fact, to believe in my own speculation. Pangenesis always has appeared to me impossible, and "perigenesis" absurd, while "continuity" never appeared to explain the ordinary phenomena, even if such a continuity had been demonstrated to be universal. My own theory appeared merely plausible till consideration of the Termites made it credible.

Hitherto advantageous characters and disadvantageous characters have been referred to, and though I have not explicitly stated that *adult* characters are meant, I fear that, unless I emphasise the opposite of that interpretation, I may be understood to refer to such characters alone. Such an interpretation is by no means intended. When I speak of characters advantageous to the individual, the characters I really mean are the whole series of characters from the very earliest stage in the production of an ovarian cell to the very last moment of life. The favoured stock-constitution is the one which leads to the formation of those new individuals which throughout the whole course of their existence are on the average best fitted for competition at each successive stage. Just as I have insisted on the more or less independent establishment of each character of the individual, so I would now insist on the more or less independent establishment of every character, not only of the adult, but of the individual at every successive stage of development.

There are animals—some *Lepidoptera* for instance—in which the most striking characteristic of the larvæ is their variety, and yet the variation among the adults is so slight as almost to defy detection. If variation in those characters of the stock-constitution which determine the external characters of the larvæ be not disadvantageous to the species, natural selection will have no power to limit that variation, although it may impose the narrowest limitation upon the variation of that same constitution so far as the external characters of the adult are concerned. The only variations in the larvæ which natural selection can limit in extent or in frequency are those which are disadvantageous to the species as a whole.

The form of a caterpillar or of a nauplius is as strictly determined as that of the adult, probably even more strictly. If the efficiency of natural selection is proportional to the intensity of the struggle for existence, then it would seem strange if the stages in which most individuals die should be least affected by that selection. For every adult *Peneus* which dies without contributing to the multiplication of its kind, it is probably no exaggeration to say that a thousand succumb to the pressure of external circumstances in the "nauplius" phase. The characters of the nauplius I hold to be determined in the same way as those of the adult, and the "inheritance" of larval characters I claim to be explicable by the theory I have advanced for the explanation of "heredity" in general.

C. Herbert Hurst.

II.

The History of the Moas, or Extinct Flightless Birds of New Zealand.

SINCE the memorable occasion in 1839 on which Sir Richard Owen received the fragmentary thighbone that led him to infer the former existence of Ostrich-like birds in New Zealand, the unfolding of the history of *Dinornis* and its allies has been a subject of continual interest. The Hon. Walter Mantell's early discovery of the bones of these great Ratite birds mingled with charcoal in the refuse heaps of the Maoris, sustained the hope for a long time that some few survivors might be met with in one of the less known outlying districts of the islands; but subsequent researches have failed to realise the anticipation, and it is now tolerably certain that the race is quite extinct. Under such circumstances, our knowledge of the Moas—as these birds are termed in the Maori traditions—is based solely upon skeletons and the mummified relics occasionally found in fissures and caves. The determination of the genera and species is thus a matter of great difficulty, and no two authors are in precise accord. At a meeting of the Canterbury Philosophical Institute (New Zealand) in October last, Professor F. W. Hutton read a memoir attempting a revision of the whole subject, based upon the fine series of skeletons now preserved in the Colonial collections; and a copy of this interesting work has lately reached us. At the same time the Professor has favoured his European colleagues with the reprint of a newspaper article stating the results of his researches in connection with the more general questions involved. This article has a bearing upon problems of such wide interest that we make no apology for reproducing it in its entirety, before discussing the important advances made in matters of detail.

I.

That the Moas have been a long time in New Zealand is certain. In addition to the immense number of bones found in peat beds and river alluvia of Pleistocene age, remains have been found near Napier, and probably also near Wanganui, which belong to the Newer Pliocene period. The bones of a small species of Moa, found two years ago under a lava stream at Timaru, are still older

and probably Upper Miocene, while the Hon. W. Mantell found in 1849 a fragment of a bone, which probably belonged to a Moa, near Moeraki in beds of Lower Miocene age.[1]

The Ratitæ are generally supposed to have originated in the Northern Hemisphere, and to have spread southwards into Patagonia, South Africa, Australia, and New Zealand. But if so, how could birds which could not fly manage to reach New Zealand without being accompanied by any mammalia? Certainly they did not precede the mammalia, and it is very unlikely that they should twice have swum across straits which were impassable to mammals —once from the Oriental into the Australian region, and again from the Australian region into New Zealand—and there are other reasons for doubting the northern origin of the Australasian Ratitæ. The existing New Zealand Ratitæ (*Apteryx* or Kiwi) are smaller than any of the others, and make a nearer approach to the original flying ancestors; and we should expect to find the smallest and least altered forms near the place of origin. Now there are in Central and South America a group of birds called Tinamus, which, although flying birds, have been shown by the late Professor W. K. Parker to resemble the Australasian Ratitæ in many particulars, and as the connection between South America and New Zealand is well known, it seems more probable that the Moas originated in New Zealand in the Eocene period, from flying birds related to the Tinamus, and that they spread from here into Australia and New Guinea, than that they should have migrated southward from Asia.

In whatever way the Moas originated in New Zealand, it is evident that the land was a favourable one, for they multiplied enormously and spread from one end to the other. Not only was the number of individuals very large, but they belonged to no less than seven genera, containing twenty-five different species, a remarkable fact which is unparalleled in any other part of the world. Africa and Arabia are inhabited by but two or three species of Ostrich; South America, from Peru to Patagonia, has only three species of Rhea; Australia has two species of Emu and one Cassowary; while eight other species of Cassowary inhabit islands from New Britain to Ceram. Outside New Zealand two species of Ratitæ are rarely found living in the same district, while a few hundred years ago there were in New Zealand several different kinds of Kiwi, as well as the twenty-five species of Moas. An explanation of this problem may perhaps be found by examining the present distribution of the Cassowaries. Here we have eight species inhabiting five different islands, and if this region of the earth were to be elevated, and the islands joined together, these eight species would mingle. If the region were to sink once more, all of them would be driven to the highest land, and might be crowded into one small island. Now we know from geology that New Zealand has gone through a series of changes in

[1] May be earlier, according to Capt. Hutton's new memoir.—ED.

level, similar to those just mentioned. In the Miocene period it consisted of a cluster of several islands, which were elevated and united in the Older Pliocene, and ultimately divided into the two islands we have now in the Newer Pliocene. If the ancestors of the Moas inhabited New Zealand during the Eocene period, they must have been separated on these islands during the whole of the Miocene, and mingled together again in the Pliocene. In this way—*i.e.*, by isolation—probably the genera originated, but the species appear to be due to variations without isolation. As is the case with most common animals, the Moas varied greatly, and there being no carnivorous animals to hold them in check, while vegetable food was abundant, natural selection did not come into play, and the intermediate forms were not strictly eliminated. Under such favourable circumstances conditions of life were easy, and the birds got larger and fatter, more sluggish and more stupid. The oldest known Moa is one of the smallest, and it is the smaller species which are found in both islands; from which we may infer that they were the only ones in existence when the two islands were united, and that the Moas since then increased in size. But the very large Moas were always comparatively rare. The commonest kinds in the North Island were only from two-and-a-half to four feet high, while those of the South Island were mostly from four to six feet in height. The giant forms, going up twelve and thirteen feet, were seldom seen.

Throughout the Pliocene period the Moas flourished greatly; but in the Pleistocene they must, in the South Island, have died in large numbers, for how else could such immense quantities of bones have got together in the peat-beds at Glenmark and at Hamilton in Central Otago? It has often been suggested that flocks of birds, attempting to escape from fires, rushed into the swamps and perished. But when we remember that these Moas died thousands of years ago, long before there were any human inhabitants to light fires, it will be seen that this surmise is quite out of the question. Only two hypotheses appear to be possible to account for the facts. Either the birds walked into the swamp and were drowned, or else their dead bodies were washed in. The first hypothesis is probably the explanation of the deposit at Te Aute near Napier, because many of the leg bones were found upright in their natural position. But at Glenmark and at Hamilton the bones were lying in all directions, as often upside down as in any other position, and the peat-beds were only a few feet thick, and filled with bones up to the very top. We cannot, therefore, suppose that these Moas were swamped, and there is evidence in both of these cases to show that the dead bodies of birds were washed in by floods. We find corroborative evidence of this in the alluvial plains of Central Otago, for these always contain numerous bones wherever a stream enters them from the hills.

But how are we to account for the number of dead birds washed down from the hills? There are two remarkable facts connected

with these bone-deposits at Hamilton and Glenmark. One is the very large proportion of bones of young birds from one-half to three-quarters grown; and the other is the absence of Moa egg-shell. These two facts seem to show that the birds perished in the autumn or winter, when the birds of the year were not full grown, and when the females did not contain any hardened eggs. Also, it is evident that dead Moas could not be washed into swamps under the present climatic conditions, and the explanation of the puzzle must lie in the fact that in Pleistocene times, when these bone-deposits were formed, the climate was very different from what it is now. At that time the eccentricity of the earth's orbit was very great, and when winter in the Southern Hemisphere happened in aphelion, long cold winters were followed by short and very hot summers. It seems probable, therefore, that the early winter snows killed large numbers of Moas and other birds on the hills, that their bodies were floated down by summer floods and avalanches caused by the melting snow, and that they were deposited in hollows at the foot of the hills. As the Pleistocene period passed away the climate got more equable and the surviving Moas once more increased and multiplied, until they were ultimately exterminated by the hand of man.

All are now agreed that the Moas were exterminated by the ancestors of the Maoris, and the only question upon which opinion is still divided is, How long was this ago? The case seems to me to stand thus. In the North Island there are several names of places in which the word Moa is incorporated, but in the great number of Maori tales and poems which have been collected by Europeans the allusions to the bird are very slight and obscure, generally, indeed, fabulous. There is also one very ancient poem called "The Lament of Ikaherengatu," in which the phrase "Ka ngaro i te ngaro a te moa" (Lost as the Moa is lost) occurs, which certainly shows that the bird was not in existence when the poem was composed. The so-called traditions of its habits appear to be, in large part at least, late deductions from these words and phrases, and we must conclude that, in the North Island, the Moa was exterminated by the Maoris soon after their arrival in New Zealand; that is, not less than 400 or 500 years ago.

In the South Island there are no names of places containing the word Moa; but here remains have been found—either skeletons lying on the surface or bones with skin and ligaments still attached—which give the impression that the birds were living here not more than ten or twelve years ago. Now the bones which are said to have strewn the surface so abundantly when the first settlers came, had all disappeared in fifteen years; so it is plain that either some change in the surrounding conditions caused the bones to decay, or that none of the bones which were so abundant in 1861 were more than fifteen years old. But as we cannot believe that Moas were abundant in Otago in 1846, we must fall back on the opinion that the fires lighted

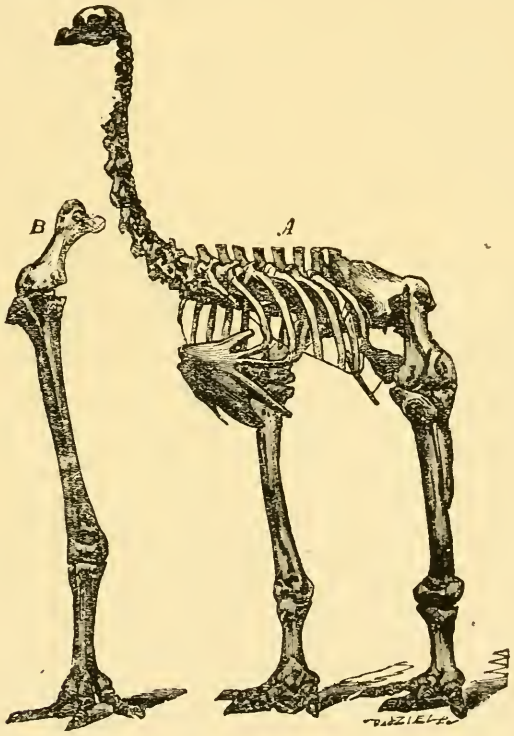
by the early settlers to clear the scrub, so altered the conditions under which the bones had been preserved that they soon decayed, in which case we cannot say how long the bones may have been lying there. It is something the same with those bones which still have dried skin and ligaments attached. They are so fresh that, unless the birds lived a few years ago, they must have been preserved under specially favourable conditions; and there are reasons for thinking that the small district of Central Otago, in which alone these remains have been found, is one specially favourable for preserving animal remains. If this be so, we cannot say for how many years they may have been preserved, perhaps for centuries, and as we have every reason to believe, upon the authority of the Rev. J. W. Stack, that the ancestors of the Ngai Tahu, who have inhabited the South Island for the last 200 or 250 years, never had any personal knowledge of the birds, we must allow that the Moa has been extinct for at least that time. On the other hand, it is quite certain that the Moa was exterminated by the Maoris, and the Maoris are not supposed to have inhabited the South Island for more than 500 years, so that the time of extinction must fall between these dates. It seems improbable that the Ngatimamoe, the last remnant of whom inhabited the West Coast sounds a few years ago, were Moa-hunters. The Moa-hunters of the South Island were not cannibals, and as Te-rapuwai and Waitaha, the tribes who preceded the Ngatimamoe, are said to have been peaceful, and to have "covered the land like ants," it lends support to the Maori tradition that it was they who exterminated the Moa, and made the shell-heaps on the beach. If this be so, the Moas were exterminated in the South Island about 300 or 400 years ago; that is, about a hundred years later than in the North Island.

F. W. Hutton.

II.

Captain Hutton prefaces his new memoir[1] with an excellent *résumé* of the early history of the discovery of their remains. This is followed by a systematic account of the various genera and species; and then we find the interesting discussion as to the origin and extirpation of the Moas already given above.

The author divides the Moas into seven genera, viz.: *Dinornis*, *Palapteryx*, *Anomalopteryx*, *Cela*, *Mesopteryx*, *Syornis*, and *Euryapteryx*;



Skeleton of *Pachyornis elephantopus* (A) and limb of *Dinornis maximus* (B).

the first including the largest, and the fourth the smallest. This classification differs considerably from the arrangement noticed on page 267 of our June issue, although the difference is really less than at first sight appears to be the case. Putting aside the genus *Megalapteryx*, which Captain Hutton does not appear to regard as a Moa at all—although we have no doubt that it is—the forms included by our author under the second, third, fourth, and fifth genera are comprised in the British Museum Catalogue under the head of

[1] The Moas of New Zealand, *Trans. N. Zealand Inst.*, vol. xxiv., pp. 93–172, pls. xv.–xvii. (1892)

Anomalopteryx. *Syornis* represents in part the genus *Emeus* of the "Catalogue"; while *Euryapteryx* corresponds to *Pachyornis* and part of *Emeus* of the latter. The discrepancy in regard to these later genera is due to the circumstance that the skull with a pointed beak referred by Sir R. Owen to the elephant-footed Moa (*Dinornis elephantopus*) belongs, according to Captain Hutton, to the so-called *Dinornis crassus*; while the former species had a broad-billed skull resembling that of *D. gravis*. Assuming this to be correct,[2] *D. crassus* will stand as *Emeus* (*Syornis*); but we have great doubt whether *D. gravis* and *D. elephantopus* of Owen can both be included in *Euryapteryx*, since, according to the restored skeleton sent home by the late Sir J. von Haast, the sternum of the two birds is quite different. We accordingly think that the elephant-footed Moa will probably still have to stand as *Pachyornis*. The following table will show the equivalency of Captain Hutton's scheme, and our provisional amendment of the system followed in the "Catalogue":—

B. M. Catalogue.	*Hutton.*
Dinornis	Dinornis.
Megalapteryx	o
Anomalopteryx	Palapteryx. Anomalopteryx. Cela. Mesornis.
Emeus (crassus)	Syornis (in part).
Euryapteryx (gravis) Pachyornis (elephantopus)	Euryapteryx.

As it will be unnecessary to enter here into the question as to whether it is preferable to use the term *Anomalopteryx* in its narrower or wider sense, we may dismiss the systematic portion of our subject with the observation that we think Captain Hutton has allowed too many so-called species to remain in the typical genus *Dinornis*.

Passing on to the more interesting subject of the origin and extermination of the Moas, the first noteworthy point is Captain Hutton's argument for the derivation of the Moas from flying birds allied to the South American Tinamus, which reached New Zealand during the Eocene period. As we observe from the article printed above, he supports this view by stating that some of the New Zealand Ratitæ are the smallest representatives of that group, and thereby show the nearest affinity to flying birds; while he further adds that there is a difficulty in conceiving how flightless birds could have entered New Zealand without being accompanied by Mammals. We cannot attach very much weight to the former argument, and we cannot but think that when the author admits that the Moas probably passed from New Zealand to Australia during the Pliocene (? Pleistocene) period, without the passage of any mammals in the opposite direction, his arguments as to the impossibility of flightless birds, as such, having reached Australia and New Zealand at an earlier period

[2] We do not at present commit ourselves by saying that it is.

are by no means convincing. Until, then, some evidence of the existence of the presumed Tinamu-like birds in the Eocene of New Zealand be forthcoming, we may be pardoned for continuing to accept Mr. Wallace's view that the Australasian Ratitæ reached their present home when they had already lost the power of flight.

On the other hand, we fully agree with Captain Hutton that, in whatever way they originated, it is evident that when the Moas were once established in New Zealand "the land was a favourable one, for they multiplied enormously and spread from one end to the other." Not only was the number of individuals large, but the multiplicity of genera and species is equally remarkable. When, however, the author further speculates as to the presence of the ancestors of the Moas in New Zealand in the Eocene period, and the occurrence of a different kind on each island of the archipelago then existing, we are loath to accept his views without some evidence from fossil remains. If the genera had been isolated in the way suggested, we should have expected that they would differ from one another more than we find to be the case. Moreover, we have considerable doubts whether several of the genera adopted by our author are really entitled to rank as such.

In regard to Captain Hutton's explanation of the occurrence of an enormous number of Moa-bones found in the peaty swamps of certain districts, we may remark that it appears to us too thorough. A climate which had such disastrous effects on Moa-life would, it seems probable, have finally exterminated the whole breed. Still, however, it may be regarded as certain that the enormous numbers in which Moa-bones occur in some spots must indicate abnormal conditions.

Finally, while admitting the cogency of Captain Hutton's argument for the extirpation of the Moas by the Maories, there may be still some doubt as to his inference from the occasional discovery of remains of the skeleton with skin and ligaments in certain fissures and caves. These remains occur only in a district in Central Otago, where, according to Captain Hutton, climatic conditions are especially favourable for the preservation of animal substances; and in that case it is almost impossible to deduce any conclusions as to their age.

R. Lydekker.

III.

The Fore-runners of the Backboned Animals.

IT has long been an interesting problem to determine the nature of the organisms in which first appeared the foundations of a backbone, which gave a new impetus to the evolution of the animal kingdom. Embryology and Comparative Anatomy have been studied in vain, Palæontology has afforded no certain information, and the rival schools of theorists are still in the midst of a conflict, at present without much sure basis in fact. Whether some form of "worm" gave origin to the fore-runners of the great backboned race, or whether a primæval relative of the arachnid "king-crab" turned upside-down and re-arranged limbs and head—these are questions still admitting of endless discussion, no doubt fruitless in their main object, but desirable from the new lines of research they continually suggest. We must, indeed, find the actual ancestry of the vertebrata if we aim at much further progress in the quest; and this is only possible under the most unusual and accidental circumstances, unless it happens that some members of each great section of that ancestry possessed a calcified (or otherwise hardened) skeleton. Much has been learned during recent years of the few skeletons already discovered in the early Palæozoic rocks, where such fossils ought to occur; and as little of this new evidence seems to have hitherto attracted the attention of those most concerned in the study of animal pedigrees, it may be of service to review some of the principal facts. Palæontology cannot yet solve the main difficulties, but it can, at any rate, establish some suggestive points.

With regard to the earliest traces of undoubted Chordate animals in Palæozoic rocks, the United States Geological Survey, nearly two years ago, roused considerable hopes of new evidence from the Lower Silurian (10); but it now appears (11) that these fossils are merely unsatisfactory fragments, and of no value whatever for our present purpose. Equally unsatisfactory, too, are the conical teeth from the Lower Silurian of St. Petersburg, previously ascribed by Rohon to fishes (5). Few will doubt that these fossils belong to Chordata, but beyond that determination no advance can be made.

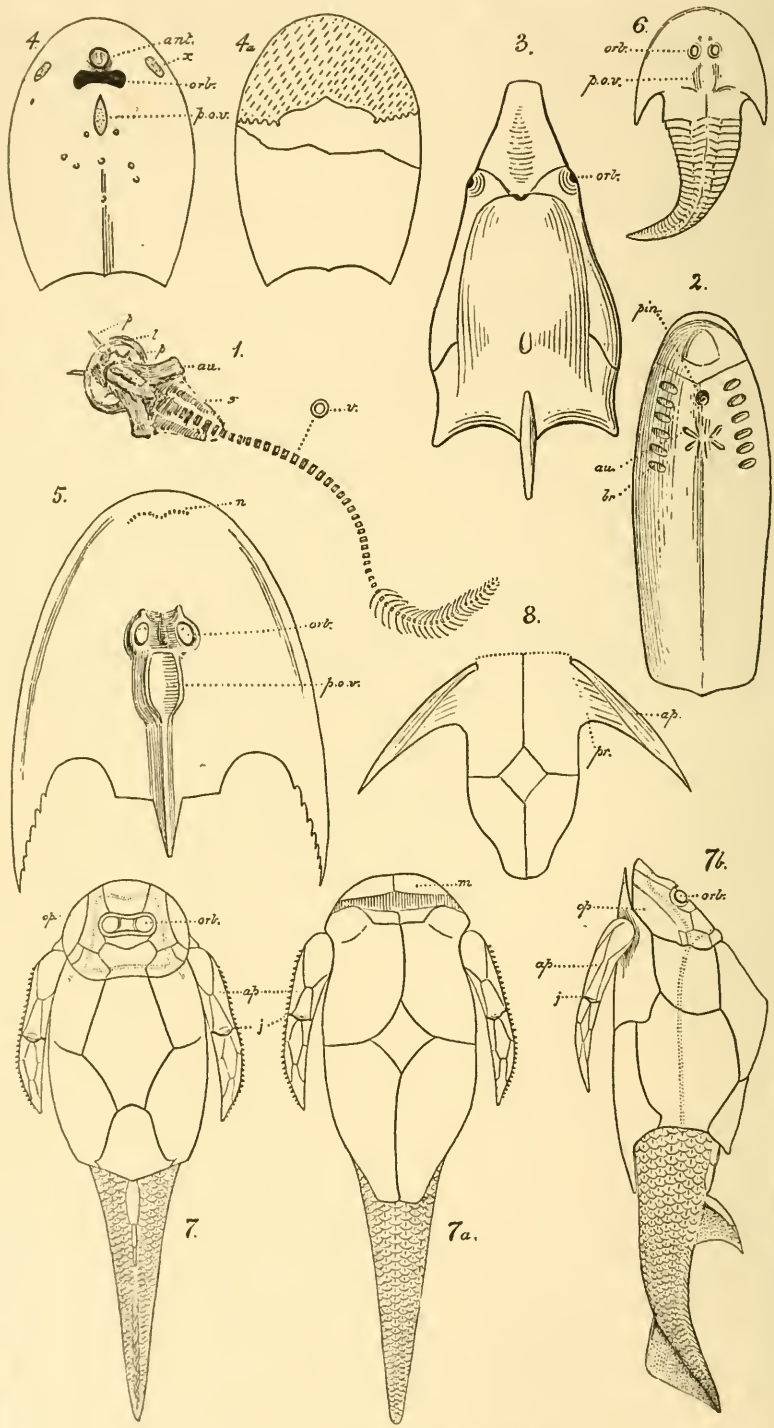
In the case of the remains of Chordate animals from the Upper Silurian and Lower Old Red Sandstone, however, discoveries already made are more satisfactory for consideration; and there are four

principal types of life worthy of special notice. There is a limbless, hard endoskeleton in the Lower Old Red Sandstone; there is a simple shielded organism, with no bony tissue in its armour, in the Upper Silurian and Lower Old Red Sandstone; a similar shielded organism, covered with few plates, but with some bone-tissue in their substance, occurs in the same formations; and a more advanced armoured creature, with a complex of symmetrically arranged bony plates, is met with both in the Upper and Lower Old Red Sandstone, but never earlier.

The limbless skeleton from the Lower Old Red Sandstone of Caithness was briefly noticed two years ago by Dr. R. H. Traquair (9) under the name of *Palæospondylus gunni*; and the writer is indebted to Mr. Donald Calder of Thurso for a small series of specimens from which the accompanying partial restoration (Fig. 1) is made. It belongs to a very small organism, rarely, if ever, exceeding two inches in length. There is a well-formed calcified cartilaginous skull, with a lateral extension on each side at the position of the organ of hearing (*au.*), but with no certain evidence of a pair of nasal capsules; the region of the nose, indeed, is so narrow as to suggest some comparison with the ordinary monorhine lampreys and hag-fishes. In front there is a pair of processes (*p.*), which may be of the same nature as those named prepalatine by W. K. Parker, or may be merely lip-cartilages; a lateral pair of minute points further recalls the lip-cartilages, and a great broad ring (*l.*) crushed upon the anterior part of the skull, bears an extraordinary resemblance to the annular cartilage forming the rim of the mouth in the lamprey. The back of the head is well-defined, and immediately adjoining it posteriorly, there is a hard shield (*s.*) presumably covering part of the back of the animal. The notochord is surrounded by a series of calcified rings (*r.*) developed in its sheath, apparently with short intervening spaces; and towards the end of the tail there are indications of delicate neural and hæmal spines.

Such is the sole information at present available in regard to *Palæospondylus*; and this, it will be observed, merely suffices to place the animal at the base of the backboned series. It *seems* to possess an unpaired nose, lip-cartilages in place of functional jaws, and no paired limbs; thus agreeing precisely with the lampreys and hag-fishes, of which the fossil representatives have long been sought. Theoretically speaking, allies of these limbless Chordata ought to have been a dominant type in the late Silurian and early Old Red Sandstone age; and it is a well-known law that animals, when dominant, attain to a higher degree of development than at any other time in their history. It is extremely probable, therefore, that *Palæospondylus* belongs to this interesting category.

While there is much probability that the skeleton just described can be paralleled among existing animals, there is no possibility of recognising any living congeners of the armour-plated organisms of



PRIMITIVE CHORDATE ANIMALS

which we publish other restored sketches. That the three types, to which reference has been made above, are all closely related and represent three grades in a great extinct group, can scarcely be longer doubted; and it is now customary to arrange them in a division of undetermined rank and value, termed OSTRACODERMI or OSTRACOPHORI.

The simplest of these armoured types are *Pteraspis* and its allies, (Figs. 2, 3, 9), which have the head and abdominal region shielded both above and below, while the tail, in some cases at least, is covered with scales (3, 12). The tissue of the shield is peculiar, each plate consisting of an outer and inner hard layer with a relatively thick middle layer of polygonal chambers, through which the sensory canal-system of the skin forms ramifying delicate tunnels. There are no bone-cells in either layer of the shield, and the external surface is always concentrically striated. The eyes (Fig. 3, *orb.*) are widely separated, being placed laterally; and towards the hinder border of the dorsal shield there is an aperture on each side, evidently to allow the water to escape from the gill-cavity. The nostrils must have been placed close to the mouth on the under surface of the head; but of both mouth and nose nothing is known.

When the dorsal shield of one of these organisms—especially of *Cyathaspis* (1, 3, 12)—is examined from the inner (or visceral) face, it shows a number of symmetrically arranged impressions, which are also of interest from the possible clue they afford to the arrangement of the soft parts (Fig. 2). A pit in front (*pin.*) between the ordinary eyes probably lodged a rudiment of the pineal eye; and a paired series of lateral indentations (*br.*) is suggestive of the original presence of well-separated gill-pouches, between which it is reasonable to infer

EXPLANATION OF PLATE.

Fig. 1.—*Palæospondylus gunni;* skeleton, twice nat. size. Lower Devonian, Caithness. *au.* auditory region of skull. *l.* supposed lip-cartilage of mouth. *p.* processes. *s.* dorsal shield. *v.* ring-vertebra, end-view.

Fig. 2.—*Cyathaspis sturi;* inner view of dorsal shield. Upper Silurian, Galicia. *au.* supposed impressions of auditory organs. *br.* impressions of branchial sacs. *pin.* impression of pineal body. [*After* Alth.]

Fig. 3.—*Pteraspis rostrata;* upper view of dorsal shield. Lower Devonian, Hereford shire. *orb.* orbit. [*After* Lankester.]

Fig. 4.—*Trematapis schrenki;* dorsal and ventral (4*a*) aspects of shield. Upper Silurian, Isle of Oesel. *ant.* antorbital sense-organ. *orb.* orbital opening. *p.o.v.* postorbital vacuity. *x.* vacuity (?) [*After* F. Schmidt.]

Fig. 5.—*Cephalaspis lyelli;* upper view of dorsal shield. Lower Old Red Sandstone, Forfarshire. *n.* undetermined rows of pits (seen only on the inner face of the shield). *orb.* orbit. *p.o.v.* postorbital valley. [*After* Lankester.]

Fig. 6.—*Auchenaspis egertoni;* upper view of head and trunk. Lower Old Red Passage Beds, Ledbury. *orb.* orbit. *p.o.v.* postorbital valley.

Fig. 7.—*Pterichthys milleri;* dorsal, ventral (7*a*), and lateral (7*b*) aspects of head and trunk. Lower Old Red Sandstone, N. Scotland. *ap.* appendages. *j.* joint in appendage. *m.* supposed upper jaw. *op.* operculum. *orb.* orbit. [*Partly modified after* Traquair.]

Fig. 8.—*Acanthaspis decipiens;* ventral aspect of shield. Lower Devonian, Spitzbergen. *ap.* appendage. *pr.* process supporting appendage.

there were supporting elements of the nature of gill-arches. A pair of >shaped impressions (*au.*) between these markings are exactly such as might result from contact with ridges upon the auditory capsules, due to a great development of the upper semicircular canals, as in sharks. In short, it seems most probable that the shield in question covered a truly fish-like head; and if it were not for the invariable absence of limbs and jaws, and the forward position of the breathing apparatus in *Pteraspis* and its allies, these animals might be placed, without hesitation, in the class of fishes. The possibility that limbs and jaws were present, but not calcified enough to be preserved, must, however, be borne in mind; while the negative evidence on this subject, and the want of information as to the nature of the tail, are factors necessitating caution in determining affinities at present.

The next group of Ostracoderms (3, 6, 12) is typified by the genus *Cephalaspis* (Figs. 5, 10), and is remarkable for the manner in which its members mimic in outward shape the contemporaneous aquatic arachnids (*Eurypterus*, &c.) The anterior dorsal shield (Fig. 5) which was doubtless confined to the head and branchial region, consists of a single piece, rounded or tapering in front, abruptly trun-

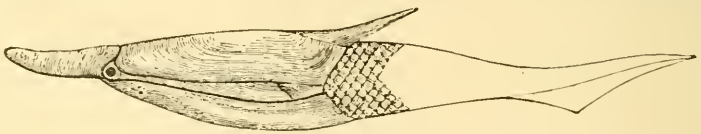


FIG 9.—Restoration of *Pteraspis*. (From Brit. Mus. Catal. Foss. Fishes.)

cated behind, and with the rounded margin reflexed inwards below to form a flattened and ornamented rim. Like that of the Pteraspidians, the substance of the shield consists of three superposed layers; but the inner layer is bony, the thick middle zone is solid though traversed by a coarse network of blood-vessels, and the outer surface is covered with a dense tuberculated tooth-like tissue. The eyes (Fig. 5, *orb.*) are placed close together in the middle of the shield; and there is a large "postorbital valley" (*p.o.v.*) behind them, really a vacuity in the shield filled with a separate plate of peculiar structure (see No. 14, p. 4). There are no apertures for nostrils, which must thus have been situated on the lower side of the head close to the mouth, and of this nothing is as yet known. At the back of the shield, on each side, occurs a small flap stiffened with little plates (Fig. 10) evidently to be regarded as the operculum; and immediately behind the shield the ordinary scaling of the body begins, without any special development for the support of limbs. Paired fins are, in fact, absent; and the Cephalaspidians possess only a system of median fins represented by a dorsal and a caudal, not stiffened by fin-rays but by little elongated scales. The great deep quadrangular body-scales form a series of interlocking rings, no doubt corresponding to the series of muscle-plates of the trunk.

In some genera of Cephalaspidians, as *Auchenaspis* (Fig. 6) and *Didymaspis*, a few of the anterior body-scales fuse together into a continuous plate above and below, and that again is usually fixed to the head-shield; but even in such a case there is no appearance of a shoulder girdle. The advance in this direction is greatest in the genus *Trematapis* (Fig. 4), which exhibits no dividing line between the original head-shield and the coalesced body-scales; and the armour thus named is further of special interest, because the small bone between the eyes (*orb.*) is loose, not fixed, and because the postorbital valley (*p.o.v.*) seems to be without an infilling plate. In *Trematapis*, also, the small antorbital organ (*ant.*) common to all Cephalaspidians, is distinctly perforated, and the view has recently been expressed by Rohon (6) that this corresponds with the special sense-organ—"stirnorgan"—of Selenka (7).

From the highly-specialised *Trematapis*, we may pass to the third great division of the Ostracoderms, of which the familiar "winged-fish" (*Pterichthys*) of Agassiz and Hugh Miller may be taken as type. These organisms have been most carefully investi-

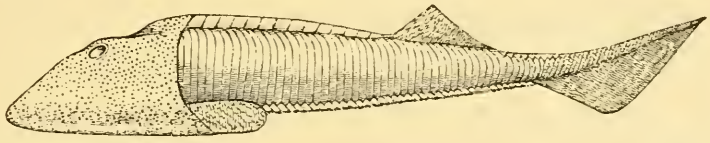


FIG. 10.—Restoration of *Cephalaspis murchisoni*, from specimens discovered by G. H. Piper, Esq. (From Brit. Mus. Catal. Foss. Fishes.)

gated by Dr. R. H. Traquair (8), and the restorations of *Pterichthys*, given in Figs. 7, *a*, *b*, are very slightly modified from his drawings. The shield, it will be observed, is essentially similar to that of those Cephalaspidians in which some body-segments are fused together; but the head is sharply separated from the trunk, and the armour, instead of being simple, is composed of a number of symmetrically arranged overlapping plates. It is also remarkable for the great pair of pectoral appendages (*ap.*), which are hollow, moveably articulated, consist of many plates, and are once jointed (*j.*). The small plate between the eyes (*orb.*) is loose, and marked inside with a pit that seems to have lodged the rudimentary pineal eye; a cheek-plate on each side is moveable (at least in *Pterichthys*), and probably formed the operculum (*op.*); while a pair of loose jaw-plates at the front of the head below (Fig. 7 *a*, *m.*) are finely toothed on their posterior border (at least in *Bothriolepis*). Of the precise nature of the nose, mouth, and jaws, however, nothing is as yet known. Behind the plated armour in *Pterichthys*, the tapering, scaly tail is often preserved, and the median fins are arranged exactly as in *Cephalaspis*.

The question now arises as to the true nature of the pectoral appendages, which form so distinctive a feature of *Pterichthys* and its allies, as compared with the two lower groups of Ostracoderms

already treated. They are commonly regarded as homologous with the paired pectoral fins of fishes, and certainly in their mode of fixation to the trunk there is much to favour that supposition; but the recent discovery (13) of an allied organism, *Acanthaspis*, in the Lower Devonian of Spitzbergen, suggests the wisdom of suspending judgment. The dorsal body-shield of *Acanthaspis* is simpler than that of *Pterichthys;* but the arrangement of the plates on the ventral aspect (Fig. 8) is identical, and there are also hollow pectoral appendages. These appendages, however, are merely simple triangular spines (*ap.*) firmly fixed by suture to processes of the body-armour (*pr.*); and if they are an inferior or special condition of the ordinarily moveable arms, it seems not unlikely that those arms will prove to be different in origin from the paired limbs of fishes.

In conclusion, it may be said that recent researches tend towards demonstrating the antiquity of the race of lampreys and hag-fishes; and evidence is accumulating to show that a great group of limbless, armoured Chordata formed a conspicuous feature in early Palæozoic life. Beyond this statement, however, we cannot yet advance, and much has still to be learned even concerning those few organisms whose main features have already been elucidated.

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IV.

Recent Observations on the Anatomy and Development of the Brachiopoda.

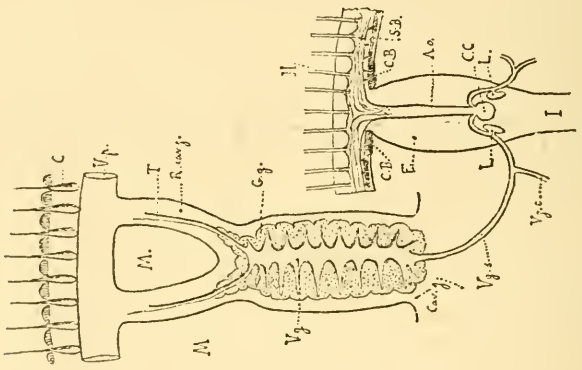
SOME important observations on the circulatory system of the Brachiopoda will be found in an interesting memoir recently presented to the Zoological Society of France (1), in which Dr. L. Joubin, Assistant Professor at the Rennes Faculty of Sciences, describes the anatomy of *Waldheimia venosa*, Solander, from the Straits of Magellan—the largest but anatomically least known species of this genus of the recent Brachiopoda. The specimens investigated were obtained from Mr. P. D. Œhlert, of Laval, and by exchange from the Zoological Department of the British Museum. It may, therefore, be assumed that the species was correctly identified, although neither figure nor description is given. *Waldheimia venosa* is a broad, globose, variable species, with a large circular foramen which mainly distinguishes it from *W. lenticularis*, Deshayes, from New Zealand.[1] To this robust form it is closely allied. In fact, Mr. W. H. Dall (2) considers typical examples of *W. lenticularis*, Deshayes, "as a local race representing for New Zealand the Patagonian *venosa* of Solander," which enjoys, of course, priority of nomenclature. The American authority, it may be added, would also include *W. venosa* (under the generic appellation of *Eudesia*) in the subfamily Terebratulinæ. *Waldheimia venosa* bears some external resemblance to another large brachiopod, the *Laqueus californicus* of Koch, from the North Pacific region, but the "snare-like" loop of this fine species when adult is quite different, and it belongs to the subfamily Terebratulinæ. It is certain that the size of the foramen of the articulated Brachiopoda is not a safe specific character, for it is much affected by the quiet or troubled condition of the waters and the nature of the sea-bottoms they inhabit.

Dr. Joubin had previously given considerable attention to the lyopomatous (3) and arthropomatous (4) brachiopoda while attached to the Arago biological laboratory at Banyuls-sur-Mer. An English

[1] A smaller species, *Wald. kerguelenensis*, Dav., was dredged by the "Challenger" in abundance off Marion Island and W. of Kerguelen Island. It is related to *W. lenticularis*, Desh., which also occurs in a fossil condition in the Upper Tertiaries of New Zealand.

abstract of the most important of his observations on the anatomy of *Crania turbinata*, Poli (= *C. rostrata*, Hoeninghaus) is appended to Davidson's posthumous Monograph of Recent Brachiopoda (5).

The study of the circulatory system of the Brachiopoda, Dr. Joubin remarks in the present memoir, is intimately connected with that of the digestive system. It is nothing more than an absorbent lymphatic system with a propellent heart, which directs the nutrimental fluid derived from the intestine towards the organs of the animal. He passes briefly in review the opinions expressed by Hancock and the older authors who maintained the existence of a regular circulatory system independent of the digestive apparatus, and those of more modern investigators, who absolutely denied the existence of any regular circulatory system. Dr. Joubin held this view so far as *Crania anomala* was concerned, and affirmed that he could not discover any structure in that genus comparable



Figure, partly diagrammatic, showing the dorsal surface of the stomach, the base of the arms, a fragment of the dorsal mantle, and the circulatory apparatus of *Waldheimia venosa*, Sol. (after Joubin). *A.O.*, aorta; *C.C.*, central heart; *S.B.*, brachial sinus; *H.*, cirri; *E.*, stomach; *I.*, intestine; *L.*, lateral hearts; *V.g.s.*, the superior dilated genital blood vessels; *V.g.i.*, inferior dilated genital blood vessels; *G.g.*, genital gland; *M.*, mantle; *Cav. g.*, general cavity; *T.*, terminations of the blood vessels; *V.f.*, peripheral lacune; *C.*, cilia; *C.B.*, brachial canal at the opening into the general cavity.

to the circulatory system as described by Hancock. But it is his intention to make further observations in this connection, as his researches on the better developed species *Waldheimia venosa* have enabled him to recognise the presence of a true vascular system in *Waldheimia*. He asserts that the heart is in direct relation with the small and numberless lacunes which surround the intestine. These channels are filled with lymph and white globular bodies charged with nutriment. They all terminate at the heart, which is a contractile organ, of which he finds Hancock's description to be absolutely correct in detail—the disposition of the muscular fibres alone excepted. The "arteries" described by that author differ both in essential structural characters and the nature of their contents from

true arteries. Dr. Joubin considers them to be lymphatic channels (lacunes) with a specialised function.

The digestive canal of *W. venosa* differs apparently but little from that of other articulated Brachiopoda. The general dimensions are described as larger, and the constituent parts as more clearly defined than in the smaller species. The mouth is formed by a shallowing of the digestive tube, ultimately terminating in an orifice within the brachial groove. The stomach is a large, deep pocket, with thick side walls. He notes the occurrence of a papillary prominence of the digestive epithelium, situated just beneath the vessel efferent from the heart, which may possibly serve as an organ of taste, but does not absolutely affirm that it is one. The so-called "liver" in this species is described as remarkably developed. It is the largest organ and attains relatively an enormous size. There are two hepatic canals in each lobe, the anterior is the larger. The cells present well-marked characters. Some are mucus cells, others granular. He considers that these two kinds of cells secrete distinct digestive ferments and that mucus is secreted by the calciform cells. As the hepatic fluid yields none of the elementary characteristics of bile, as understood among the superior organisms, and contains distinctive products not found at all in the biliary fluid, Dr. Joubin would change the name of this secreting organ into that of "stomacal gland," maintaining that the so-called "liver" of the Brachiopoda presents none of the features characteristic of that structure in other animals.

The organs described as "accessory hearts" by Hancock were not found to be present in any of the specimens of *Waldheimia venosa* Dr. Joubin examined. He considers them as "accidental vesicles" anomalous and inconstant. The occurrence, however, is noted and figures and descriptions are given of structures which he names *supplementary hearts*. These differ completely both in structure and relations from the "accessory hearts" of Hancock. They cannot be otherwise designated, if the name of "heart" be applied to the principal organ, for their structure is identical, and they perform the same function, that of receiving the lymph originating from the intestine. These supplementary hearts are symmetrically situated, the one on the right and the other on the left of the posterior region of the stomach. The organs are in reality oval-shaped blood-channels, larger and more dilated than in general. They are usually filled with white corpuscles. The principal heart receives and the median aorta conveys the lymphatic fluid of all the dorsal region of the stomach, the "supplementary hearts" collect that derived from the inferior and lateral regions, the nutrimental fluid passes through all these organs. The first, or principal heart, nourishes the brachial system; the two latter the genital organs *via* the two main lateral blood-vessels, which alone are truly vascular. These are composed of an external epithelium, a central layer of amorphous tissue, and an internal epithelium most distinctly characterised.

Dr. Joubin gives figures and descriptions establishing the course of the circulation in *Waldheimia venosa*. It becomes apparent that the lymph leaves the central heart and is received by the central aorta and the two lateral vessels, reinforced *en route* by the stores collected in the two supplementary hearts; it courses through all the vascular ramifications of the genital glands, falls into the general body-cavity, and bathes the exterior of all the organs. The function of the central heart is that of propelling the lymph in order that it may bathe the innumerable pores of the digestive system. "It is, therefore, strictly defined, a lymphatic heart intimately connected with the digestive apparatus from which it is a derivation."

Dr. Joubin offers in conclusion some observations of less importance relative to the special histological structure of the voluntary muscular system of *Waldheimia venosa*, which in all other respects resembles that of other species of the genus. The muscles are minutely striated. His memoir is well illustrated with twenty-six figures. The diagram of the course of the circulation in *Waldheimia venosa* (see p. 604) would be more readily understood if the lettering were not reversed. It is a welcome instalment of the author's researches on the general morphology of the recent Brachiopoda.

The philosophical palæo-biologist, or student of "still life," may be said to commence his researches at the point where the embryologist, or student "from the life," ceases his observations. It is only by a combination of the best results of both methods of investigation that we can hope to deduce a rational and enduring systematic classification of the Brachiopoda, and to solve the problem of their evolution. A remarkable uniformity has already been established in the embryology of the ancient Silurian types and their modern descendants in the memoirs of Morse, Brooks, Beecher, and Clarke. Adult conditions of ancient fossil species are shown to represent the youthful and adolescent stages of more modern forms. Interesting observations published at intervals during the last three years on the "Development of the Brachiopoda," by Mr. Charles E. Beecher, of Yale College Museum, and Mr. John M. Clarke, Assistant Palæontologist of the State of New York, mark a new departure in this direction (7). These are based on a careful study of the principles of growth, the acceleration of development, and the effects of mechanical genesis. They record the geological sequence of genera and species, and incidents in the ontogeny or development of the individual and the phylogeny of the race.

In a recent article (9), Mr. Beecher describes the stages of growth and decline, and applies to the Brachiopoda the nomenclature and methods first employed by Professor Alpheus Hyatt in his remarkable memoirs on the classificatory values of these stages for the Mollusca in general and a branch of the Cephalopoda in particular (Genesis of the Arietidæ). The early embryonic stages of the Brachiopoda are thus defined:—1, Protembryo; 2, Mesembryo; 3, Metembryo; 4,

Neoembryo; the true larval stage as Typembryo, with the addition of Dr. R. T. Jackson's term Phylembryo for the later phases of the last-named stage, when the animal, for the first time, can be referred definitely to the class to which it belongs.

It is at the close of the phylembryonic stage that the palæontologist *per se* should commence his investigations, for the initial shell, the protegulum, is then developed. There is a remarkable similarity in its appearance in all forms. It is noteworthy that "representatives of such distinct groups as *Rhynchonella*, *Spirifer*, *Athyris*, *Nucleospira*, and the Meristoids resemble each other so closely at this stage that the genus can be then determined only by comparatively trivial features" (7). Nearly twenty years ago, Morse pointed out the Linguloid form of the shell assumed in "the early stages of *Terebratulina*." It now appears that the adolescent *Shizocrania* resembles the youthful age of *Orbiculoidea*, and that the young *Orbiculoidea* recalls the genus *Paterina* in the "paterina stage" of its development. The protegulum has been observed by Mr. Beecher in forty genera of the leading members of the class, recent and fossil (8). It is described as always corneous and imperforate. The genera with pedicles sufficiently long to admit of free axial movement have elongated and rostrate shells (8). Breadth of form is the correlative of a short pedicle, while the discinoid resulted from mechanical conditions of growth.

The brachiopod next passes through its youthful or "nepionic" stage, when the shell is smooth, or with few plications, into the adolescent or "nealogic" period, and attains, at maturity, the "ephebolic" condition—the maximum of individual or ontogenetic development. Varieties and abnormalities usually begin to express themselves early in the development of the shell, and we learn that "such divergence from the normal form rapidly increases as maturity approaches (7), and they are gradually diverted from the harmony characterising the incipient stages of other members of the group." Soon after becoming adult, they commence to age, and pass through first a "clinologic" stage of arrested growth, and, subsequently, the "nostologic" phase of absolute senility.

These latter stages are well represented in the well-known, widely-distributed lobate Silurian orthoid *O. biloba*, Linné, by the disappearance of the bilobed condition in old age, as Mr. Beecher has fully described in his paper on the "Development of Bilobites" (10). In the degenerate form from the Lower Helderberg, designated by Conrad as *Orthis biloba* var. *varicus*, the features of extreme lobation, so characteristic of the adolescent and mature conditions of the *Orthis biloba*, are ultimately re-absorbed, and this variety differs at last but little in shape from the non-lobate orthids. The Gothland form known as *O. biloba* var. *verneuilianus*, Lm., presents a tendency to vary in the opposite direction. The lobation becomes more and more pronounced with increasing age, and the shell exceeds in size the normal species *Orthis biloba*, which is the directly linear or epacmic form, and enjoys

the widest distribution. The varieties *verneuilianus*, Lm., and *acutilobatus* (Ringueberg) are acmic or indirect divergents originating from the "nealogic" period of *O. biloba*. The variety *varicus* is the senile, a paracmic condition derived from the "ephebolic" or mature state of that species. In fact, the ontogeny of this very variable species, which begins life and ends it as a normal orthid, indicates its position as an aberrant species of that genus, and as its abnormality—the bilobed condition—was clearly defined in the specific name, we scarcely see the necessity for removing it from the genus *Orthis*, as defined by Dalman and established by Davidson, who declined to recognise the bilobate species and its varieties otherwise than as a genuine species of the genus *Orthis*. There seems even less necessity to disguise so well-known and much-studied a brachiopod as the living *Thecidium mediterraneum*, Risso, under the sub-generic designation of *Lacazella*. If biologists and palæontologists continue to indulge their Adamite denominative propensities in this direction, the student of the future will lose much time in re-establishing the identity of text-book genera and species.

Important conclusions are derived from comparisons of the geratologous phases of brachiopodal development when ornamental and other indications of maturity disappear in inverse order to their introduction. Age is expressed in the thickening of the shell, which takes place after the individual has reached adult size, and produces a varicose aspect, or truncated margins, and plications gradually vanish. There is an increase in the vertical diameter, the beak becomes involuted, and the deltidium, or deltidial plates, may be re-absorbed. The revolutions of the spirals become thicker and more numerous in the spire bearers. In the senile condition, or final nostologic phase of life, brachiopods, like human beings, shed their ornamental appendages and revert in extreme old age to the characteristics of their "nepionic" or youthful period. Such incidents (like premature baldness) may even be accelerated and transmitted. If we comprehend Mr. Beecher's argument correctly, the genera and species developed by the processes of "accelerated geratologous heredity," such as *Strophalosia* and *Austoleges* among the Productidæ, generally occur in families after the maximum of genera and species is attained. He cites *Gwynia*, *Platydia*, *Cistella* and *Kraussina* among the recent Terebratuloids and *Atretia* among the Rhynchonellidæ as examples of such nostologic types (9). This view clears up many difficulties. The relative affinities of *Gwynia* and *Platydia* were left undetermined in Davidson's "Monograph of the Recent Brachiopoda" (12). By some authors *Gwynia* and *Atretia* are regarded as immature forms of other genera. But the swollen appearance of the margins of the shell is very apparent in the Australian species (*Atretia brazieri*, Dav.) (6), and indicates maturity. Mr. Friele deduced a similar conclusion as to its generic value from distributional data, as it was dredged in northern waters in great abundance where no specimens of *Rhynchonella* were ever obtained.

The wide distribution in the North and South Atlantic and the South Pacific Oceans of this geratologous type renders it additionally interesting. It is evident that it is not always an easy matter to distinguish between the youthful and senile brachiopod, for the Brachiopodist meets with the same difficulty as the philologist treating of the Chinese language, namely, that of determining whether it is in its first or second childhood.

While fully admitting certain resemblances between the typembryo of the brachiopod and the larval stages of *Spirorbis*, Mr. Beecher refers to the existence of several important differences. He shares the opinion expressed by Joubin, Œhlert, Balfour, and other recent investigators, that the Brachiopoda constitute a distinct and independent class. There seems to be a general reaction in favour of this view, consistently upheld throughout by Dall and Davidson. King was the first to express the opinion that the articulated Clistenterata were the degenerated descendants of their aniferous predecessors, the Tretenterata. But it has always been difficult to account for the presence of an anal opening in the hingeless forms, and its absence in the articulated genera. In this connection the facts are justly emphasised by Mr. Beecher, that the dorsal beaks of *Amphigenia*, *Athyris*, *Atrypa*, and *Rhynchonella* are usually notched, or perforate from the union of the crural plates above the floor of the beak, leaving a passage through to the apex, and that a similar opening occurs between the cardinal processes in *Strophomena*, *Stropheodonta*, and allied genera with a furrowed deltidium, as in *Leptæna*. "This character, which cannot be considered as in any way connected with the pedicle opening (9), points to the existence in the early articulate genera of an anal opening *dorsal to the median line*," a character which Dr. Joubin first demonstrated is peculiar to *Crania* (3, 5) among the recent species. This, the "dorsal foramen" of King and Hall, is also known as a "visceral foramen." Mr. Œhlert has suggested that it was probably occupied by the terminal portion of the intestine.

Mr. Beecher considers the group hitherto known as *Arthropomata* (Owen) or *Clistenterata* (King) as retrogressive so far as the loss of eyes and the closure of the intestine are concerned; progressive mainly through expansion of anterior elements and limitation of pedicle opening to one valve.

Davidson (11), in his masterly "Summary to the British Fossil Brachiopoda," published in 1884, classified the Brachiopoda in general into fourteen named families and two unnamed ones, containing, respectively, obscure genera with uncertain affinities. In 1891, Beecher legitimately proposed a new classification, based in a great measure on the nature of the pedicle opening, its limitations and associated modifications (8). He divided the class into four orders. The first and second correspond with the *Lyopomata* of Owen = *Tretenterata*, King. The third and fourth with the *Arthropomata* of Owen = *Clistenterata*, King.

1.—The ATREMATA, in which the pedicle is free, embraces the genera included in Davidson's family groups of the Lingulidæ and Obolidæ and the Silurian Trimerellidæ.

2.—The NEOTREMATA, which comprises the majority of genera in the Discinidæ, Craniidæ, and Siphonotretidæ as established by Davidson.

3.—The PROTREMATA includes the Orthidæ, Strophomenidæ, Porambonitidæ, Productidæ, and Thecidiidæ.

4.—The TELEOTREMATA contains the Rhynchonellidæ, Spiriferidæ, and Terebratulidæ of that great authority.

This scheme possesses the merits of natural grouping from a phylogenetic standpoint. Waagen and Beecher concur in regarding *Orthis* as the lowest and simplest form of the articulated genera. Mr. Beecher believes *Kutorgina* to be related to *Orthisina* and *Strophomena*, of which it is the ancestral type. This is the main point of difference between Davidson and Beecher, so far as the relations of genera and families are concerned, as the former never included *Thecidium* with the Terebratulidæ. Mr. Beecher considers this genus as descended from ancestors with a habit of fixation, so far back as the Silurian, and that its affinities are with the Strophomenoids, which have also a single plate deltidium. *Thecidium* bears, in his opinion, "the same relation to other Brachiopoda that Ostrea bears to Avicula among the Pelycypoda." It may be added that M. Eugène E. Deslongchamps (15) regarded the division between the Rhynchonellidæ and Spiriferidæ as much less absolute than at first apparent. He cites the recent spinose *Rhynchonella* from Japanese waters (*R. Döderleini*, Dav. MS.) as bearing less resemblance to the Acanthothyrid, or spiny Rhynchonellæ of the Jurassic seas, than to the genera *Spirigera* and *Atrypa* of the Palæozoic oceans. The only known examples of this remarkable "survival" are preserved in the Strassburg Museum, and "a good haul" of this species is much to be desired in the interests of science (13).

Davidson and King showed very clearly the relations of the Trimerellidæ with the Lingulidæ, while admitting that from the rudimentary characters of the hinge and other external features, they might be regarded as related to the Clistenterata (14). They appear, in fact, to be somewhat intermediate in structure between the two orders. Beecher and Clarke consider it not improbable that phylogenetic development tended in two main channels, one leading through *Strophomena*, *Orthisina*, *Leptæna*, *Chonetes*, *Productus*, &c., and the other in the direction of *Rhynchonella*, *Spirifer*, *Atrypa*, *Retzia*, and *Terebratula* (7).

The promised continuation of Mr. Beecher's researches on this subject will be awaited with interest. The results already published should induce "species-mongers" to pause in the manufacture of specific designations for ill-defined species, often representing different

ages of growth, or local variations, resulting from changed environment. But we are not sanguine.

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AGNES CRANE.

V.

Botanical Nomenclature.

FOR some considerable period, questions connected with botanical nomenclature have been agitated with an energy which may have appeared to some entirely disproportionate to the importance of the subject. The Laws of Botanical Nomenclature, drawn up by M. Alphonse De Candolle, and adopted by the International Botanical Congress held in Paris in 1867, were, as might have been expected, subjected to criticisms from various quarters. These criticisms have been carefully considered by M. De Candolle, and the more important of them were dealt with in his pamphlet, "Nouvelles remarques sur la Nomenclature Botanique," published in 1883, and embodied in a new edition of the Laws appended thereto.

The law of priority, as it has been called, is accepted as a general principle by all botanists, and, of course, by all zoologists also; my remarks throughout this paper are entirely confined to the botanical side of the question. But when the law comes to be applied, differences of opinion are at once manifest. Is a generic name which has become familiar by frequent or even by general use, to be superseded by an earlier and disused synonym? And, in the case of a species, is the oldest published specific name, no matter in what genus, to be thereafter inseparable from the plant? or is the name by which the plant was first placed under the genus in which it is now retained to be regarded as its proper appellation? It would seem, at a cursory glance, that both of these questions might easily be settled, and that there should be no difficulty in arriving at a conclusion which would commend itself on the score of logic, common-sense, and convenience. But this conclusion is, unfortunately, not justified by events.

It would be impossible in a paper like this to deal with the numerous points which have arisen in connection with botanical nomenclature. A treatise of considerable bulk would be needed to do justice to the subject. I will therefore only, as briefly as possible, deal with the questions raised above, pointing out in doing so the various considerations which have brought about the existing state of chaos, and giving, in conclusion, the lines which have been laid down to conduct us back to a sane mode of procedure.

I.—Generic Names.

During the last century and the early part of the present, when workers were few and isolated and means of publication and communication restricted, it was inevitable that a considerable crop of synonyms should arise. Later, certain botanists seem to have assumed an arbitrary power, by virtue of which they superseded names of less popular men, on grounds which could only have been tolerated from their eminence of position, or from a failure to recognise the rights of priority. Thus Robert Brown substituted *Xerotes* (1810) for Labillardière's *Lomandra* (1804) without assigning any reason for the change; and superseded R. A. Salisbury's *Chlamysporum* (1809) by his own *Thysanotus* (1810) for reasons which would not satisfy modern requirements. Salisbury, indeed, as I have shown at some length elsewhere,[1] was the victim of something very like a conspiracy to ignore, as far as possible, his work and his names; and it is certain that many of the latter must be restored, and take precedence of Smith's and Brown's. Whether this will be the case with his Proteaceous genera, published under peculiar, and I fear not over-creditable, circumstances,[2] may possibly be open to question; but that *Hookera* of Salisbury (1808) must replace *Brodiæa* of Smith (1820) is absolutely certain. If the well-known moss-genus *Hookeria* be considered as approximating too closely in name to *Hookera*, that also must disappear; for Salisbury published *Hookera* in March, 1808, while the paper in which *Hookeria* was established was not even read until a month later.

Smith's division of the Linnean *Nymphæa* (in 1808–9) into two genera, for one of which he retained the Linnean name, proposing *Nuphar* for the other, was anticipated by Salisbury in 1805–6, who called the white-flowered species *Castalia*, leaving *Nymphæa* for the yellow-flowered plants. But Smith, supported by Goodenough, deliberately set aside Salisbury's names "upon irrefragable [*i.e.*, classical] grounds," [3] and, with very few exceptions—*e.g.*, Rees's Cyclopædia—his names were accepted by all subsequent botanists until Professor E. L. Greene called attention to the necessary revival of the earlier titles. The tardy restitution of Salisbury's names has caused some trouble and confusion, but for this Smith and his followers must be held responsible. Salisbury was undoubtedly, and perhaps deservedly, unpopular; but this cannot justify the wholesale boycotting of his names.

A similar course of action has been adopted with regard to Rafinesque, whose eccentricities have been allowed to obscure his undoubted genius. Our American friends, to whose methods of procedure I shall have to refer later, have devoted much energy and research to the unearthing and rehabilitation of Rafinesque's names,

[1] *Journal of Botany*, 1886, pp. 49, 296. [2] *Loc. cit.*, p. 297.
[3] *Loc. cit.*, 1888, p. 7.

and, although I cannot follow them entirely, it is certain that many of his names must be restored. The genus *Echinocystis*, founded by Torrey and Gray in 1840, upon *Momordica echinata*, Muhl., was anticipated by *Micrampelis*, published by Rafinesque in 1808, with a diagnosis and an indication that it was based on the same species; and many other examples might easily be cited.

It cannot be too much regretted that the great opportunity for establishing an enumeration of genera which would have complied with the requirements of the law of priority should have been neglected. The publication of Bentham and Hooker's *Genera Plantarum* afforded this opportunity, but the learned authors, although in many instances aware that there was an earlier name than that which they adopted, deliberately placed this as a synonym. Had the contrary course been adopted, it is hardly too much to say that, so far as genera are concerned, our nomenclature would, by this time, have been established upon a solid basis. Instead of this, however, the illustrious sanction thus given to the retention of names admittedly not the oldest, and the adoption of a similar limitation and naming of genera by so many subsequent authors, has placed these names in a position of false authority. It is true that, more and more, these false names are being set aside by individuals, but in the *Genera Plantarum* the whole work might have been done once for all. The various portions of Colonial Floras which issue from Kew have, for the most part, followed the *Genera* in nomenclature; and, worst of all, Mr. B. D. Jackson's great *Index*, which is making satisfactory progress, follows the same lines, although the dating of each name makes it plain which should be retained. It is scarcely an exaggeration to say that many thousands of names which stand in the *Index* will have no existence, save as synonyms, so soon as any monographer takes the trouble to restore the right names of the genera; and for this unnecessary distention of our already overcrowded list the authors of the *Genera* must be held responsible.

It is right to say that this ignoring of the oldest names was justified by Mr. Bentham, and has lately found a defender in Mr. W. B. Hemsley, of the Kew Herbarium. According to the former, "names which have been long and universally adopted must be considered as having acquired a right of prescription to overrule the strict laws of priority. It would, indeed, be mere pedantry, highly inconvenient to botanists, and so far detrimental to science, now to substitute . . . *Fibichia* for *Cynodon* . . . or *Singlingia* [*Sieglingia*] for *Triodia*."[4] It is satisfactory to know that, in spite of this *ex cathedrâ* pronouncement, and of the weight deservedly attaching to Mr. Bentham's utterances, these two names, as I have pointed out elsewhere,[5] have been adopted in the last two issues of the *London Catalogue*, and *Sieglingia* appears in two English local floras.

[4] "Notes on Gramineæ," *Journ. Linn. Soc.*, vol. xix., p. 19, 1882.
[5] *Journal of Botany*, 1892, p. 54.

Mr. Hemsley[6] defends the action of Bentham and Hooker on the ground that as "the Kew botanists, for reasons of their own, took no part in the proceedings of the Congress" at Paris, they were "not bound down by the law of priority," and "were not always consistent, even from the point of view of expediency and convenience." He speaks as though the law of priority was first formulated at the Paris conference, and was, therefore, only binding on those who took part in it! I have reviewed Mr. Hemsley's defence elsewhere,[7] and need not dwell upon it at length; it is an elaborate plea for "convenience" as a principle, and, to my mind, carries with it its own condemnation. "Convenience" and "expediency" are, at best, but temporary, and no satisfactory or lasting structure can be raised on so shifty a foundation.

So far as we can understand, the plea is largely based on the inconvenience which the restoration of the right names would inflict upon—gardeners! The Kew botanists, Mr. Hemsley tells us, are "almost exclusively responsible" for "the botanical nomenclature current in gardens," and this must not be disturbed by scientific considerations. Yet Bentham and Hooker did not hesitate to relegate the plants known in gardens as *Gloxinia* to the genus *Sinningia*; and if this can be done in one case, it is not clear why it is impossible or objectionable in others. If Mr. Hemsley distinguishes between changes made on scientific and those made on literary grounds, he will hardly deny that the (temporary) resulting inconvenience is equal in both cases.

An example of what has taken place will make my meaning clear. In the *Genera Plantarum* the genus *Æschynanthus* of Jack is maintained, the earlier name *Trichosporum* of D. Don being placed as a synonym. Mr. C. B. Clarke[8] connives at this suppression, and even justifies it, in the following words: "*Æschynanthus*, Jack, was published in January, 1823; but *Trichosporum*, D. Don, being published July, 1822, has the right of priority; *Æschynanthus*, however, having been accepted for half a century, it *would not be expedient* to relinquish it." He proceeds to describe twenty-two species, eight of which are published for the first time. Later, Mr. Clarke monographed the whole genus,[9] still retaining the later name, and added thirty new species. Had the earlier name been taken up in the *Genera*, more than half the genus would have been described as species of *Trichosporum*; as it is, an already over-burdened nomenclature may be encumbered with a set of useless synonyms, so soon as a Dr. Taubert (*see* p. 619) shall come forward and substitute *Trichosporum* for *Æschynanthus* throughout the genus. And this is justified on the ground of "convenience" and "expediency"!

A brilliant opportunity for wholesale changes is afforded by the

[6] *Nature*, Dec. 24, 1891, p. 169.
[7] *Journal of Botany*, 1892, p. 53.
[8] *Fl. Brit. Ind.*, vol. iv., p. 337.
[9] *DC Mon. Phan.*, vol. v.

enunciation of the formula "once a synonym always a synonym."[10] This was promulgated by Dr. N. L. Britton in April, 1891,[11] and was hailed with enthusiasm by his fellow-reformer, Professor E. L. Greene, although it wrings from him the melancholy confession that after the many years during which they have laboured at correcting the errors of their predecessors, not even the beginning of a settlement has been attained. "Change and confusion," says Professor Greene, "are sure to be the order of the day so long as revertible names are not everywhere ignored." A name once employed and dropped, must never, under any circumstances, be used again. "The sooner systematic botanists agree upon the principle 'once a synonym always a synonym,' the earlier will botanical nomenclature *begin to be found* a settled thing." Professor Greene then proceeds, with his usual promptitude, to put his principles into practice, and at once gets rid of several well-known names, *e.g.*, *Torreya*, Arnott (1830), for which *Tumion*, Rafinesque (1840), is to be substituted, *Torreya* having been previously employed and rejected for other genera. This also gives him an opportunity for inventing entirely new names; *Osmaronia* is to supplant the well-known *Nuttallia* of Torrey and Gray, *Chrysamphora* supersedes *Darlingtonia*, DC., and so on. This new device for increasing an already redundant nomenclature is to apply to species as well as to genera, and if Dr. Britton, Professor Greene, and their friends are let alone, they will probably succeed in renaming the American flora three or four times over before botanical nomenclature becomes "a settled thing."

The priority of a name, according to Dr. Britton, is established in some cases by its position on a page! Thus, Adanson proposed the names *Tissa* and *Buda* for the group of plants since called *Spergularia* and *Lepigonum*. *Cæteris paribus*, it would have appeared natural, should either be revived, to adopt that which stood first on the page; but Dumortier, who first (in 1827) called attention to Adanson's names, preferred to adopt *Buda*, the second in position. Dr. Britton, however, considered that *Tissa* had a prior claim, inasmuch as it preceded *Buda* on the page, and he did not scruple to substitute *Tissa* where *Buda* had been employed.[12] His discovery of a yet earlier name, *Corion*, has not only set this question at rest, but has given Mr. N. E. Brown an opportunity of inventing another set of synonyms, to each of which his own name is attached.[13] "I accepted *Tissa* rather than *Buda*," says Dr. Britton, "for the simple reason that it stands first on the page in Adanson's *Familles*. That is priority, I am sure." Priority of *place*, yes; but will anyone seriously contend that one part of a page is *published* before another? If so, there is a fine

[10] *Garden and Forest*, vol. iv., p. 202. [11] *Pittonia*, vol. ii., p. 187.

[12] See *Journal of Botany*, 1890, pp. 157, 295.

[13] *Supplement to English Botany*, ed. iii., p. 48.

field open for much more extensive changes—*e.g.*, the botanists who, with Bentham and Hooker, unite *Amygdalus* and *Prunus* must adopt the former name for the aggregate, inasmuch as *Amygdalus* precedes *Prunus* in Linné's Genera!

II.—Specific Names.

Notwithstanding the examples just given, however, it is comparatively easy to arrive at a common basis of action with regard to the names of genera: those of species present far greater difficulties. To attempt anything like a complete summary of the points which have been raised would occupy considerable space, and although it would incidentally pay a gratifying tribute to human ingenuity, its practical utility would be slight. M. Alphonse De Candolle has dealt with the matter at length,[14] and I propose here only to refer to some of the views which have been promulgated since the publication of his essay. These have mostly proceeded from the new school of American botanists—men whose ability cannot be called in question, although their zeal in matters connected with nomenclature seems to me to outrun their discretion. We are all agreed as to the desirability of establishing a permanent specific appellation, and in condemning the practice, which at one time was not infrequent,[15] of superseding a name by one considered more appropriate. But how the permanent name is to be secured is a matter on which there is much difference of opinion; and the reformers—as is not uncommon with reformers—are by no means at unity among themselves. Another debated matter on which there is considerable difference of opinion concerns the authority to be cited for the name of a plant.

The main point at issue is not a new one, although it has gained new life from its recent presentment. Is the specific name first bestowed upon a plant to be maintained (with certain necessary exceptions) ever after, no matter to what genus the plant may be transferred? If so, who is to stand as the authority for the new combination? The latter point was definitely settled by the Decandollean Laws,[16] but in 1888 a sub-committee of the younger American botanists, headed by Dr. N. L. Britton,[17] formulated the desirability

[14] *Nouvelles Remarques sur la Nomenclature Botanique*, 1883.

[15] R. A. Salisbury and S. F. Gray are familiar examples. Nuttall is another instance. The plant usually known as *Impatiens fulva*, Nutt., had two earlier names—*I. biflora*, Walter (1788), and *I. maculata*, Muhlenberg (1818). Nuttall says: "As several species are spotted I have not adopted the last name, and changed the former because it was deceptive." (Gen. N. Amer. Plants, vol. i., p. 146).

[16] "For the indication of the name or names of any group to be accurate and complete, it is necessary to quote the author who first published the name, or combination of names, in question."—*Art.* 48.

[17] *A Preliminary Catalogue of Anthophyta*, etc. See *Journ. Bot.*, 1888, p. 257.

of following the practice of zoologists in maintaining the earliest specific name,[18] and proposed to cite in parenthesis the name of its original inventor, followed by that of the author who placed it in its present position. Thus, to take an example, the plant first placed by Pursh in its present genus *Pycnanthemum*, and generally known as *P. linifolium*, Pursh, was called by Linné *Thymus virginicum*. The sub-committee restored Linné's name and substituted for Pursh's name, *P. virginicum* (L.) B.S.P.[19] This, they were convinced, would "in the not distant future be generally accepted as the only law that promises a reasonable fixity of botanical names"; and Dr. Britton further commends it as "just, rational, and stable."[20]

The best comment upon the "stability" of this new and cumbrous mode of citation is that supplied by its chief promoter. In 1891 Dr. Britton threw in his lot entirely with the zoologists, and "would quote [only] the original author of the specific name, leaving the author of the binomial to be brought out in the synonymy of the species. "This," he adds, "has the advantage of doing away with the double citation"[21]—the "just, rational, and *stable*" method had endured for less than three years!

Not only is the specific name in ordinary cases to be held sacred, but it must remain even when it has been subsequently adopted for a genus separated from the type. Thus *Cochlearia coronopus*, Linn., was taken as the type of a new genus, *Coronopus;* and Gaertner, who established it, called the species *Coronopus ruellii*. This plant, according to Dr. Britton,[22] must be called *Coronopus coronopus* (L.); in 1888 he would have styled it "*Coronopus coronopus* (L.), Britton;" botanists generally would cite it (not without a feeling of irritation at the needless multiplication of synonyms) as "*Coronopus coronopus*, Britton." Here we find a very pretty difference of opinion among the leaders of the new school; for Professor E. L. Greene, who shares with Dr. Britton the glory of initiating the neo-American movement, sees in this duplication of names "a natural unfitness which all our sense of what is proper in nomenclature shrinks from."[23] Whereupon Mr. Conway Macmillan takes a hand in the game, and thinks Dr. Britton's suggestion "so excellent that it will scarcely fail of universal adoption after a season of recalcitrant objection";[24] and Mr. T. D. A. Cockerell, in adding another list of such combinations (in which, by way of contributing a little novelty on his own account, he adopts a small initial for the second name), goes one better by

[18] I am aware that the Laws prescribe the retention of the specific name when a plant is transferred to another genus. But I do not understand this to be retrospective. There has been considerable divergency of practice on this point, even among those accepting the Laws.

[19] *Journ. Bot.*, 1888, p. 295.

[20] *Loc. cit.*

[21] *Garden and Forest*, vol. iv., p. 202.

[22] *Bull. Torrey Bot. Club*, 1891, p. 268.

[23] *Pittonia*, vol. ii., p. 214.

[24] *Bull. Torrey Club*, 1892, p. 15.

pronouncing that "if the identical name for both genus and species is to be avoided, it is the genus that must go."[25] We may be thankful that this gentleman did not proceed forthwith to formulate a new series of generic names to meet the possibility thus indicated, as Professor Greene has done in his paper already referred to[26] (see p. 616).

The position now claimed for the specific name has been fully discussed in an admirable note by Professor L. H. Bailey—one of the ablest of the American botanists—published in the *Botanical Gazette* for 1891, p. 215. He applies the term "monomialism" to the new scheme, and says: "They are telling us that two words do not constitute the name of the plant, but that *the name, per se,* is the second word of the two. In other words, *Saccharinum* is the name of the sugar maple, and *Repens* is the name of the white clover. This is the monomial system of nomenclature, and its devotees are delving through every author in the hope of finding *the name* of the plant. When this name is found—or supposed to be found, which amounts to the same thing—it is attached to some generic name to which it was never designed to fit, and the twain, to which an algebraic formula has been attached, is given to the world as the monomio-binomial name of the plant." I cannot quote at greater length Professor Bailey's vigorous and well-timed protest, but his concluding sentence must be cited. "One of the most mischievous features of the whole thing is the ease with which authors of local floras obtain a cheap notoriety by making new combinations—which will likely be changed by the next cataloguer—and the extent to which it fosters the notion that making a new name and differing from an authority are the chief ends of systematic botany."

With regard to the citation of authorities, it seems to me that the name of that author is to be adopted who has first placed a plant in the genus to which it is recognised as belonging. This view is in accordance with the practice prevailing at Kew. There is, however, a manifest want of right feeling in the application of the rule in cases where a man has done nothing but transfer a plant from one genus to another, adding his own name to the new combination. In the *Botanisches Centralblatt* for 1891 (xlvii., pp. 385–395), Dr. Taubert restores a number of Aublet's names, which take precedence of those by which the genera have usually been known—among them *Swartzia*, *Derris*, and *Macrolobium*, which give place to *Tounatea*, *Deguelia*, and *Vouapa* respectively. He proceeds to enumerate the species, to which, in almost every case, he attaches his own name. Unless some justification can be found for disregarding them, it would seem that these names must stand; but Dr. Taubert's action is contrary to general practice, as well as to good taste. In cases where the restitution of an old generic name has been established, it is customary to leave the

[25] *Id.*, p. 95.

[26] *Pittonia*, vol. ii., pp. 185–195.

specific combinations as a whole to be dealt with by a monographer of the genus; and this has the further advantage of preventing an undue increase of synonyms, as the revision of a genus usually results in the sinking of a certain number of species, which thus do not require renaming. There is no evidence that Dr. Taubert has studied any one of the genera for the species of which he claims to stand as authority. The notion at one time prevailing in some quarters, that a certain amount of "credit" attached to the citation of the name of an author, is entirely set aside by such action as this.

One word must be said as to the citation of early (and purely imaginary) authorities for names, adopted by Professor Greene, whose enthusiasm for antiquity has led him into regions far more remote than his fellow labourer has ventured to tread. "*Lupinus*, Catullus"; "*Euphorbia*, Pliny"; *Amygdalus*, Theophrastus"; "*Linum*, Vergil"; are only samples of his charming restorations.[27] The accidental binomials employed before the binomial system was invented are also to be accredited to their original authors, so we must write "*Ranunculus flammula*, Dodoens"; "*Bromus sterilis*, Gerarde" (why does so scrupulous a stickler for accuracy give our Elizabethan botanist a final "e"?); "*Artemisia vulgaris*, Cæsalpinus," and the like.[28] Dr. Britton, however, with whom I am glad to find myself for once in accord, refuses to countenance Professor Greene's action, which appears to him "to be straining a point for history's sake."[29]

III.—A Proposal.

The rapid progress which botanical nomenclature was thus making towards a state of chaos hardly less formidable than that from which we were freed by Linnæus in 1737–53, was brought to a climax last year by the publication of Dr. Otto Kuntze's ponderous *Revisio Generum Plantarum Vascularium*. The individual pranks of American botanists might have been left to neutralise each other; but a work of 1,011 pages, preceded by an introduction of 155, and containing a specific renaming of more than 30,000 plants, called for prompt and decisive measures. Dr. Kuntze's book (which space will not allow me to notice fully) has been carefully analysed by Mr. B. D. Jackson in the *Journal of Botany*,[30] and has been reviewed at length in most foreign botanical periodicals. Its publication, indeed, has called forth a strong protest from the whole botanical world, with the exception of those Americans who, in some degree, share the author's views; and the veteran Alphonse De Candolle has taken a prominent part in the protest.

To the Berlin botanists, however, headed by Professors Engler,

[27] *Flora Franciscana* passim.
[28] *Pittonia*, vol. i., pp. 179–182.
[29] *Bull. Torrey Bot. Club*, 1891, p. 159.
[30] 1892, pp. 57–62.

Ascherson, and Urban, is due the credit of initiating a practical measure which, if adopted, will put a stop to future divagations, and, it may be, neutralise the mischief already wrought. They have sent out to the botanists of the world a memorandum which has already received the adhesion of a very large number, and which is likely, if adopted, to be productive of good results. The document is as follows:—

Since the time of Linnæus, botanists have continually endeavoured to gain a uniform nomenclature, and these endeavours were completely justified on account of an easier mutual understanding. We know very well that certain differences will always remain, because the decision on some questions only depends on the author's subjective opinion. But we hope that a gradual and continual reformation will bring an essential improvement. O. Kuntze's *Revisio Generum* has raised an evident perturbation, and will cause a complete confusion; therefore we thought it necessary to propose the following four resolutions, which refer only to the genera:—

I. The starting-point of the priority of the genera as well as the species is the year 1752, resp. 1753.

II. Nomina nuda and seminuda are to be rejected. Pictures alone, without diagnoses, do not claim any priority of a genus.

III. Similar names are to be conserved, if they differ by ever so little in the last syllable; if they only differ in the mode of spelling, the newer one must fall.

IV. The names of the following larger or universally known genera are to be conserved, though after the strictest rules of priority they must be rejected; in many of them the change of the names now used is by no means sufficiently proved. [This list I do not think it necessary to reprint here; it is likely to receive many additions.]

Ad. I. After Alph. De Candolle had proposed to take the year 1737 as the starting-point of the priority of genera many botanists had acknowledged it. But we think that the turning-point from the ancient botany to our modern science rests in the introduction of the binomial nomenclature. Therefore we propose, after a previous communication with Alph. De Candolle, to remove the starting-point for both, the species as well as the genera, as far as to the year 1753, resp. 1752, date of the *Species Plantarum*, ed. 1 (1753), with the 4th ed. of the *Genera Plantarum* (1752). Before that time the scientific position of Linnæus is not superior to Tournefort, Rivinus, and many other botanists, who often had described and segregated the genera more exactly than he did.

Ad. II. Many genera have been founded on a picture only, without a diagnosis. No doubt, by means of it a species sometimes can clearly be made out and recognised, and if the picture is a good one, all the characteristics of the plant can be observed. But a picture can never show the special characteristics alone, which raise the genus above the other of its affinity. A genus only gains priority by a verbal diagnosis, and *nomina nuda* and *seminuda* are to be rejected;

therefore the following works cannot claim a right of priority:—Rumphius, *Herbarium Amboinense* (1741–1755); Burmann, *Flora Indica* (1768); Patr. Browne, *History of Jamaica* (1756); Lamarck, *Illustration des Genres* pro parte, &c.

Ad III. There are to be conserved *Adenia* as well as *Adenium*, *Acnista* as well as *Acnistus*, *Alectra* as well as *Alectryon*, *Apios* as well as *Apium*, *Rubia* as well as *Rubus*, *Bellis* as well as *Bellium*, *Chloris* as well as *Chlorea* and *Chlora*, *Glyphæa* as well as *Glyphis* and *Glyphia*, *Calopogon* as well as *Calopogonium*, *Atropa* as well as *Atropis*, *Galax* as well as *Galaxia* and *Galactia*, *Danaë* as well as *Danais*, *Drimia* as well as *Drimys*, *Glechoma* as well as *Glechon*, *Hydrothrix* as well as *Hydrotriche*, *Micranthus* as well as *Micrantheum*, *Microtea* as well as *Microtus*, *Platystemma* as well as *Platystemon*, *Silvæa* as well as *Silvia*, &c.; we doubt that there is any scholar who will confound them. On the contrary, *Tetraclis* and *Tetracleis*, *Oxythece* and *Oxytheca*, *Epidendrum* and *Epidendron*, *Oxycoccus* and *Oxycoccos*, *Asterocarpus* and *Astrocarpus*, *Peltostema* and *Peltistema*, are only different modes of spelling the same word, and the newer one is to be refused, if they name different genera.

Ad IV. The impulse that led to the acknowledgment of the right of priority was only the vivid desire to create a stable nomenclature. If we see that by the absolute and unlimited observance of the principle we probably gain the contrary of what we intended, we, who have ourselves made the rules of priority as a law, have the right to amend the latter. Therefore we present a list of genera that have more than a merely scientific interest, or that are very large, and we propose to conserve them in spite of the rules of priority, in order to avoid a general confusion by the change of many thousand names.

When the memorandum was first submitted to us at the British Museum, we thought that the date 1737 might continue to be accepted for genera, and we wrote in that sense to Professor Engler. In his absence, Professor Ascherson replied, pointing out that this date had only been fixed by M. De Candolle in 1883, and that that botanist had no intention of implying that all genera established between 1737 and 1753, and ignored by Linnæus in the *Species Plantarum*, should be resuscitated. M. De Candolle now fully concurs in the proposals of the Berlin memorandum, and adopts the fourth edition of the *Genera* (1752) for genera, and the first edition of the *Species* (1753) for the species. The fact that the fourth edition of the *Genera* is not actually by Linnæus himself, is met by the statement that it was recognised by him as authentic, seeing that he styled the next edition, by himself, the fifth. After this explanation, the British Museum botanists withdrew their objection, and gave their corporate assent to the only article of the Berlin memorandum as to which they had had any doubt in the following terms:

"We do not see how we can stop short of 1737 when dealing with genera *as genera*. But as binomial names—and every specific name includes, of course, both the generic and the trivial designations—did not exist until the publication of the *Species Plantarum* in 1753, we agree that a name given in that work cannot be set aside for one the generic part of which was ignored or set aside by Linnæus; and we

therefore acknowledge that this practically fixes the date of genera to 1752."

Whether this solution of the difficulties which exist will receive the adhesion of a sufficient majority of botanists is at present doubtful: that some solution is needed is certain. The subject is well worth the attention of all who have at heart the establishment of a recognised standard of nomenclature on practical lines, and the present seems a favourable opportunity for settlement.

James Britten.

SOME NEW BOOKS.

Fur-Bearing Animals, in Nature and in Commerce. By H. Poland. 8vo. Pp. lvi. and 392. With 16 illustrations. London: Gurney & Jackson, 1892.

With the exception of the "Fur-bearing Animals" of Dr. Elliott Coues, which deals only with the Mustelines of North America, and was published as far back as 1872, and Mr. H. W. Elliott's various works on the seal-fisheries of Alaska, the latest of which dates from 1882, we have scarcely any books devoted to wild animals considered chiefly from a commercial point of view. Consequently, the scientific naturalist is not unfrequently at a loss to know where to turn in order to obtain reliable information as to the number of any particular species of mammal yearly slaughtered for the sake of its pelt. We are, therefore, glad to welcome Mr. Poland's work, which appears to give all available information relating to the fur trade. The author gives a very long list of mammals of which the hides or pelts are more or less used in commerce; but as varieties or local races are placed under distinct headings, the total number of animals appears greater than it would be if they were arranged strictly under specific headings.

In the Introduction Mr. Poland gives us a valuable series of tables of the numbers of furs imported into Europe and North America and Canada, as well as those passing through the hands of the Hudson Bay Company. The European returns, owing to the abolition of duty, cease with the year 1844; but the other two sets of tables commence with the middle of last century and are continued down to 1890 or 1891. In the text further details are given as to the number of pelts of the various species which come into the market. These tables enable the naturalist to gain some idea of the appalling destruction of animal life which is continually going on for the purposes of commerce; while they likewise serve as an indication of the relative degree of abundance of any particular species, and also as to whether its numbers are being perceptibly diminished. In regard to the number of pelts furnished by certain animals, we may mention that in 1844 the common squirrel yielded no less than 2,289,838 skins; while in 1883 the Hudson Bay Company exported upwards of 1,082,999 skins of the Musquash, the total annual number of the pelts of that animal being estimated on page 261 at between two and three millions. Then, again, in 1889 the number of skins of the Australian Phalanger (Opossum) imported into the United States reached the enormous total of 3,169,571.

So far as we are able to judge, Mr. Poland has succeeded admirably in his task of collecting and correlating the returns of the number of skins of the various animals of which he treats; and from this point of view his book will always have a considerable value. Unfortunately, however, for his reputation, the author has thought it necessary to favour his readers with certain observations as to the

structure, habits, and distribution of the same animals, some of these observations being of the most startling nature.

With the present abundance of books on Natural History, and especially on mammals, we should have thought it would be possible for any writer who has no special knowledge of systematic zoology to give a fairly creditable account of the leading features of the animals he notices. Mr. Poland seems, however, to have carefully avoided reading all the more important recent works on mammals, and derives a considerable amount of such information as he possesses from the somewhat antiquated works of the Rev. J. G. Wood.

We notice, in the first place, that the author has a total lack of literary style, his sentences being short, disconnected, uncouth, and in some cases ungrammatical; while many of his remarks appear decidedly naïve. As an instance of grammar, we may quote the following sentence from page 304, where we read (the italics being our own) that "A few skins of the Harnessed Antelope are imported with monkey skins, and *they* are principally used for leather, although *it* would make up well for fur."

Not less glaring is the author's careless proof-reading of the scientific names of the various animals of which he treats—a matter which should have received the utmost attention in a work largely intended for readers who are not zoologists. As instances of this slipshod carelessness, we select the following, viz.:—(p. 11) *Semnopithecus schistanus*, for *schistaceus;* (p. 31) *Felis geoffroy*, instead of *geoffroyi;* (p. 32) *F. passerum*, for *pajeros;* (p. 53) *Cynæturus*, in lieu of *Cynælurus;* and (p. 59) *Nandinia binstata*, for *binotata*. Again, on p. 306, we have *Kerwas*, instead of *Kemas*, for the Tibetan Chiru Antelope, of which, by the way, the proper name is *Panthalops*.

With regard to the errors in zoology, the following are some of the most glaring. Thus on page 13 we find the astounding statement that all the Lemurs are confined to Madagascar; while on page 113 we are surprised to learn that in Northern India an "American species, the Racoon, is found." We presume the latter statement has originated from the name "Himalayan Racoon" having been applied to the Panda (*Ælurus fulgens*); but on turning to pp. 155,156, we find the author in hopeless confusion on this subject. We have, indeed, the Panda recorded on the latter of these pages under its proper title, without any reference to its affinity with the Racoons. On the preceding page we have, however, an animal mentioned under the name of the "Cashmere Racoon," with the sapient remark that "this extremely rare animal inhabits Cashmere, and the extreme North of India"; and it is added that "the *head* is ringed like the American Racoon." In the latter remark, the author makes the trifling error of writing *head* instead of *tail;* and, as a matter of fact, the animal to which he refers must be the Panda itself, which, by the way, is totally unknown in Kashmir, or anywhere else in the North-west Himalaya.

The author is almost equally at sea with regard to the Civets, which he appears to consider merely as "cats," since he commonly speaks of them by that name. First of all he alludes to the common Indian Civet (*Viverra zibetha*) under the title of the "Chinese Bush, or Civet Cat," and states that it is an inhabitant of China and Nepal, and is probably merely a variety of the African Civet (*V. civetta*). Subsequently, it is added that "a local variety is the Indian Civet *Viverricula malaccensis*." Now if the author had taken the trouble to consult Jerdan's or Blanford's works on the Mammals of India, he would have found that *V. zibetha* is to the full as distinctively an Indian as a

Chinese animal, its range extending from Bengal to South China; and he would not have written such arrant nonsense in regard to its relationship to *V. civetta*, or as to the very distinct *Viverricula malaccensis* being merely an Indian *variety* of this so-called Chinese species.

Mr. Poland seems, indeed, to be in a state of utter confusion as to what constitutes a species and what a variety, or, indeed, a genus. Thus, when treating, on pp. 170, 171 of the Indian Sloth-bear (*Melursus ursinus*), he observes that this "bear would probably interbreed with the Black Bear of America, and if the offspring of these two bears should prove fertile, it would necessitate their being classed as one species." Has the author, we may ask, ever seen these two animals, and does he know that they differ from one another more than any other two bears he could have mentioned,—the difference even extending to a disparity in the number of the teeth?

A still more appalling error confronts us on page 304, where the West African Zebra Antelope (*Cephalophus doria*) is alluded to as the "West African Striped Deer," without any technical name. We had thought that in these days even the proverbial schoolboy knew that there were no deer in Ethiopian Africa. As minor errors, we find the Beaver alluded to on page 247 as the largest existing rodent, while on page 245 the Red Himalayan Marmot (*Arctomys caudatus*) is said to be of a "deep grey hue," instead of yellowish tawny or orange, with a black back and tail. Again, on page 144 we find the Chinese Otter (whatever species that may be) alluded to by the name of *Lutra felina* (misprinted *felisa*), which properly designates a South American form.

In treating of the Kolinsky (*Mustela sibirica*), on page 114, Mr. Poland hurls an unjust accusation against English zoologists by observing that this animal "appears not to be mentioned in any work on Natural History in this country, and to be unknown to scientists with the exception of the Russian professors." If the author had turned to the late Dr. Gray's British Museum Catalogue of Carnivores, &c., published in 1869, he would have found the animal described on page 94; while in a recently-published work, entitled "The Study of Mammals," in which the author might possibly find some information that would be worth his attention, this animal is mentioned on page 586.

We might go further, but enough has been stated to show that, however valuable may be the author's information as to the particular subject of which the work treats, yet that his zoology must by no means be taken as gospel. In conclusion, we should like to ask Mr. Poland from what animal the figure given on page 127 as a representation of the Glutton was taken, and also how he acquired the information (p. 230) that the total number of Moles in Britain is between five and six millions.

R. L.

LES MOLLUSQUES: Introduction à l'étude de leur Organisation, Développement, Classification, Affinités et principaux Types. By Henri Coupin. For the use of Candidates for the Licentiateship in Natural Science. 8vo. Pp. 261. Paris: Carré, 1892.

THE degree in Natural Science in the French universities would appear, according to our author's preface, to be more difficult of attainment than that in other faculties, and this inequality is to be attributed to the absence of suitable text-books treating of the subjects of examination. It is with the view of filling up one, at all

events, of these deficiencies that M. Coupin has prepared the treatise mentioned above. The work is intended for students preparing for examination, and from this standpoint it must, in fairness, be judged. We may say at once, then, that, though not perfect, it is much superior to the majority of works compiled with such an object in view.

Each class of the Mollusca is considered separately, and all generalisations referring to the group as a whole are deferred to the end of the work; a mode of treatment which has many advantages over the more usual plan of giving generalities first and details afterwards. As regards classification, we find six classes adopted:—*Acéphales*, *Scaphopodes*, *Amphineures*, *Gastéropodes*, *Ptéropodes*, *Céphalopodes*. No objection of weight can be urged against the use of *Acephala* for the first of these, though it is, perhaps, more suitable as applied to a dichotomous division of the Mollusca, and contrasted with *Cephalophora* or *Glossophora*. In his discussion of this group, the author departs from his usual practice of selecting a concrete example for description, giving only an account of an ideal type, with certain comparative observations. This seems to us an error of judgment; the study of an actual form impresses upon the mind of the student a quantity of definite information, and comparisons can be just as well instituted with a tangible example as with an ideal type.

With this exception, the treatment of the *Acephala* may be taken as representing that adopted in the case of the other classes, and it is on the whole satisfactory. The different systems of organs are taken up seriatim, beginning with the shell, mantle, muscles, and so forth. The descriptions are clear and concise, and are profusely illustrated by cuts, some copied from well-known works, but the greater number evidently, as stated by the author in his preface, reproductions of lecture diagrams. Many are rather rough, but they are clear, and a great help in the elucidation of the text. The work is well up to date, and in many cases references are given to original sources of information, though not so frequently as might be desirable. Following upon the anatomy are a few pages treating of embryology, then comes a *résumé* in half-a-dozen lines of the main characters of the group, and finally a systematic view of its principal families, with occasional mention of conspicuous genera. Some of these notes are good and to the point, but others are too brief to be of any use; as for instance, under "9° *Mactridæ*" the only information vouchsafed is "Les *Mactra* ont une coquille ventrue." Precisely the same character is emphasised in the case of the *Arcadæ*.

It seems needless to follow the author in detail through his treatment of the remaining groups. We cannot, however, conceal our astonishment that he should retain the Pteropoda as a distinct class, when he is evidently well acquainted with the work of Pelseneer and others on this question, and, by his own admission (p. 191), attaches great weight to their arguments.

The descriptions are in the main accurate, though errors in matters of detail occur here and there, as, for instance, when the figure of a tentacular club of *Sepia tuberculata* is made to do duty for that of *S. officinalis*, and no mention is made of the infrabuccal cushion on which the female *Sepia* receives the spermatophores. Taken all in all, however, the book is probably the best small text-book extant of malacology as opposed to conchology. It is much to be regretted that its pages are disfigured by a plentiful crop of misprints.

W. E. H.

An Introduction to the Study of the Genera of Palæozoic Brachiopoda. Part I. By James Hall, assisted by John M. Clarke. [Geological Survey of the State of New York, Palæontology, vol. viii., 1892.] Large 8vo. Pp. xvi., 367. Pls. 43. Price $5.

The study of fossil Brachiopods, especially the Palæozoic, is beset with many difficulties. It has long been considered, and with much reason, that the only true basis of classification is to be found in the variations of internal structure, in the muscle markings, and in the calcified support of the lophophore or so-called "arms." Rarely, however, is a specimen found that shows or even has preserved this internal structure. Many a fossil must be ground down or dissected out with infinite labour, only to find nothing in the end. And when something has been discovered, and we are told, for instance, that a fossil long known as *Atrypa* is really a *Nucleospira*, still the geologist or the working palæontologist is not much better off; for the resemblances in outward shape between some species of widely-separated genera are often so great that we cannot, by ordinary methods, tell with which we have to deal.

For these reasons a hearty welcome must be extended to anyone that points out features of classificatory value hitherto unrecognised, or that applies to the study of the Brachiopoda principles that their students have up till now neglected. This has recently been done by Dr. C. E. Beecher, of New Haven, Conn., in some interesting and suggestive articles in the *American Journal of Science* (April, 1891, and August, 1892). He has been the first to regard the Brachiopoda, not as a number of separate species and genera that have somehow to be arranged in several families, but as a great series of living and growing individuals, one linked to the other and transmitting to it its own characters, though in a slightly modified form.

America certainly seems to be the country to which we must look for future advances in our knowledge of the Brachiopoda. That there is magnificent material has long been known, but hitherto we have had to trust too largely to the Cincinnati collectors for the interpretation of it. At last there is arising a school of palæontologists who look at fossils with the eyes of evolutionists and morphologists and not with those of species-mongers, and who do not despise the literature of the subject because it happens to have been published in a far country or written in a foreign tongue. There have been palæontologists in America, of course; among those dealing with Invertebrata, the names of Meek and Worthen and Shumard are bright examples. It is not the absence of good work that we have so often had to lament, as the presence of so much that is bad. In the domain of the Brachiopoda, at least, there will in future be scant excuse for bad work; for there has just appeared the first part of a book that, while gathering up the labours of the past, indicates clearly the direction of future toil.

This work, of which the title is given above, is the joint production of the veteran palæontologist of America and of one of the most promising members of the new school. Professor Hall also acknowledges his indebtedness to his private assistant, Charles Schuchert, who has already published some valuable systematic papers on American Brachiopods.

The part now issued (June, 1892) retains the Articulata and Inarticulata as convenient temporary divisions, and deals with all the latter, and with the *Orthis* and *Strophomena* groups of the former. It is an interesting feature of the work that no attempt has been made

to split up the included genera into families; they are, however, arranged in such an order that their affinities are clearly seen, and so that one genus naturally leads on to the next. To touch on minor points, we rejoice to see that the writings of Pander at last receive proper recognition, a result of which is, that the name *Clitambonites* resumes the place long usurped by the *Orthisina* of D'Orbigny, a restoration that has, we believe, already been made in our own Natural History Museum. The much-abused name *Strophomena* is at last restricted to what has long been recognised as the type of Rafinesque's genus, viz., "*Leptæna planumbona*" of Hall. The well-known *Strophomena rhomboidalis* is naturally restored to its proper place in the *Leptæna* of Dalman (whose original plates, by the way, are here reproduced to the great advantage of American readers); while the forms usually called *Leptæna* by modern writers take their true name of *Plectambonites*, Pander.

These changes, which will, of course, appal a good many people, will be welcomed by all who have to work on the Brachiopoda, as not only doing justice to the older writers, but as bringing the matter out of the confusion into which it had been cast by the arbitrary restrictions and alterations of those who followed them. After all, attempts at compromise in these questions generally work more harm than good, and it is best in the end to follow the simple Law of Priority. We are a little apt to think too much of ourselves and of the momentary inconvenience that results, the inconvenience of re-writing a few labels or re-arranging a few specimens; and we do not think enough of those who are coming after us, and who are the inheritors of our weak-minded muddle-headedness no less than of any little advance in knowledge we may be fortunate enough to make.

F. A. B.

Our Temperaments: Their Study and Their Teaching. A Popular Outline. By Alexander Stewart, F.R.C.S., Edin. Second edition, carefully revised; indexes and additional illustrations, including chromolithographs from drawings by Lockhart Bogle. 8vo. Pp. xxviii., 412. London: Crosby, Lockwood and Son, 1892.

It should be generally known—though it does not appear to be—that there are four main types of physical organisation to which, or to compounds of which, every man may be referred. These are the Temperaments, and they are named, in accordance with the system most prominent in each one, the Sanguine, the Bilious, the Lymphatic, and the Nervous, which last has replaced the old Melancholic. Medical men, from Hippocrates to Sir J. Crichton Browne, have recognised not only the existence of temperaments, but the importance of their study; and yet the physical and mental characteristics of each temperament are still far from known. Shakespeare and Addison remind us that the theory has been appreciated by poets and prose-writers of all time; and yet the word Temperament is, unfortunately, misused in our daily conversation and our daily Press. These thoughts have urged Mr. Stewart to publish the work of which a second edition lies before us.

Were the temperaments nothing more than expressions of outward characters, such as the red hair, blue eyes, florid complexion, and thick-set build that make up the sanguine temperament, their study would be little more than learned trifling. It is the theory that these outward marks are the signs of certain predicable mental

characters and of a tendency to certain diseases that invests the temperaments with their importance. Some such belief influences us all in our dealings with our fellow-men; the stage faithfully mirrors it in its tradition that the hero must be of sanguine, the villain of bilious temperament (the low comedy is generally lymphatic). Stage-types, however, are rare in real life, and it is the prevalence of organisations in which the elements (to use Shakespeare's phrase) are mixed, that is, of compound temperaments, which raises difficulties to the practical application of the theory. It is on the discrimination of these mixed types that Mr. Stewart lays particular stress. He claims to have rendered the study more easy by the ascription of form, as well as colour-characters, to the chief temperaments. Since, however, the form-characters of the sanguine, bilious, and lymphatic temperament are all the same, it does not appear that this indicates any great advance.

Still, as of old time, the great battle is being fought between matter and spirit. Is it really true that every mental and moral trait of an individual is indissolubly bound up with the structure of his flesh and blood? Nay, more, that the connection is so great that from the outer we could, with but a little more knowledge, predicate the inner man? Does a man enter on life with ready-made works, a given motion, and a spring that will work so long and no longer—the only thing that can modify his action mere external circumstances? These are questions of supreme importance, but they will not be answered by vague generalities. The great want in Mr. Stewart's book consists in facts. A few are quoted from Mr. Francis Galton and Dr. Beddoe; but beyond the author's own statement, we can find no indication that he has studied the subject in a scientific manner.

The book, however, is an interesting compilation and contains some sensible, if rather obvious, remarks on education, the choice of a profession, and the duty of biographers. *Apropos* of the remarks on the last subject, we may complain that Mr. Stewart does not tell us what his own temperament is. We infer, however, that he is sanguine, since he seems to have the following mental characteristics:—"Favourable conclusions thoughtlessly drawn," "Equally happy in the pursuit of little as of great ends," "Not minutely informed."

Woodwork. By S. Barter. Pp. 343. London: Whittaker & Co., 1892. Price 7s. 6d.
Manual of Instruction in Woodwork. By George Wood. Pp. 104. Leeds: E. J. Arnold, 1892. Price 5s.

Circular 44, issued by the Science and Art Department in 1890, contains several suggestions respecting the course of manual instruction in elementary schools. Paragraph 5 suggests that "clear ideas respecting the growth and structure of woods may be given by means of a small collection of the different descriptions of timber used in carpentry." Writers of books on Manual Training in Woodwork, with this suggestion in view, have thus included one or more chapters dealing with the purely scientific aspect of the subject, but it is to be regretted that authors capable of giving valuable information on the best methods to adopt in manual training classes did not secure some competent co-worker to write the strictly botanical part of the book. If it is necessary to carry out all the Department's suggestions in one book, there is a clear case for collaboration. The astounding

errors into which the writers have fallen make it quite necessary to warn teachers about to consult books on manual instruction that they should read the chapters on the growth and structure of trees with much caution, or, better still, they should not read them at all. The information which they require can best be obtained from a good text-book on elementary botany, or from that capital book "The Oak," by Marshall Ward.

In illustration of our contention, we have selected the two recently published works mentioned above. In each of these books we find whole paragraphs incorrect in every detail, the writers having anything but "clear ideas" of how a tree really grows, and what is the true nature of the current of water which passes from the roots upwards. The transpiration current, as such, is not mentioned, and the true function of foliage-leaves, together with the distribution of the plastic substances to all growing organs, is totally ignored. The result of this year's examinations having just been published, it would be interesting to know how many teachers who used these books failed to express themselves clearly in answering questions on the mode of growth of plants.

In the first-mentioned work we note, on page 40: "A slowly-flowing current of sap, obtained principally by the roots, ascends the tree, and the wood-forming materials contained in it feed and thicken the walls of the tracheides." On the subject of felling we are told: "A tree should always be felled when the sap is at a standstill—the height of summer—when it has ceased to run up the tree, but has not yet begun to return; or the winter, when it has completely run down" (page 44).

From this the teacher of manual training is to obtain "clear ideas," which he will hand on to his class. It is no great stretch of the imagination to picture a boy going home from his lesson with a confused notion that sap has a kind of clockwork action; it occasionally "runs down," and sometimes "stops."

The following, from the second work, reads like a huge joke: "The rising or common sap containing much moisture flows towards the leaves. Oxygen and moisture are evaporated, while carbonic acid is absorbed by means of the stomata. The sap, now changed to proper sap, descends in the autumn towards the roots between the bark and the stem, depositing new woody fibres in the cambium (sticky substance) surrounding the stem. Thus, a new deposit of woody matter is formed from the sap all round the stem."

It is rather startling to be gravely told that the sap contains much moisture, and that the proper sap descends in autumn after staying up aloft for so long!

We trust that should a second edition of either of these handbooks be called for (they are capital guides respecting the manual instruction) the writers will see that the part dealing with the growth and structure of trees is entirely re-written by persons competent to deal with the subject.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. R. von Lendenfeld has been appointed Professor of Zoology at Czernowitz, Galicia.

Dr. Henry B. Ward has been appointed Instructor in Invertebrate Morphology in the University of Michigan, Ann Arbor; and Dr. Charles L. Edwards directs the Biological Institute of the University of Texas at Austin.

The opening address of the present session at the Owens College, Manchester, will be delivered early this month by Mr. F. E. Weiss, the newly-elected Professor of Botany.

The Annual Report of the British Museum for 1891 has been received. The Statement of Progress in the Departments of Natural History will be read with much satisfaction, and the list of publications issued during the year includes eight volumes of the well-known Catalogues. The number of acquisitions is especially large, including no less than 1,186 presents, and several of these are of great value. The Natural History collection bequeathed to the Trustees by the late Dr. R. M'Cormick, includes many important specimens from the Arctic and Antarctic Regions; and a large collection of Zoological specimens made by Mr. J. J. Walker in the seas off the W. and N.W. coasts of Australia adds many valuable marine organisms. A fine series of nearly 290 horns and heads of Indian mammals has been presented by Mr. Allan O. Hume, C.B.; and another large collection of skins and spirit specimens of mammals from the same country is the gift of Dr. W. T. Blanford, F.R.S. Among beetles, there is another donation from Messrs. Salvin and Godman; and among moths the chief present is a series of Ceylonese specimens, selected from the cabinet of Mr. E. E. Green. The largest donation of fossils is that of Mr. S. J. Hawkins, comprising valuable specimens from the Chalk of Kent, and mention ought also to be made of a unique portion of skull of the Saiga Antelope from Thames gravel at Twickenham, discovered and presented by Dr. J. R. Leeson.

The Director records the extension of the building for the accommodation of specimens preserved in spirit, and especially points out the desirability of increasing this collection. Greater attention is now paid to anatomical characters, as indicating the true position and affinities of animals, and these are often not exhibited in the dried specimens which formerly sufficed for the needs of the zoologist, but can only be studied in specimens preserved in alcohol.

Those who are interested in the British Museum more as a place of education than a store-house, will observe with especial pleasure the report of last year's course of lectures on Geology delivered by Professor Alleyne Nicholson on the Swiney Foundation. The attendance of the public was far larger than formerly, the average number being 214, and on several occasions the late-comers could not be admitted from want of accommodation. The director remarks that if the atten-

dance is maintained in future years, it will be necessary for the trustees to consider the question of providing a properly-constructed lecture-theatre. Professor Nicholson's course this year begins on Monday, October 3, at 3 p.m., and will be continued on each Monday, Wednesday, and Friday for one month. The subject is, "The Great Periods of Geological History," and a detailed syllabus may be obtained from the director of the British Museum (Natural History), South Kensington.

We learn that the collection formed by Mr. Hose in Borneo has been acquired by the British Museum. This collection is rich in birds, and will, it is hoped, yield many forms previously undescribed or of special interest.

The Palm House at Kew has lately been enriched by a healthy young specimen of the Palmyra Palm (*Borassus flabelliformis*), a present from the Gaekwar of Baroda. It has a trunk 2 feet in diameter and 4½ feet in length. Attempts to grow this palm to any size at Kew have hitherto failed, though at present there are some promising seedlings raised from seeds presented by Sir M. E. Grant-Duff in 1886. The rich collection of Filmy Ferns has just been arranged in a new house measuring 50 feet by 14, with a central path and two cases running the full length. The Museum has also been enriched by a present from Mr. Charles Holme of an extensive valuable series of products illustrating the uses of the Bamboo in Japan. It gives a good idea of the great value of this tree-like grass, and the ingenuity of the people in adapting it.

The recent disastrous hurricane at Mauritius almost completely destroyed the beautiful Botanic Gardens. The *Kew Bulletin* quotes from a letter of Mr. W. Scott, the Acting-Director, who says:—"The sugar-canes have been levelled, and their leaves threshed into fibre. The gardens at Pamplemousses are a complete wreck; the oldest and finest trees have been uprooted, and the trees that are still standing are reduced to bare poles. The fruit trees have been so smashed up that I can hardly recognise them. In some parts the wreckage is piled up nine feet high, and it will be months before we can get it cleared away."

The British Institute of Public Health held its annual Congress in Dublin this year, under the presidency of Sir Chas. Cameron, on the 17th August and following days. The first two days were chiefly occupied with the reading of papers, and the concluding days with excursions. Some papers on bacteriological subjects were of interest to naturalists. Dr. M. Weeney, of the Mater Misericordiæ Hospital, Dublin (which posseses the only bacteriological laboratory in Ireland), dealt with the question as to which *Bacillus*, *B. typhous*, or *B. coli-communis*, causes typhoid fever. Mr. W. E. Adeney, in a paper on the Chemical Bacteriology of Sewage, attributed the oxidation of waste organic matter to the life-processes of certain micro-organisms. The most important of the excursions was that to the artificial lake among the Wicklow Hills, where the waters of the Vartry are impounded to form a supply for the City of Dublin.

The fifteenth annual meeting of the Midland Union of Natural History Societies is reported in the *Midland Naturalist* for September. It was held at Oswestry, on August 24th and 25th, under the presidency of Mr. A. T. Jebb, of Ellesmere, and there was a good attendance. The report of the Council makes special reference to the function of the Union as an organiser of scientific work—"a central organisation, by means of which the scientific work of the more active societies in the Midlands may be rendered more effective"—and then laments the meagre character of the results with which it has to deal. In Botany, indeed, the Executive Committee reports that no completed paper or papers of a character to warrant the bestowal of the Darwin medal have appeared in the *Midland Naturalist* during the past three years, and the award is thus not made this year. The subject

for next year's award is that of Geology. During the last session the Cheltenham Natural Science Society and the Worcestershire Naturalists' Field Club have joined the Union, while the Tamworth Natural History and Antiquarian Society has ceased to exist.

At the twenty-first annual Conversazione of the Chester Society of Natural Science held last month, the Kingsley Memorial Medal was awarded to Mr. Osmund W. Jeffs, the energetic secretary of the British Association Committee on Geological Photographs.

Several new publications of the British Field Clubs have lately been issued. Vol. vi. of the *Transactions of the Hertfordshire Natural History Society and Field Club* is now completed by the issue of parts 8 and 9, which comprise a record of proceedings and excursions, with an index and list of members. The *Essex Naturalist* continues actively to record the fauna and flora of the county to which it relates. Few of the papers are of more than local interest, but the proximity of Epping Forest and of the Essex marshes will make the journal interesting to the London naturalist. We are sorry to read so many records of the extermination of rare species.

The *Proceedings and Transactions of the Croydon Microscopical and Natural History Club*, Feb. 11, 1891—Jan. 13, 1892, contain little original matter beyond tables of rainfall. The reports of the six sub-committees (or sections) occupy only four pages, and among the chief achievements of the geologists during the year it is recorded that photographs were taken "of an interesting old gabled farmhouse." The Malden Natural History Society issues a small 4-page monthly *Gazette*, price one penny, with very brief general articles. Part I. of the new volume (xxiv.) of the *Transactions of the Cardiff Naturalists' Society* contains a readable report on Meteorology, by F. G. Evans; an article on Hardy Fruit Culture in South Wales, by A. Pettigrew; a description of the Neath Valley, by T. H. Thomas, and smaller notes.

The *Transactions of the Norfolk and Norwich Naturalists' Society* for 1891–92 contain several papers of more than local interest, though as usual their strong point is local natural history. The President, Mr. F. D. Wheeler, writes of the extinction of the Fenland lepidoptera; Dr. C. B. Plowright on Neolithic implements in West Norfolk; Mr. Southwell on the St. Helen's Swan-pit (near Norwich); Mr. Clement Reid on the Natural History of Isolated Ponds; Mr. Aplin on the distribution of the Red-backed Shrike; and we have the usual miscellaneous notes on local meteorology, ornithology, and fishes. Why does not this society do anything with the marine invertebrata? Norfolk has an extensive coast-line.

The *Report and Transactions of the Penzance Natural History and Antiquarian Society* for 1891–92 completes vol. iii. of the new series. The Council suggests the award of prizes to juveniles for collections of specimens by way of fostering an interest in Natural History, the society being much in want of members and contributors to its museum. The Transactions deal chiefly with antiquities, but there are also valuable lists of sea-anemones, corals, beetles, and mosses.

OBSERVATIONS AND CORRESPONDENCE.

Professor Giard on the Evolution of Flat-Fishes.

Professor Giard replies to my article on "The Evolution of Flat-Fishes" as if I had commenced the controversy between us by making an attack on him. The fact is, on the contrary, that my criticism of his note on an abnormal turbot was a defence of my experiments as described in the *Zoologischer Anzeiger* against his attempt to prove that they were superfluous on the one hand and inconclusive on the other. The Professor deliberately maintained that the existence of specimens of *Rhombus maximus* in which the head was in certain respects monstrous or abnormal, and in which the blind side was pigmented all over like the eyed side, was in itself a sufficient proof that the coloration of the upper sides of flat-fishes was due to the direct action of light, and that pigment was produced on the lower sides of such fishes when those sides were exposed to light. My opponent still refuses to admit the cogency of my arguments and the validity of my defence, and I will try, therefore, to answer his objections.

Professor Giard distinguishes between what he calls monstrous Pleuronectids which show arrested development and the double flat-fishes whose existence he says I stated to be recorded in numerous works on Ichthyology. The distinction is valid, and I have never ignored it: there are ambicolorate specimens which are normal in all respects except colour, and there are ambicolorate specimens in which the head and eyes have the abnormality described in Giard's specimen. But what I said was, that the "monstrous Pleuronectids which show arrested development without stoppage in growth," to use the Professor's words, and not merely ambicolorate specimens, were recorded and described in a large number of ichthyological treatises, and that the specimens recorded did not belong exclusively to the one species *Rhombus maximus*, as Professor Giard stated, but to other species and other genera as well. So much he now admits, citing numerous cases in literature. He refers to a specimen of the turbot described by Couch, and quotes that author as stating that the same monstrosity is not very rare in *Zeugopterus punctatus*. I regret that he has not given volume and page for these references to Couch, for in Couch's general work on "Fishes of the British Islands," vol. iii., I find a description and figure of the monstrosity in question in the flounder, *Pleuronectes flesus*, but no mention of it in connection with *Rhombus* or *Zeugopterus*.

But in correcting his original statement Professor Giard says the monstrosity occurs among the flounders, "whose development is as slow as that of the turbot," and that it may be stated in a general way to occur among the Pleuronectidæ of gradual metamorphosis (palingenetic development). I confess I cannot fully understand these statements, but so far as they convey a meaning to me I entirely disagree with them. The exact age of the turbot and flounder at the beginning and end of metamorphosis, and the exact time occupied in that process, has never, so far as I know, been ascertained. But it is certain that the development of the turbot and brill is in some respects peculiar and different not only from that of the flounder, but from that of all other Pleuronectidæ. The two species of *Rhombus* have a well-developed air-bladder during their metamorphosis, and, in consequence of this, swim near the surface of the sea until the metamorphosis is complete, or nearly so; they also attain an unusually large size before they acquire the adult characters. I have specimens of the turbot 3·6 cm. in length in which the transformation is by no means finished. The flounder, on the other hand, and all species of the genus *Pleuronectes* have no air-bladder during metamorphosis, and begin to lie on the bottom at the very commencement of that process. The flounder has acquired the characters of the adult

when it is 2 cm. in length, and yet it is larger, when first hatched, than the newly-hatched turbot. All other genera whose larval stages have been described resemble *Pleuronectes* in their development and differ from *Rhombus*, excluding, of course, the so-called *Plagusia*, which has a peculiarity of its own. I cannot understand, therefore, what is the similarity in development between turbot and flounder to which Professor Giard alludes. I equally fail to understand how any Pleuronectidæ can be distinguished from the rest as having a gradual metamorphosis or palingenetic development, since in that respect they all agree. Every Pleuronectid is hatched as a perfectly symmetrical larva swimming vertically in the sea, and only acquires the peculiarities of the adult after a gradual metamorphosis, in which one eye passes from its original position to a new position on the opposite side of the head.

If Professor Giard has discovered a true Pleuronectid which is hatched in the asymmetrical condition, he should say so; and I should be the first to admit that he had made a most remarkable and interesting discovery. It is, of course, true that there is another mode of metamorphosis among Pleuronectidæ besides that which occurs in the majority of genera such as *Rhombus* and *Pleuronectes*. This other mode is that described by Steenstrup and Agassiz in *Plagusia*, in which genus the eye passes through the base of the dorsal fin to reach the opposite side of the head. But surely the Professor would not assert that this mode is less palingenetic than the other? It is possible, indeed, that the early development of the dorsal fin is a cenogenetic character, but the phylogenetic history is repeated in the bilaterally symmetrical larva and the gradual torsion of the eyes in *Plagusia* as in other Pleuronectidæ.

I am not concerned to reply to Professor Giard's remarks about the abundance of ambicolorate specimens in which no other abnormality is present. I need only say that I am perfectly aware of their comparative commonness, and the point is not in dispute between us.

We come now to Professor Giard's reply to my criticism of his assumption that the abnormal turbot he described must have swum in a vertical position.

Neither in his original note nor in his reply to my article has the Professor asserted that he ever saw an abnormal turbot, of adult size, swimming vertically in the water; he has simply said that his specimen *devait nager* in that position. He asserts he had the testimony of Day and McIntosh in support of that opinion, but this statement is not strictly accurate. As I stated in my previous article, I have failed to find a particle of direct evidence, a single recorded observation, to prove that abnormal flat-fishes swim vertically. Day (*loc. cit.*) says that double flat-fishes "are seen to swim vertically and to be more frequently found near the surface of the water than those which progress in a normal manner." But he does not say he has seen this himself, nor mention anyone who has seen it. As for Professor McIntosh, he has not in either of the works cited by Professor Giard, namely," Marine Invertebrates and Fishes of St. Andrew's," and the memoir on the "Development of Teleostean Fishes," made any statement at all in reference to the position in swimming of monstrous adult specimens of flat-fishes of the same kind as Professor Giard's turbot. I am considering now abnormal monstrous specimens of adult size, not larvæ at a stage of metamorphosis, which are, of course, normal. As there are no recorded observations as to the mode of swimming of the monstrous adult specimens, certainly none cited by Professor Giard, I could not, and did not, assert that such observations were erroneous.

The Professor's method of controversy seems to me lamentably disingenuous. He assumes that his monstrous turbot swam vertically, and I pointed out that there was no evidence to prove this; yet he now maintains that he had the testimony of Day and McIntosh to rely upon, although, as I have shown above, neither of those authorities gives any evidence. My remark, "this is an error of observation," referred to a perfectly distinct and separate question, namely, the position, in swimming, of *normal larval* specimens whose metamorphosis is in normal progress. Professor Giard, in his original note, stated that Professor McIntosh had described "un individu normal de 9 millimètres de long sur 7 millimètres de large, dont l'œil droit était déjà arrivé sur la crête du dos, et qui nageait encore dans la position verticale." I replied that the assertion of its swimming in a vertical position was

due to an error of observation, basing this reply not, as the Professor says, on the observation of an adult double plaice, which has nothing to do with the case of a larva, but on the repeated observation of dozens of larval turbot at the stage of metamorphosis described by Professor McIntosh. But I find, on referring to the passage in question (*Trans. Roy. Soc. Edinb.*, vol. xxxv., pt. iii., p. 846), that I owe an apology to Professor McIntosh. The error is not an error of observation on his part, but an error of citation on the part of Professor Giard. Professor McIntosh does not state that the larval specimen in question swam constantly in the vertical position. He does not state that he saw it alive. All he says in reference to its position is as follows: "The appearance of certain spines on both sides after the right eye is at the edge, indicates the possibility that for some time the fish may *occasionally resume* the vertical position in swimming."

I must mention here, lest I should be suspected of omitting anything, that the only other citation of Professor McIntosh by Professor Giard, namely, from the "Marine Invertebrata, etc., of St. Andrew's," refers to a young turbot which had an eye on each side, and in which the dorsal fin did not form any hooked process, and which was, therefore, quite different from the typical form of arrested development under consideration.

Professor Giard states that the double plaice which I described in my article as being under my observation in captivity was not monstrous and only imperfectly double. I stated that it *was* monstrous in Professor Giard's sense: that the right eye was on the edge of the head and the dorsal fin terminated behind that eye, though it does not form so prominent a hook as in other cases. What excuse is there to be made for the Professor when he thus asserts that my specimen has the head and eyes normal, although I expressly described the head and eyes as abnormal?

Professor Giard now admits that ambicolorate specimens with normal eyes lie horizontally on the ground. That being so, how can an ambicolorate specimen, with an abnormal head, be a proof of the direct action of the light in producing pigment on the side where it is normally absent?

Professor Giard now affirms not that the monstrous specimens swim all their life vertically, but that they remain longer in that position than others, in any case long enough to allow the influence of the light to act efficaciously on the side ordinarily colourless. But he has less evidence, if possible, to support this than he had to support his original assumption. My contention has been throughout that evidence is required on the question of the relation of pigmentation to light. I produced some evidence in the published result of my experiment. Professor Giard has failed to appreciate that evidence; he has produced no evidence himself, but, while recording a specimen of a familiar abnormality, has attempted to maintain that the existence of this abnormality renders my evidence superfluous. He accuses me of an astonishing misconception of the laws of cross-breeding. The misconception was not mine, for I simply pointed out the necessary consequences of the action of inheritance on the colour of flounders as conceived by himself. Supposing we accept his correction, what is the application to the results of my experiment? The progeny cannot be intermediate, he says, between two progenitors, but must resemble closely one or the other. Therefore, the progeny of a right-sided and a left-sided flounder must be either right-sided or left-sided. But the flounders in my experiment were coloured on both sides, and, therefore, the possibility of the occurrence of reversed examples among their ancestors had nothing whatever to do with their condition, as Giard originally suggested.

This controversy was not of my seeking, and I can only regret that Professor Giard should have been betrayed in discussing this subject into a temporary departure from the accuracy and caution which he usually exhibits. Any zoologist who reads the literature must admit that satisfactory evidence as to the causes which produce abnormalities of colour and structure in flat-fishes is not at present available. That the typical ambicolorate monstrosity in which the metamorphosis of the head is permanently arrested is produced by the action of light, or by the direct action of conditions at all, is a pure assumption unsupported by any evidence, and, therefore, to maintain that the occurrence of such specimens is a proof that pigmentation of the skin is dependent upon light, or its absence on the absence of light,

is simply to ignore the nature of proof. All that I have ventured to maintain is that, under the conditions of my experiments, the action of light determined the production of pigment on the lower side of flounders. It by no means follows that the occurrence of pigment on the lower side of a flat-fish under natural conditions has been due to the direct action of light.

In conclusion, I will add a few words of explanation which will, perhaps, enable Professor Giard to understand why I felt it necessary to criticise his note so severely. Like himself, I have a strong leaning towards Lamarckian principles, and I considered that the almost universal fact that pigment is more abundant on the dorsal than on the ventral surfaces of animals, and the fact that whereas in fishes generally the ventral surface, which is turned downwards, is pigmentless in the flat-fishes, the whole of the lower side has become pigmentless and the whole of the upper uniformly pigmented, indicated strongly that the incidence of light rays on the skin caused the deposition of pigment. I studied the question of ambicolorate specimens and could find no sufficient evidence that in them the lower sides had been more exposed to light than normal specimens, and I felt that sufficient evidence in a case of this kind could never be got by observation of fishes in their natural wild state. I came to the conclusion that questions of this kind could only be decided by experiment. Having succeeded beyond my expectation, I was naturally indignant when a zoologist of reputation, from whom I had reason to expect sympathy and support, should come forward and publicly depreciate my methods and my results, and endeavour to throw the whole subject into the hopeless quagmire of fallacy which I was trying so carefully to avoid. J. T. CUNNINGHAM.

PROFESSOR MIVART ON THE FOUNDATIONS OF SCIENCE.

THE *ultimate* basis of all science (and it is an ultimate basis which forms the object of Professor Mivart's discussion) can be nothing less than the general conditions of consciousness upon which the very possibility of experience depends. It is possible to enunciate a large number of propositions, psychological, logical, or otherwise, which *may* be self-evident and which *may* be necessary, but which may yet have no claim to be the foundations of science. Furthermore, the validity of many such propositions remains doubtful so long as their connections with principles more fundamental are still to seek.

Professor Mivart's "Intellectualism" seems to be in this predicament; for as a system it begs the whole question of the theory of cognition. Thus, of the four "essential truths" each of the first three lays down as a postulate without proof or definition (and so presumably as ultimate) what is in truth the very problem to be solved. The first talks of *substantial* and *continuous* existence, and the second of *real* existence, without any suggestion of a suspicion that these words raise the whole question as to what is the nature of human knowledge; while the third even more naïvely asserts that there are truths which are apart from the existence of any or every mind and that *this* is matter of knowledge.

As an example to prove that we have knowledge which is not conditioned by our faculties, Professor Mivart instances the case of number. But surely it may be questioned whether "facts concerning number," to use Professor Mivart's phrase, would exist at all if it were not for our faculties of perception. Thanks to our faculties of perception we have objects to count, and if intuition did not give us objects to count, can we suppose that numbers would have for us any significance whatever?

The objection which Professor Mivart makes to the Kantian view of Space and Time (which, indeed, has often been made and refuted before) does not seem to me serious, because it is really irrelevant. Space and Time certainly do take their place as abstractions whenever in the course of our studies we make *them* the objects of empirical thought. As abstractions, they then come before us among other contents of consciousness. But the fact that we can or must think of them thus when we treat them as objects has nothing to do with the question as to whether they have a just claim to be regarded as fundamental conditions of the production of experience by sensuous intuition.

The "other truth" which Professor Mivart puts forward as of special impor-

tance for Biology, justifies even more forcibly the charge which has in the preceding remarks been made against *Intellectualism*, that it makes no attempt to face the really fundamental problems for which critical Idealism has, whether successfully or not, at least offered us its solution. We may *talk* of "a principle which is neither a material substance nor a physical force," but philosophy has yet to begin *its* work. Surely we have a right to ask what do you mean by material and immaterial? What do you mean by substance? What is the source of these conceptions? What is their content? What is their validity? And so long as such questions remain unanswered we may well doubt whether the system of Intellectualism does more than offer doubtful dogmas in the place of firm foundations for the sciences.

R. J. Ryle.

The Grammar of Science.

Allow me to thank Dr. Ryle for drawing attention to a misprint or slip of the pen in my Review (Nat. Sci., vol. i., p. 305) of Professor Pearson's *Grammar of Science*. "Perceptual conception" is obviously self-contradictory. What I wrote, or intended to write, was "perceptual surface."

Allow me also to assure Dr. Ryle that (as those who have done me the honour to read what I have written thereon are in a position to know) I do trust implicitly, and without fear or hesitation, in the results of psychological analysis; that I do not wish to "shelve" (in the sense of neglecting) either physics or psychology, though I do advocate our keeping them on different shelves; that it is not I who forget "that human experience is none the less real . . . . if it can be shown that the terms which express that experience owe all their significance to their inseparable association with the nature of our perceptive faculty"; and that, if I do serve two masters, I have at least the wit to know what service is due to each.

C. Lloyd Morgan.

The Permanence of Oceans and Continents.

In Dr. Wallace's paper on "The Permanence of the Great Ocean Basins," published in the August number of Natural Science, p. 418, he mentioned as a reason for bringing forward new arguments in favour of the theory of permanence the circumstance that that theory had been attacked by Mr. Jukes-Browne in "The Building of the British Isles," and by myself in an address to the Geological Society, in 1890. Mr. Jukes-Browne has very justly shown, in his paper on "The Evolution of Oceans and Continents," printed in the September number of this journal, that Dr. Wallace's arguments are directed against views not supported by either of us and now held by but very few, if by any, well-informed geologists. How far such extreme views were advocated in my address, and in what respects that address should be regarded as an attack on the permanence theory, will, I think, be shown by the following extract, taken from the concluding remarks (*Proc. Geol. Soc.*, 1890, p. 107):—

"It will thus be seen that while the general permanence of ocean-basins and continental areas cannot be said to be established on anything like firm proof, the general evidence in favour of this view is very strong. But there is no evidence whatever in favour of the extreme view accepted by some physicists and geologists that every ocean-bed now more than 1,000 fathoms deep has always been ocean, and that no part of the continental area has ever been beneath the deep sea."

I think anyone who reads my address will see that this passage is a fair summary, and that my principal arguments applied, not to the theory of general permanence, but to the view then held by Dr. Wallace and some other naturalists and geologists, that the continental area is limited by the 1,000-fathom line. This view is now admitted to be untenable by Dr. Wallace, and as he concurs in the possibility of ancient land-connections between the three southern continents and the Antarctic land, he concedes every case that I dwelt upon except one, the existence, in Mesozoic times, of land uniting Madagascar and India, to which he does not refer. Under these circumstances, I think anyone reading Dr. Wallace's paper might suppose that the differences between us are much greater than is really the case.

The three arguments now brought forward by Dr. Wallace, are, as he says, "altogether inconsistent with any general interchange of oceanic and continental

areas"; but the first does not apply to partial and local changes; the second, as Mr. Jukes-Browne has shown, is open to considerable question; and the third, which appears to me of more weight, is an old argument restated, for it depends entirely upon the absence of deep-sea formations in continental rocks. This I have always regarded as the strongest and most important piece of evidence in favour of the permanence of continental areas. I dealt with the subject at some length in the address already quoted, and I can only add that the caution I then recommended in accepting the evidence as complete, and in supposing that no deep-sea deposits exist in continental areas where none have hitherto been detected, has been justified by several additional discoveries of such deposits in various parts of the world.

Professor James Geikie, in the very interesting address recently delivered to the Geographical Section of the British Association at Edinburgh, expressed himself thus:—"The continental plateau and the oceanic hollows have never changed places, although from time to time portions of the latter have been ridged up and added to the margins of the former, while ever and anon marginal portions of the plateau have sunk down to very considerable depths." This appears to me fairly to represent our present knowledge of the subject, and to be in no respect opposed to the views I expressed in 1890. W. T. BLANFORD.

THE DISPERSAL OF FOREST TREES.

I WOULD ask Naturalists to study during the present month the dispersal of acorns. Seedling oaks are found abundantly in peat-mosses, and on open heaths at considerable distances from any mature trees. The dispersal of the acorns is generally, and I believe correctly, referred to the agency of rooks, which at this season desert the open fields for the oak copses. Actual observations are wanted as to how far a rook will carry an uninjured acorn in its beak, and whether the carrying of the acorn into the open to eat is a habitual action, or only done occasionally. One constantly finds torn (not gnawed) acorn-husks and half-eaten (pecked) acorns in open fields.

The question involved is a wider one than the mere inquiry: Why do seedling oaks appear in such unexpected places? Taking the oak as a type of our forest trees, we would inquire: How fast can a forest spread after a climatic or other change has rendered the country habitable? And also: Can large-fruited trees, like the oak, cross a strait or arm of the sea? The oak has a large, soft fruit; if fruit of this character can be carried considerable distances by birds, then it may be possible to account for the whole of our existing flora, without any land-connection with the continent since the passing away of the cold of the Glacial Epoch.

CLEMENT REID.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

DR. HENRY MCCOOK (3100, *Chestnut Street, Philadelphia, U.S.A.*) desires the address of Mr. Stillbers, of London, who is reported to have successfully reared spiders for purposes of silk-culture. His address or references to any published account of his experiments will greatly oblige.

ERRATA.

OWING to the negligence of a clerk, the corrected proofs of Messrs. Jukes-Browne and Cunningham's contributions last month were overlooked The two last paragraphs in Mr. Jukes-Browne's article on Oceans and Continents were transposed; and Mr. Cunningham is not responsible for the erroneous statement (in his review of Romanes' "Before and After Darwin," p. 544) that "Haeckel never used the name *Prophysema*." The Editor regrets the mishap and tenders apologies.

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NOTES AND COMMENTS.

Tennyson.

AMID the numerous criticisms of Tennyson's work, there is one aspect which does not seem to have received the recognition it deserves, and which forbids that his recent death should be here passed unnoticed. It is that which has placed him as one of the foremost of English Nature-poets. Tennyson's Nature-touches are well worthy of study, not only for their own intrinsic beauty, but because they are in many ways typical of the limitations, as well as of the strength, of his genius. Other poets may have surpassed him in their depth of insight, or their originality of thought; but as a close and accurate observer of Nature, and as an interpreter of her charms in exquisitely-melodious English, Tennyson stands unrivalled. His works are crowded with allusions to the phenomena of Natural Science, and it is little exaggeration to say that each of them is absolutely perfect. Take, for example, his famous picture of the birth of the dragon fly in "Two Voices":—

> "To-day I saw the dragon-fly
> Come from the wells where he did lie.
> An inner impulse rent the veil
> Of his old husk: from head to tail
> Came out clear plates of sapphire mail.
> He dried his wings: like gauze they grew;
> Thro' crofts and pastures wet with dew
> A living flash of light he flew."

His allusions to birds, especially of those of "the level waste" of his early home on the Lincolnshire coast, show his close familiarity with their cries and habits: his rarer references to "scarped cliff and quarried stone," and to "Oh, Earth! what changes thou hast seen," show his acquaintance with the principles of geology. It is, however, upon his botanical allusions that his fame as a naturalist will

mainly depend, and it is here difficult to select from the crowd of splendid images that rise to our memory. Tennyson's range of botanical observation was wide. He noticed the broad features in the vegetation of a whole hill-side, as how the olive groves "turn all hoary to the wind"; he studied the seasonal variations of a single tree, as of the yew, whose "gloom was kindled at the tips, and passeth into gloom again"; or in the minute coloration of a single flower, as of the "horse chestnut's hectic flush." In our national galaxy of poets we have had probably an unusually large share of Nature-poets, but among them all Tennyson is unique in the closeness of his observation united with the perfect polish of his language. Pope, probably, comes nearest to him in the elaborate care with which his images have been finished, but the classical influences that have moulded the style of the "Essay on Man," prevent its vying in popularity with the purer English of "In Memoriam." Tennyson, again, was as much Wordsworth's superior in the simple beauty and ease of his language, as he was his inferior in his breadth of view and the depth of his philosophy. Both poets were keen observers of Nature, but whereas Wordsworth was a philosopher, Tennyson was —at least in his allusions to Nature—a miniature painter with a consummate mastery of the technique of his art. Hence, while Wordsworth has left a deep and permanent impression on human thought, Tennyson has bequeathed to us a gallery of artistic gems. A comparison of the late Laureate with Goethe is still less in his favour; for whereas many of Goethe's Nature-touches are fully equal to Tennyson's in beauty of thought and expression, the latter has left us none of the important morphological generalisations by which the great German did so much to advance the progress of botanical science.

Tennyson, in fact, was not a great thinker, and the estimate that accepted him as such has long since been discredited: his work is valuable rather as the expression of one popular school of contemporary thought; his poetry will thus be of the highest service to the future historian of the intellectual development of the Victorian era. As Burke said, "the popular orator is the man who is a little, but only a little, in advance of his time." Tennyson's popularity may demonstrate that this maxim applies also to poets. Tennyson was carried away by the wave of scientific progress of the early part of our era; and as he tells us he once nourished

> "A youth sublime,
> With the fairy tales of science, and the long result of Time."

And there were occasions when Tennyson seemed to have grasped the most advanced teaching of his time: the pantheistic conception of the unity of life was never better expressed, even by Goethe, than in Tennyson's "flower in a crannied wall":—

> "Little flower—but if I could understand
> What you are, root and all, and all in all,
> I should know what God and man "

But it was apparently only "the fairy tales," the sensational conclusions and not the methods of scientific thought, that fascinated him; and when

> "Half the marvels of my morning, triumphs over time and space
> Staled by frequence, shrunk by usage, into commonest commonplace."

the fairy tales lost their charm. Hence his enthusiasm for science soon vanished. "Science grows but beauty dwindles," he laments in one place: "the sun and moon of our science are both of them turned into blood," he protests in another. The astronomy of his early years had resolved the Milky Way, examined nebulæ, and discovered new planets and satellites: these results could appeal to Tennyson's imagination, and he would with pleasure watch

> "the Pleiads, rising thro' the mellow shade,
> Glitter like a swarm of fire-flies tangled in a silver braid."

The astronomy of his old age compelled the light of the stars to deliver their message and tell us of the chemical composition of the heavenly bodies: but it did not interest Tennyson to know that tellurium had been discovered in the stars, for he simply "knew that their light was a lie."

With his loss of interest in the results of scientific research, Tennyson grew intolerant of the school of thought it had established. The man who 50 years before gloried that he was "the heir of all the ages in the foremost files of time," who then cried his loudest

> "Forward, forward, let us range,
> Let the great world spin for ever down the ringing grooves of change,"

now confessed that this was all "the babble of a foolish past,"

> "Fires that shook me once but now to silent ashes fall'n away."

Like Newman, who welcomed the night because he held the daylight "garish," Tennyson felt that scientific progress had only led us—

> "From a cheerless night to the glare of a drearier day,"

and the author of "In Memoriam" could now draw the spiteful sketch of "Edgar" in the "Promise of May," and the burlesque of the monomaniac of "Despair."

Hence the naturalists of the younger generation had sadly learnt to realise that we could expect no more the manly enthusiasm of Tennyson's younger days, and that we must trust "for songs to encourage us not from his lyre." Nevertheless, his poems have always been looked for with pleasure, for even amid the most petulant of his latest lamentations there was sure to be some splendid image, some polished couplet, some echo of the "curlew's call," to remind us that Tennyson was still a naturalist as well as a poet. Hence regret that he did not always "sweep into the younger day" is finally lost in gratitude for the echoes of the birds' songs, the pictures of the flowers, and all the exquisite Nature-touches which he has left to help us "to smell the meadow in the street."

The Crisis in the United States Geological Survey.

Foremost among the news of scientific movements this month must be placed a serious announcement from America. All European students of geology will learn with deep regret that Congress has this year refused the United States Geological Survey its usual appropriation, and that all field work has therefore been suspended, while the members of the staff have been ordered so to finish off their reports that they may either be published or kept in manuscript. As the news first reached England, it seemed as if the final winding up of the Survey were contemplated, but this, there is reason to hope, is not the case. The problems of the geology of the United States are of such wide interest, and they have been handled by the official surveyors with such originality and breadth of view as to have exercised great influence on contemporary geological thought; a complete suspension of the Survey would, therefore, be nothing less than a scientific catastrophe. The study of American geology is attended with such difficulties arising from the vastness of the distances to be traversed, and the unsettled state of the country, that any detailed work is beyond the power of single individuals, and only a great national department could hope to surmount the obstacles. The Monographs and Bulletins issued by the Survey have, moreover, generally been regarded as the finest series of geological works ever published, and the generosity with which they have been distributed to foreign geologists is frequently quoted as an example to European Governments. On this side of the Atlantic it has, therefore, been generally felt that, while many of the United States Departments, such as the Bureau of Statistics, have much to learn before they reach the European standard, the Geological Survey was an institution of which the citizens of the United States might be justly proud.

Bitter attacks have been made on the Survey, such as that which caused a painful sensation in Europe three years ago; but the most venomous shafts of this were obviously inserted by the editors of the papers in which the controversy raged. After deducting the bluff about "hurling azoic facts" and "picking up palæontological gauntlets," the general impression left was simply that two famous palæontologists could not agree as to whether the skull of a certain fossil ought to have been mounted on the end of its neck or on the end of its tail. The speech of Secretary Noble at the opening of the International Geological Congress at Washington last year encouraged the hope that this quarrel had not damaged the credit of the Survey; though we fear it must have had some effect, since the section of Vertebrate Palæontology is no longer to be officially recognised. The Chief Secretary stated that the United States Government fully realised the immense national value of the work of the Survey, and that, large as was the annual appropriation for its service, no grant was voted more readily. We may, therefore, fairly

hope that the recent emphatic contradiction to the statement of the Secretary of the Interior implies no alteration in the recognition of the necessity of a State Survey, but only doubts as to the efficiency of the present Administration.

A national Geological Survey is of course merely supported as a commercial investment; there are no State-supported botanical or zoological surveys, except in so far as forestry and fishery boards may be regarded as such. A nation is, therefore, justified in asking, not how far questions of abstract speculation have been advanced, but as to what progress has been made in the preparation of the maps, for upon these so largely depends the economical working of mines, the development of mineral wealth of newly-settled districts, the determination of the most profitable routes for roads and railways, and the establishment of the best water supply. It is generally recognised, at least on this side of the Atlantic, that the fundamental duty of a Survey is to survey, and that the progress in mapping is the best guide by which those responsible for the expenditure of the public funds can determine whether the nation is getting the best return for its money. Judged by this standard, even the most friendly critic of the United States Geological Survey must admit that it has not done as much as might have been expected. Many most valuable maps have been issued on special subjects, such as those in the Atlases to Emmons' monograph on "The Ore-Deposits of Leadville," to Dutton's "Cañon of the Colorado," and to Hayden's Rocky Mountain Survey, and also the smaller ones scattered through separate monographs, such as in Gilbert's Henry Mts. and Lake Bonneville. Of the general map, however, only one sheet appears to have been issued. This covers the very limited District of Columbia, and it is very doubtful if the scale of coloration could be widely applied. The illustrations which accompany the publications have been more successful, and these are unsurpassed for their educational value and excellence of execution. One cannot, however, but feel that they have sometimes been extravagantly used for subjects of only secondary geological interest. While we have been left uninformed on many points of great interest, such as the Snake River lava flows, we have received such a plethora of illustrations of Pleistocene scenery and Dismal Swamps, that one has been tempted to exclaim, "This is magnificent, but it is not geology."

The Ethnographical branch of the U.S. Survey has also absorbed a rather large share of attention. It is no doubt very pleasing to read the story of "How the black rabbit caught the sun," not only in English but in Cehina; but it strikes the ordinary Briton as rather a curious arrangement for ponderous monographs on the Cehina language to be issued by a Geological Survey, for the language is not even extinct yet. The charge has recently been made against the present Director of the Survey that he has systematically favoured the work of the Ethnological branch, in which he is most interested, at

the expense of the Geological. The pamphlet in which this accusation was made was marked throughout by such bitterness and manifest unfairness, that we should not be disposed to attach much weight to that alone; but we have heard the same view expressed in Europe. So far as Major Powell's writings allow us to judge, he is a daring explorer, and an ardent ethnologist. Both his predecessors, Clarence King and Hayden, made important contributions to geological literature, but the writings of the present Director have not been sufficient to inspire any great faith in his capacity as Director of so difficult an undertaking as the Geological Survey of the United States. There are on the staff men whose names are household words in modern geology, and if, as a result of the forthcoming State enquiry, Major Powell should be retained as head of the departments of Ethnology and Irrigation, and the Geological Survey be separated as a distinct institution, with such a geologist as Dr. G. K. Gilbert at its head, the recent action of the Congress will result in ultimate good to science.

The Australian Barrier Reef.

Since the days when Flinders described the great Australian Barrier Reef in the second volume of his "Voyage to Terra Australis," and King prepared his oft-quoted Admiralty Chart, the reef has always occupied a prominent place in the literature of the Coral question. Owing to its vast extent—ranging 1,200 miles in length—it forms by far the most conspicuous of reefs, ancient or modern. The great size of the lagoon channel, which sometimes amounts to 70 miles in width and 60 fathoms in depth, further gives it an especial interest in connection with the subsidence theory, by which Darwin explained its method of formation; the theory being supported not only by the depth of the lagoon, the great height of its steep seaward face, and the fact that the open ocean beyond is interrupted only by a few small atolls. Nevertheless, in spite of the interest of the reef, comparatively little has been done upon it, mainly, no doubt, owing to its vast size. This is especially to be regretted, as a knowledge of the structure of existing barrier reefs is of primary importance to geologists; for probably most of the fossil reefs, such as those of the Corallian rocks of England, belonged to this class. Moreover, the fascinating problems connected with Darwin's theory have, to a large extent, monopolised the attention of students who have had the opportunity of working on coral reefs, and thus our knowledge of the biological questions connected with them has flagged. The pools and lagoons in coral reefs are exceptionally prolific in life, both animal and vegetable, and offer a most tempting field for work; but, with some few exceptions, such as Dr. Hickson's sojourn on the reefs of Celebes, and Walther's study of those of the Red Sea, this has been neglected.

The recent proposal for the establishment of a biological laboratory beside the reefs of Barbadoes seems to make little progress towards realisation. The announcement that a large monograph upon the Australian Barrier Reef is now in the press is, therefore, very welcome, especially when we know that it comes from the pen of so experienced a zoologist as Mr. Saville Kent.[1] As the fishery inspector of several of the Australian colonies, Mr. Saville Kent has had ample opportunities to make himself acquainted with the marine fauna of that continent; and during three years' investigation for the Queensland Government of the fisheries on the reefs, he has had exceptional facilities for the study of its fauna and the mode of its formation. Mr. Saville Kent is well-known to zoologists as an authority on the Protozoa, Sponges, and Cœlenterata, and we may, therefore, hope to learn much upon each of these groups. As the author has, however, been engaged upon official enquiries, we must expect that a large proportion of the volume will be devoted to economic questions, such as the development of the fisheries. These are extremely varied and valuable, including fish, oysters, pearls, mother-of-pearl, trepang, sponges, dugong, and turtle.

The work will be illustrated by 16 coloured quarto plates and 48 plates in photomezzotype. By the courtesy of the publishers we have been allowed to see these, and to reproduce one as an example. The coloured plates have all been copied from paintings from nature by the author, so that their accuracy may be relied on. They illustrate the fauna of the reefs, and those showing the corals in the natural colours of life will be of especial value. Probably, however, the most important of the plates are the reproductions of photographs of the reef grounds, showing not only great areas exposed at low tide, but also submerged parts of the reefs. Among these, we may especially remark the plate of Skull Reef, showing a wide expanse of coral grounds, and one of Jelly Mould Reef, showing a great diversity of coral genera, including the branching *Madrepora*, the globular masses of *Porites*, and the large clumps of various Astreans. Another plate shows a fine series of typical genera, among which we recognise *Mussa*, *Agaricia*, *Fungia*, *Galaxea*, and *Madrepora*.

One of the most interesting plates to geologists is that giving a view of an inshore reef when living, and one when eroded. The destruction of the specimens of *Madrepora*, *Porites*, and *Millepora*, and the persistence of the rounded banks and clumps of the Astreans is very striking.

Photographs of living coral reefs have been made before; one taken by the Hon. R. Abercromby, at Feejee, is well-known through the enlarged copy in the coral gallery in the British (Natural History) Museum. Mr. Saville Kent's photographs are, however,

[1] The Great Barrier Reef of Australia; its Products and Potentialities. By W. Saville Kent, F.L.S. Announced by Messrs. W. H. Allen & Co.

far superior to any we have previously seen, both as works of art, and from the varied nature of the views. They enable us to realise how the scenery of a reef alters with the nature of the genera that formed it. Thus one shows us the shrubby aspect of reefs in which *Madrepora* preponderates, in contrast to the flat, smooth terraces of some species of *Millepora*, or to the hummocky, mound-studded expanses of reefs formed by *Goniastræa*, *Diploria*, and other Pseudothecalia. The photographs must all have been taken before the days of telescopic photography, and the views across wide tracks of level reefs therefore suffer by the distortion of distance. But it is marvellous that, with such difficult subjects, the views are as successful as they are; the genera can usually be recognised, while the general effect is as beautiful as it is instructive. Mr. Saville Kent must be as good a photographer as he is a zoologist, and whatever may be our judgment of the book when it appears, all students of Corals and Coral questions will be grateful to the author for these splendid representations of coral life and growth.

Biological Stations.

It is gratifying to announce remarkable progress of late in the foundation and extension of Biological Stations. We learn from the *Feuille des Jeunes Naturalistes*, of September 1, that Professor Sabatier has now made all arrangements for the erection of the new buildings of the Station established eleven years ago at Cette, on the Mediterranean coast, in connection with the University of Montpellier. The requisite sum of money has been collected, and the erection of the laboratory is now far advanced. The situation is especially favourable for the study of all kinds of marine life, both animal and vegetable, and many important researches have already been carried on in the temporary institution. Another Marine Biological Station has lately been established by private munificence, and by donations from learned societies, at Bergen, on the coast of Norway. This will provide accommodation for ten naturalists, and will be under the direction of Dr. J. Brunchorst. Arrangements are also in progress for the foundation of a small station at Cumbrae, on the west coast of Scotland, with special facilities for the investigation of the marine algæ. In India, too, there is shortly to be a new Biological Laboratory at the Calcutta Zoological Gardens, this with the special object of facilitating researches on snake-poison.

The Anti-Vivisection Challenge.

Apart from the exhibition of beautiful groups of marine life in the Aquaria, the general public manifest but little interest in the ordinary Biological Stations. It is only when a Biological Laboratory is the scene of physiological experiments on the higher animals, that public attention is particularly attracted to it; and this not from any

interest in the results of scientific research, but on account of the misplaced sentiment of a certain band of agitators termed the Anti-Vivisectionists. The British newspapers during the last month have devoted much space to the questions raised by these agitators, in reference to a discussion inaugurated by Sir Andrew Clark and Professor Victor Horsley at the recent meeting of the Church Congress. The result, however, is far from edifying, both parties displaying an animosity that is to be regretted in such a discussion; and such heated controversy will doubtless continue so long as the Anti-Vivisectionists rouse the ire of the Physiologists, not merely by their display of ignorance of the subject in dispute, but also by their tissue of mis-statements and misquotations.

It appears that Sir Andrew Clark and Sir James Paget are too busy to enter into a public correspondence as to whether vivisection has ever done any good to anybody. Certain daily papers seem to regard this as a surprising confession of the weakness of the case for vivisection. The writers seem quite unable to realise the medical view of such a request; they might have understood the preposterous nature of the challenge if the President of the Geological Society and the Director-General of the Geological Survey had been asked to show that the use of the hammer had ever furthered geological science, or if the Presidents of the Institute of Journalists and Incorporated Society of Authors had been invited to discuss whether the world owed anything to the use of the pen. How do these journalists imagine the Church Congress would receive an invitation to consider whether even the slightest good had ever been wrought by prayer books? Besides, the discussion of medical details in the columns of the public press would be dangerous; for what with the British Matron and the County Council, the medical contributors to the controversy would certainly get into serious trouble.

Phenology.

Quite apart from the researches conducted in Biological Stations and Laboratories, however, there is much to be done by the ordinary field observer in Natural History, and we note with pleasure another recent effort to systematise some of the researches of local workers. In the *Midland Naturalist* for October, Mr. F. A. Bellamy seeks to enlist the aid of observers in the "science of appearances," or more properly speaking, in the record of the first appearance each year of certain wild flowers, birds, and insects. The object is to study the relations between Natural History and Meteorology, on a plan suggested as long ago as 1875 by the Royal Meteorological Society. This plan, which has been revised from time to time, has been carried on to some extent, but other students are needed who will make observations in accordance with the instructions issued by the Meteorological Society.

The following is a complete list of plants, birds, and insects, of which observations are required :—

Plants.

1.—Hazel (*Corylus avellana*).
2.—Coltsfoot (*Tussilago farfara*).
3.—Wood Anemone (*Anemone nemorosa*).
4.—Blackthorn (*Prunus spinosa*).
5.—Garlic Hedge Mustard (*Sisymbrium alliaria*).
6.—Horse Chestnut (*Æsculus hippocastanum*).
7.—Hawthorn (*Cratægus oxyacantha*).
8.— White Ox-eye (*Chrysanthemum leucanthemum*).
9.—Dog-rose (*Rosa canina*).
10.—Black Knapweed (*Centaurea nigra*).
11.—Harebell (*Campanula rotundifolia*).
12.—Greater Bindweed (*Convolvulus sepium*).
13.—Ivy (*Hedera helix*).

Birds.

1.—Song Thrush (*Turdus musicus*).
2.—Swallow (*Hirundo rustica*).
3.—Cuckoo (*Cuculus canorus*).
4.—Nightingale (*Daulias luscinia*).
5.—Flycatcher (*Muscicapa grisola*).
6.—Swallow (last seen).

Insects.

1.—Honey Bee (*Apis mellifica*).
2.—Wasp (*Vespa vulgaris*).
3.—Small Cabbage White Butterfly (*Pieris rapæ*).
4.—Orange-tip Butterfly (*Anthocharis cardamines*).
5.—Meadow-brown Butterfly (*Epinephile janira*).

Arctic Expeditions.

A PRELIMINARY notice of Lieutenant Peary's Expedition to Greenland has lately appeared in the newspapers, and the results prove to be most valuable and satisfactory. The expedition. it will be remembered, left the United States in July of last year, under the auspices of the Philadelphia Academy of Sciences, wintering in the Arctic regions, with the intention of reaching the North of Greenland this summer over the inland ice. The party successfully passed the winter at McCormick Bay as arranged, and on May 15 of the present year Messrs. Peary and Astrup started on their northern journey, reaching the extreme limit of Greenland in latitude 81·37 early in July. The land on the coast was almost free from snow, and the musk-ox was found to be abundant, while foxes, hares, ptarmigan, and other birds, besides insects and flowering plants, were also met with. The sledge journey of about 1,300 miles was accomplished within ninety days. Besides adding so much to our knowledge of the geography of Greenland, the expedition is said to have brought back the richest collection of specimens of the fauna and flora of the Arctic regions hitherto made by any explorers.

The French Expedition to Jan Mayen and Spitzbergen, organised and despatched last June by the French Minister of Public Instruction, under M. Charles Ribot, has also safely returned. The Alpine flora was studied especially on Jan Mayen, and observations were made on the movement of some of the glaciers of Spitzbergen. A very large collection of fossil plants was obtained and brought back to the Paris Museum of Natural History.

More Newspaper Science.

It is curious to remark how slowly scientific discoveries penetrate the minds of those who are supposed to relate them for the benefit of general readers. The author of an article on the Australian marsupial mole (*Notoryctes typhlops*) in the *Saturday Review* of October 15 refers to the animal as if it were still only vaguely known, observes that there is still no information as to its size, and concludes by expressing the hope that before long we may have "better accounts, and possibly a specimen or two, of this singular animal." Considering that *Notoryctes* was the principal form of life discussed at least at two meetings of the Zoological Society of London last Session, while detailed reports on the anatomy and affinities of the animal have already appeared, the *Review* is somewhat out of date. We may also observe that Professor Stirling has deposited specimens of *Notoryctes* in the British Museum, the Museum of the Royal College of Surgeons of England, and the Museum of Comparative Anatomy at Cambridge, where they are accessible to all naturalists. Typical specimens in the British Museum have also been exhibited for some time in the public galleries.

Ethnologists may expect some valuable and interesting observations in Dr. Flinders Petrie's forthcoming work on Medum, the oldest dated site in Egypt. Dr. Petrie has been excavating round the Pyramid of Medum (Haram-el-Kaddab), the tomb of Senefru, the first king of the fourth dynasty, and has unearthed the temple of the pyramid, still roofed and perfect. We learn from the *Daily Chronicle* that, as regards the inscriptions, the most interesting are undoubtedly some *graffiti* on the walls, one of which certainly goes back to the sixth dynasty (B.C. 3300—3100), and which reads, "Thrice beautiful is the name of King Senefru." The first painted texts are of the period of Thothmes III. (B.C. 1600), and it is conjectured that shortly after B.C. 1500 the temple became buried in the sands, thus escaping the destructive hands of later builders.

The Rev. B. J. Harker (Gilnow Park, Bolton) and Mr. Thomas Harker (Grassington, Yorkshire) appeal for contributions to a fund now being collected for the exploration of the tumuli and ancient earthworks at Grassington. Preliminary excavations have yielded numerous human remains, urns, ornaments, and implements, in part referred by Professor Boyd Dawkins to the Bronze Age; and it is expected that more systematic work will lead to results of great value.

The "Queen's Ostrich," about which we have read so much in the papers, has arrived at the Zoological Gardens in London. It is

a particularly fine male bird, and shows a character which we have never before seen in an ostrich at the Gardens. It has a bright red neck—possibly a sexual adornment. The Ostrich tribe generally (the Emeus and Cassowaries, at any rate) show bright colours in this region only, which is, after all, on account of its great extent, a very favourable situation for the development of sexual colours. Unfortunately, the Ostrich has within the last few days been in a less robust state of health than when it arrived; the black boy who accompanied it from Sierra Leone has been summoned in consultation, and has given his opinion that the Giraffe house, which, in the want of any proper inmates, has been given over to the Ostriches, is too hot even for an Ostrich. We believe that this has been remedied. Her Majesty the Queen has most generously presented the bird to the Zoological Society.

Visitors to the Zoological Gardens should also notice some of the new birds and other animals brought by Mr. Frank Finn from Zanzibar and Mombasa, and presented to the Society. This collection includes at least three species which have never been exhibited before in the Gardens. The most interesting of these is, perhaps, a small Water Hen (*Limnocorax*), of which there are two individuals located in the western aviary. As the object of Mr. Finn's visit to Africa was to collect Oligochæte worms for Mr. Beddard, he is to be congratulated on having been able, without neglecting his proper work, to enrich the Society's Menagerie by so large a number of interesting living animals.

The question of the extinction of the American prehistoric horse is re-opened by Dr. E. L. Trouessart in *Science* for September 30. Among other evidence, Dr. Trouessart discusses the recent discoveries of Tertiary horses in South America, and he concludes that the stories as to the absence of the horse in the New World prior to the landing of the Spaniards and Portuguese are quite correct. The extinction of the horse in America "appears to have coincided with that of the Proboscidians (elephant and mastodon) and can consequently be attributed to the same causes, which are to be sought in the environment of these Herbivora (nourishment, nature of the soil, &c.)."

Professor Victor Hensen, of Kiel, is editing the results of the investigation of the pelagic fauna ("plankton") of the Atlantic Ocean conducted by the expedition under his charge in the year 1889. The work is announced by Messrs. Lipsius and Tischer, of Kiel and Leipzig, and will comprise five large quarto volumes, illustrated by more than 300 plates. Each group of organisms is being treated by a specialist.

In the last part of the *Ann. Sci. Nat. Zool.* (vol. xiii., Nos. 4, 5, 1892), Messrs. Milne Edwards and E. L. Bouvier complete their description of the *Pagurus*-like crustaceans of the deep sea. It is well-known that the submerged mountain-chain extending north and south in the Atlantic—the chain of which the Azores, St. Paul, and Ascension are isolated peaks—forms a division between two distinct faunas on the bed of the ocean; and the difference between the Paguridæ of the two sides proves to be very marked.

Mr. Clement Reid favours us with an interesting note on the "Cheese Snail." This species, *Helix obvoluta*, is often spoken of as a doubtful native of Britain, and in most books it is said to be confined to a small portion of the chalk escarpment on the borders of Hampshire and Sussex. The species is, however, abundant in all suitable localities in Sussex, as far east as the river Arun, and its rarity is due to the rarity of suitable stations. *Helix obvoluta* is extremely particular as to its habitat, needing shade, with a dry calcareous soil and a steep slope. It appears, also, to have exceptionally small facilities for dispersal, for it is almost entirely confined to the few remaining patches of woodland and copse which have never been under cultivation, and is absent from the extensive beech woods planted a century ago. As the occurrence of this typical woodland species seems intimately connected with the survival of patches of the ancient forest, it should be carefully searched for in other counties where suitable conditions occur. In all probability, *Helix obvoluta* is less local than is imagined.

The year 1892 has been a noteworthy one for Lepidopterists, on account of the abundance of the Clouded Yellow Butterfly, *Colias edusa*, in Great Britain. Records of its occurrence in many counties have been published in the entomological journals. This species and its congener, *C. hyale*, are well known to be numerous in Britain at irregular intervals, and the cause of their sudden appearance and disappearance has long been a puzzle. In his work on the British Lepidoptera, now appearing, Mr. C. G. Barrett inclines to the view that pressure of numbers on the continent causes a migration to our shores, and that many of the migrants establish themselves and breed here. The present is said to be the best "edusa year" since 1877.

The discovery of the host of the rare Stylopid insect *Elenchus tenuicornis* is announced in the *Entom. Monthly Mag.* (2), vol. iii., p. 249, by Mr. E. Saunders, who captured near Surbiton a nymph of a small Homopteron (*Liburnia*, sp.) from the ventral region of whose abdomen a male *Elenchus* was in the act of emerging. It has hitherto been believed that *Elenchus*, like most other Stylopidæ, was parasitic on bees, and species of *Bombus* have been thought to be its hosts.

The only other Stylopid known to be parasitic on a homopteron is *Colacina insidiator*, whose host is a Bornean lantern-fly, *Epora subtilis*. In the same number of the journal the Rev. A. E. Eaton gives a new description of *Elenchus*, with figures, from Mr. Saunders' specimen, and from a male taken free by himself. The female is still unknown.

Dr. A. Irving has printed in full, for private distribution, a paper on the Bagshot Beds of Bagshot Heath, read last June before the Geological Society of London, and published in an abridged form in the "Quarterly Journal" of that Society. The paper is a controversial one, and deals with a number of minor points connected with the stratigraphy of the Bagshot area, with special reference to a paper by Mr. H. W. Monckton. Geological combats may be interesting and profitable, but the records of them are hardly suitable for publication in the journal of a learned Society, at any rate in the form of a professedly scientific paper: it is quite sufficient if the fighting is done during the discussion, of which brief notes are printed. In connection with this subject we may recall attention to some remarks made by Professor Judd in his Address to the Geological Society in 1888. He then lamented "that the tendency towards bringing immature and controversial papers before the Society is on the increase"; and he was quite right in urging that authors should bring forward "the result of carefully matured effort," rather than "preliminary notes" to be followed by a series of papers that would, from want of concentration and completeness, necessarily burden the literature.

Dr. Charles Barrois (*Annales de la Société Géologique du Nord*, vol. xx., p. 75) has just published a memoir on the Graptolites of France. His conclusions agree with those of Professor Lapworth and others in Britain on the succession of zones marked by different forms of Graptolites in both Ordovician and Silurian strata. In France, as in this country, the species are preserved most abundantly in carbonaceous shales or slates; but they occur also in calcareous and arenaceous rocks, so that they were widely dispersed irrespective of sedimentary conditions. Their importance, therefore, in correlating formations in distant areas is apparent. Several forms of Graptolites that have been obtained in Britain are at present unknown in France, and those that have been found in the Corbières, the Pyrenees, Ardennes, and Brittany are, as a rule, poorly preserved and difficult of identification. In Languedoc and Normandy, better specimens have been obtained. Dr. Barrois' memoir is most valuable in summarising the present state of knowledge.

Most of the geological formations of Europe are known to have their equivalents in the New World, but it is remarkable that, until quite recently, the Americans have been unable to recognise in their country any representative of our familiar Lias The discovery,

however, has at last been made, and Messrs. J. S. Diller and A. Hyatt (*Bull. Geol. Soc. America*, 1892, pp. 369–412) announce the occurrence of the formation near the summit of the Sierra Nevada, in Plumas Co., California.

In a paper on the Trias of Cannock Chase (read before the Liverpool Geological Society, 1892), Mr. T. Mellard Reade refers to certain "indented pebbles" the depressions in which have usually been attributed to the effects of pressure. He is of opinion that the surfaces of siliceous pebbles at the point of contact in the Triassic rock have been eroded by the solvent power of water.

In reference to M. Carnot's observations on fluoride of lime in fossil bones noticed last month (p. 565), Mr. T. L. Phipson contributes to the *Comptes Rendus* (vol. cxv., p. 474) an observation of some interest. He remarks that many years ago he analysed a piece of fossil wood from Cretaceous sandstone (evidently Wealden) in the Isle of Wight, and found so large a percentage of phosphoric and fluoric acids, that mineralisation appeared to be almost entirely effected by phosphate of lime with a small admixture of the fluoride.

The most detailed account of the Dover Boring hitherto published in any journal has just appeared in the *Annales des Mines* (1892, pp. 227–232). The particulars are given on the authority of Mr. Brady, the Engineer, and they are communicated by M. E. Lorieux.

In the October number of the *Scottish Geographical Magazine* the Prince of Monaco publishes a new chart of the currents of the North Atlantic, presented to the British Association at the Edinburgh meeting this year. Nearly 1,000 floats were used by the Prince in the course of his researches, and of these 227 were returned with particulars of their discovery on various coast-lines. A curious great vortex is recognised in the ocean to the west of the Azores. "Just as in an atmospheric cyclone, there exists in this oceanic vortex a region of calm, where the waters do not follow any regular direction, and when the floats enter this region they remain there often for months or years."

The Times publishes news of Mr. W. M. Conway's Expedition to Mount Godwin Austen and its vicinity in the Hindu Kush, already referred to in Natural Science (March, p. 6). The exploration of the glaciers is being prosecuted with success, and Mr. Conway has reached an elevation of 25,000 feet.

Another expedition has just left England for Eastern Africa under Lieutenants C. H. Villiers and Bennett Stanford of the Horse

Guards. The party intends to proceed by an unknown route from Kismahu to Lake Rudolf, and will probably return across Somaliland. Mr. J. W. Gregory, of the British Museum, accompanies the expedition as naturalist and geologist, by permission of the Trustees of the British Museum, and it is hoped thus to obtain important acquisitions for the National collection.

Mr. William Wilson sends a note from Alford, Aberdeen, N.B., on the phenomenal duration of the flowers of the Broom (*Cytisus scoparius*), which he has observed this season. He says it has been generally a late season for the flowering of plants, and it is usual in such years to find a very meagre flourish upon the Broom. This season, however, the flourish has been exceptionally large. What has still more surprised those acquainted with the habits of the shrub is the very long time during which it has remained in flower. This cannot be due to the cold weather, as in former cold years the bloom has suffered. Mr. Wilson, who claims to have studied the shrub for several years, believes that the bacterioids associated with the roots are the cause of several peculiarities of the plant, and thinks the above peculiarities in the flowering are in some way due to the reaction of the nature of the season on those organisms.

Baron Ferdinand von Mueller contributes a note to the October number of the *Journal of Botany* on the sudden death of Mr. Robert Fitzgerald, F.L.S., which took place on August 13, at his residence, Hunter's Hill, Sydney, in his sixty-second year. The Baron says, "his orchidographic researches, supported by a rare ability in plant-drawing, are beyond all praise. The greater number of Australian orchids are done by him for his superb work, but it is to be lamented that he did not live to complete it." In 1869 he visited Lord Howe's Island with Mr. Charles Moore, "when for the first time the almost entirely endemic vegetable treasures of that isolated spot between New Zealand and Norfolk Island became revealed." He there ascended the "dangerously steep Mount Lidgbird" discovering the 40 ft. high *Dracophyllum fitzgeraldi*. "His observations on the fertilisation and hybridisation of numerous Australian species are highly important." His researches were mainly in New South Wales, though he made a lengthened tour to West Australia to study in the living state its largely unique Orchid flora.

The Narcissus seems to be ousting the Potato in the Scilly Islands, according to the *Gardener's Chronicle* for September 3. The Narcissus crop now takes the precedence of the tuber; many who grew potatoes largely a few years ago now growing none at all. Fields of hundreds of thousands of beautiful Narcissi may often be seen in bloom in February and March. We are told that one grower will have under cultivation this season no less than 20 acres.

In the *Gardener's Chronicle* for September 3rd is a description of a new fruit, raised by Mr. Culverwell, a hybrid between the gooseberry (*Ribes grossularia*) and the black currant (*Ribes nigrum*). As both the plants belong to the same genus, the production of a fruit-bearing hybrid is not a surprise. One who has seen the bush says it "looks like a gooseberry bush with currant-like leaves, and the fruits are like small gooseberries in bunches of two, three, or four on one stalk." The fruits were seedless, of the size of black currants, but coloured like a red gooseberry and beset with fine hairs. The spines so noticeable in the gooseberry were absent. The flavour "partook of that of the gooseberry mixed with that of the black currant, an improvement on both as it seemed to us."

Lieutenant Stuart Wortley, who has been experimenting at the Agricultural College at Downton on the poisonous principle of the Yew, has arrived at a somewhat curious result, which he has communicated to the *Times*, with an account of his experiments. His object was to test a theory which declared the female Yew to be harmless to cattle, the male plant only being poisonous; and the Lieutenant, having chemically analysed the plants, declares that *taxin*, the presumed poisonous principle, is contained chiefly or entirely in the male plant. We may accordingly grow the female in our parks and pastures with no fear of the results to our cattle, but, as the *Gardener's Chronicle* aptly observes, "how are we to know before flowering which is the male and which the female?" Sometimes, too, the female Yew produces also male flowers; will it then also develop taxin and become poisonous? M. Ch. Cornevin, in his *Plantes Vénéneuses*, published in 1887, mentions no difference between the sexes, but says that the young leaves are comparatively harmless, while as they grow older and deeper in colour they become more and more poisonous.

In *Zoe* for last July, Mr. Wm. W. Price tells of his discovery of a locality for the gigantic red-wood trees (*Sequoia gigantea*) further north than any hitherto known. In Placer County, fifteen miles east of Forest Hill, on one of the streams flowing into the middle fork of the American River, at an altitude of about 5,000 feet, he found a small grove of six trees occupying about an acre of ground. Though unrecorded by naturalists, it is not unknown to prospectors, as many dates were found cut in the bark of neighbouring alder trees. The two largest sequoias are about 12 ft. in diameter, but a fallen trunk measured 20 ft. across. The position is a very sheltered one, shut in by the sides of a small, thickly wooded cañon, a fact which may account for the presence of these stragglers so far north. So secluded is the spot, that the trees cannot be seen till within a hundred yards, and Mr. Price was only enabled to find it by the help of a miner from

the almost deserted camp of "Last Chance," about eight miles from the Grove.

Messrs. Cassell & Co. on October 12 issued the first number of a new weekly periodical entitled *Cottage Gardening*. The journal, which will also deal with poultry, bees, allotments, simple cookery and housekeeping, is edited by Mr. W. Robinson, author of "The English Flower Garden." The price is one halfpenny. The present issue consists of eight pages, all readable matter, there being no advertisements. The front page is occupied by a short eulogy on the English cottage, illustrated by an engraving from a picture by Alfred Parsons. The original is evidently a very charming little picture, but the reproduction is, unfortunately, somewhat of a smear. There are four other illustrations, the best representing a potato known as the white elephant, the others rather poor, that on the back page, of the cottager's pig, a "sow of the large white breed," supported by a gate and a bank of earth, being very black and white. The letter-press contains several pages of useful hints, such as advice on planting bulbs, rose cuttings, the best crysanthemums for October bloom, the extermination of the gooseberry caterpillar, window boxes in winter, the raising of early tulips for room decoration. Instructions are given for the laying-out, trenching, and manuring of allotments, and there are also some notes on poultry keeping, bees in cottage gardens, and the pig. A page is devoted to domestic affairs, and there is, of course, the inevitable prize competition. Answers to correspondents will be a feature, and the journal will doubtless prove a boon to the class for which it is intended.

We have recently been favoured with a glimpse of parts vi. and vii. (Spongozoa and Rhizopoda) of the "Museum Normanianum; or A Catalogue of the Invertebrata of Europe, and the Arctic and North Atlantic Oceans, which are contained in the collection of the Rev. Canon A. M. Norman, M.A., D.C.L., F.R.S., F.L.S., &c. Printed for Private Distribution. Durham [printed], 1892." Canon Norman's idea of printing a catalogue is excellent, for so many type-specimens are contained in his collection, and we hope other large collectors will follow his example. But why not issue the Catalogue for sale? The fact of its being *privately* distributed renders it of little value, since those most in want of it often remain quite ignorant of its existence. We notice, moreover, that Canon Norman has proposed for certain species some new names, which we forbear to quote; they cannot possibly be considered as published, and if they become entered in lists will be productive of much inconvenience. It is bad enough to propose names in indexes, footnotes, or other obscure places, but when this is done in privately-printed pamphlets, the nuisance is intolerable.

I.

The Evolution of Consciousness.

I PROPOSE to set before the readers of NATURAL SCIENCE, in as brief and simple a form as possible, an hypothesis concerning the evolution of consciousness which appears to me to be the legitimate outcome of enquiries which are strictly scientific. I use the word consciousness in its broadest and most comprehensive sense, as embracing all that belongs to feeling, sentience, or the psychical aspect of existence.

Let us take our stand at starting on the firm foundation of common observation. I hold in my hand an object, not unfamiliar to some naturalists, a briar-wood pipe. I touch it, see it, taste it, smell it, and hear its pleasant gurgle as I draw the smoke into my mouth. If anything in the world is real to me, that pipe is real. I shall take it as my standard of real existence. Now in what does this real existence consist? I may state the answer to this question in two forms, both of which are different expressions of the same fact:— (1) An object in consciousness exists. (2) There is a consciousness of the object. These are obviously only different ways of putting the same fundamental fact of experience. But in the first, prominence is given to the objective side of the fact, while in the second prominence is given to its conscious or subjective side. Physical science takes up the objective side for special study and investigation. It explains the object-in-consciousness which is for direct observation a pipe in terms of other objects-in-consciousness, molecular vibrations, modes of energy, and so forth. It thus reaches what Dr. Johnstone Stoney calls the diacrinominal hypothesis, that all objective phenomena are constituted by motions or groups of motions; or some other such physical explanation of the objective world. *But* it never shakes itself free of consciousness. It explains this object-in-consciousness in terms of other objects-in-consciousness. It can do no more.

Let us here digress for a moment, that we may clearly grasp what is meant by the words "object" and "objective." They are used in two closely-connected and yet somewhat different senses. In what may be called the narrower sense, the word "objective" applies to all that belongs to the primitive perceptual object-in-consciousness as contrasted with that which belongs to the consciousness-of-the-object. Thus all that we know, or can know, concerning my pipe, as regarded by physical science, is objective knowledge, as contrasted with all that we know, or can know, concerning my impression or idea

of the pipe as a mental product: the latter belonging to the subjective sphere. In the more extended sense an object is that which is in the focus of the mind's eye. My pipe is an object of perception, in the focus of the organic visual sense. Molecular vibrations are objects of conception in the focus of the eye of thought. Both are objects-in-consciousness, and both are equally real. Physical science, then, takes up the objective side (in the narrower sense) of the fundamental fact of experience, and endeavours to explain it in terms of other objects-in-consciousness, such as modes of energy.

Psychology, on the other hand, takes up the subjective side of the basal fact of experience, namely, the consciousness-of-the-object. It explains the existence of the pipe in terms of perceptions, impressions and ideas, sensations, and so forth. It may thus reach the conclusion that my pipe is what I have elsewhere termed a "construct," and that the mind has, at the bidding of an external stimulus, the power of building up the results of previous experience into that particular object-in-consciousness. *But* it can never shake consciousness free of the object-in-consciousness. Inseparably given in the simplest observation of daily life, they remain inseparably united throughout the whole range of knowable existence.

This may be enforced by an analogy which is somewhat more than an analogy. Let consciousness be represented by the visual sense, and the object-in-consciousness by the light waves which stimulate it. For clear vision, there are required not only a highly-developed organ of vision, but also a well-illuminated object in the focus of vision; and for any vision at all, however elementary vague or dull, *both* factors must co-operate. There must be some sort of visual sense, however rudimentary, and some sort of light stimulation, however indefinite. So, too, for consciousness there must be some form of sentience, however rudimentary, and some sort of object, however indefinite. Consciousness without an object is a metaphysical abstraction. And psychology, when it takes up the subject-side of the fundamental fact of experience, and endeavours to explain it in terms of those objects-in-consciousness which we call perceptions, sensations, and the like, does not attempt to tell us anything about consciousness out of relation to objects.

Now there are certain people called metaphysicians, who try to get beyond the bounds of this inevitable relationship. On the one hand there are some who say that objective matter or energy exists as such independently of consciousness. On the other hand, there are others who say that subjective consciousness or mind-stuff has this independent existence; and the exasperatingly confusing thing is, that these people must needs apply the term *real* in a new sense to these most unreal of all abstractions. Dr. Johnstone Stoney has suggested in place of the word *real* in this unnatural sense the word *autic;* and it would be a great blessing if his suggestion were generally adopted. Autic existences are called noumena as opposed to

phenomena. Noumena are, therefore, existences out of that inevitable relation which is given in fundamental experience; and as I know nothing whatever about noumena or autic existences, I leave them without further ado to those who pretend to such knowledge.

I deal entirely with phenomena, but with phenomena in their double aspect—objective and subjective. Let me represent in tabular form the distinction between the two :—

By Psychology.		By Physical Science.
Explained as the consciousness of an object in terms of those subjective phenomena we term sensations and perceptions, &c.	Pipe	Explained as an object in consciousness in terms of those objective phenomena we term matter and energy, &c.

I have before contended in these pages that it is part of the very grammar of science to keep these two aspects distinct, and I see no cause to abandon this position.

Let it be clearly understood, then, that in suggesting an hypothesis with regard to the evolution of consciousness, I am dealing with the conscious aspect of phenomena as given in practical experience, and am not attempting to get behind phenomena, or to shake myself free of a relationship which I hold to be inevitable.

Now the pity of it is that I cannot, as the world is now constituted, get directly at the conscious aspect of phenomena in any other being or thing than myself. It is a dreadful drawback; but there is no use whining about it; one must make the best of what one *can* do; and what I, *quâ* man, can do is this. I can study with all possible patience and care the association of conscious and physical phenomena in beings which, though not me, are, at any rate, uncommonly like me, and which, though they cannot directly show me their consciousness, can, at least, in various ways assure me that they are conscious. I do not pretend that the results of this study are of assured and incontrovertible validity; but I do think that we have some grounds for assuming as a working hypothesis that consciousness is associated in me and my like with certain complex modes of energy in my brain or some part of me. If this be so we get the possibility of a correlation which may be thus expressed in tabular form :—

Psychical Aspect.		Physical Aspect.
Presenting to Subjective Study certain phenomena we term States of Consciousness.	Man.	Presenting to Objective Study certain phenomena we term Neural Modes of Energy.

Or, by means of a very rough diagram, it may be put thus :—

YOU. ME. THE WORLD.

The small circles represent you and me, or that part of us which is concerned in sentience and thought. The white half of me repre-

sents my consciousness in relation, directly or indirectly, with the great world of objective phenomena. But if you could get at my brain—my black or objective side—you would find that while I was experiencing sentience and thought, you were observing modes of nervous energy ; and if we then compared notes we should find that the consciousness and the neural energy varied, not independently, but in strict correlation. This may be due either (1) to an invariable association of states of consciousness with neural tremors (dualism) ; or (2) to the fact that neural tremors and states of consciousness are, so to speak, the different ways in which the same occurrences present themselves to you and to me at the supposed moment of observation (monism). For my present purpose it matters not which view is adopted.

Let us first consider the modes of energy—my black side. I assume that they have been evolved. But evolved from what ? They certainly were not present as such in the fertilised egg-cell from which I have been developed. From what then have they been evolved ? The answer given by evolutionists is : From the simpler modes of energy of that fertilised egg-cell. And from what has this, in common with all that is organic, been evolved? Well this takes us a long way back, and the answer is little more than either a guess or a pious (perhaps some would say impious) belief; but from the standpoint of thorough-paced evolution organic modes of energy have been evolved, somehow and somewhere, from inorganic modes of energy. That is the only suggestion natural science, as such, has to make in the matter. From the objective aspect of the evolutionary process, therefore, neural modes of energy have been evolved from *infra-neural* modes of energy of decreasing order of complexity till we reach the simpler modes of energy of inorganic nature.

Now let us turn to the states of consciousness—my white side. I assume that they too have been evolved. But evolved from what ? They presumably were not present as such in the fertilised egg-cell from which I have been developed. Well, then, from what have they been evolved ? I do not pretend to do more than make a guess, but I try to guess on scientific lines. I suggest that, just as the neural modes of energy have been evolved from infra-neural modes of energy, so have states of consciousness been evolved from *infra-conscious* states ; and just as the infra-neural modes of energy stretch back (on the evolution hypothesis) through decreasing orders of complexity till we reach the simple modes of energy of inorganic nature, so, I suggest, do the infra-conscious states, which are associated with the infra-neural modes of energy, stretch back through decreasing orders of complexity till we reach the simple infra-consciousness of inorganic nature ; and further, in reference to the rough diagram of p. 661, I suggest that just as the nervous energy of you and me, which has been evolved from the black or objective side of the world, are associated with states of consciousness or a white side, so, too, does

the whole black world of infra-neural energy have its white or infra-conscious aspect, *if we could only get at it*. The worst of it is that we cannot do so.

Voilà tout! That is the suggestion; speculative if you will, but not as it seems to me unscientific.

A few words in conclusion to present the matter from a somewhat different point of view. Consciousness exists: of that there is no doubt. How did it come to exist? There seem three possible answers to this question. (1) It was directly created in man, or in some lower organism from which man has been evolved. (2) It has been directly evolved from energy. (3) It has been evolved, as I have suggested, from infra-consciousness.

Now the first answer, that of special creation, is, in my opinion, a logically tenable one, and one with which I have sincere sympathy. I do not hold it myself, because it does not seem to me either the highest or the most probable view of the matter; but if others hold it on these grounds, so let it be; and these remarks apply in due degree to the mode of origin of organic nature, and of man. With the second answer, I am in distinct and direct antagonism. I do not think it has a single genuine fact of observation or a single rational inference from observation in its favour. Its supporters may be left to make out a case for it if they can. The third answer is that which I hold and advocate. If, then, these three answers exhaust the logical possibilities of the case, and if the second is inadmissible through default of evidence in its favour, we are left in presence of the first and third. Either special creation or evolution from infra-consciousness, there is no other alternative. *Uterum horum mavis accipe.*

C. Lloyd Morgan.

II.

Primæval Man: A Palæolithic Floor near Dunstable.

ALL persons interested in the subject of the antiquity of man have read Professor Prestwich's recently-published papers in the *Quarterly Journal of the Geological Society* and the *Journal of the Anthropological Institute*[1] on the stone implements found on the chalk plateau of Kent in the Darent district. Palæolithic implements of a similar class have for several years past been found by me in the drift which caps the highest chalk hills of south Bedfordshire and north Hertford-

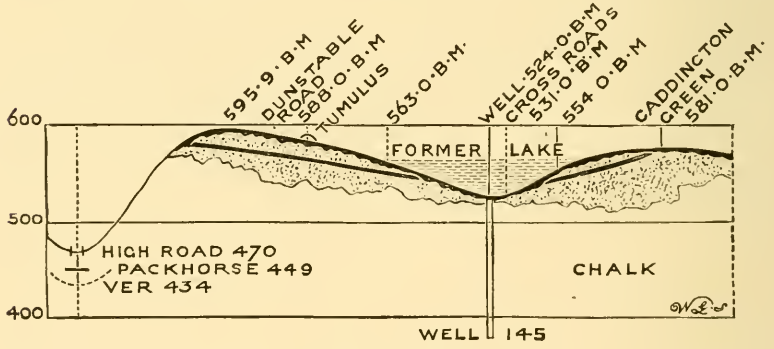


FIG. 1.—Section through Caddington Hill, Dunstable.

shire, and a brief preliminary note from my pen on these discoveries will be found in *Nature*, vol. xl., June 13, 1889, p. 151.

I must say here that the implements I am about to mention are *bonâ fide* implements and works of art. The dubious and disputed forms which have been described as implements of human make from the chalk plateau of Kent occur here also in large numbers, but it is impossible for me to accept them as of undoubted human origin. Excellently made and symmetrical implements occur on the Kentish chalk plateau, and similar implements occur on the hills here, and to such well-made instruments of undoubted human design I alone refer.

The accompanying section (Fig. 1) is taken through one of the hill-tops near Caddington, about two miles to the south-east of Dun-

[1] Noticed in NATURAL SCIENCE, April, 1892, p. 85.

stable, east of the high road, and looking north; the horizontal scale is two-and-a-half inches to one mile, the vertical scale half-an-inch to one hundred feet. The figures show the hills (by no means the highest of the district) to be from 581 ft. to 595 ft. above the Ordnance datum, and from 130 ft. to 146 ft. above the nearest dry chalk valley. The Ver rises as a mere drain a mile or more to the south. "B.M." on the Section means "Bench-mark."

The upper thick black line represents highly tenacious red brown drift clay, which was at one time probably continuous as a flattish stratum over where the dry chalk valleys now exist, and before the valleys were lowered by denudation to their present depth. A similar stratum caps all the hill-tops in the neighbourhood. In this red brown clay, slightly abraded, ochreous, well-made Palæolithic implements and flakes are frequent. Both types of tool occur, pointed and ovate.

Beneath this stiff clay, Tertiary clay, shown by the dotted tint, occurs. This clay rests directly on the chalk rock, shown by the lower thin undulating line.

The Tertiary clay is shown in the illustration as divided by a thick black line, this line representing a Palæolithic land surface, living place, or floor, on Tertiary clay. The clay beneath the black dividing line has never been disturbed, but the clay above the floor line has in very remote times been relaid.

The section proves that the extensive depression still existing on the hill-top was once a lake on the practically impervious Tertiary clay, and that the men who lived on the old clay surface were not true riverside men, but veritable Palæolithic lake-side dwellers.

The stone tools found by me on this ancient living-place were obviously made before the upper mass of water-laid Tertiary clay was deposited, as well as before the time of the deposition of the implementiferous, contorted, deep-brown drift above. The brown drift is, moreover, capped by another ancient drift with abundant black Lower Tertiary pebbles.

The Palæolithic implements found on the floor, unlike those from the drift, are never ochreous, as the Tertiary clay both above and below this floor is not an ochreous matrix. The floor is in some places more than twelve feet below the ochreous implementiferous drift above. Whatever age, then, the Tertiary clay tools may be, they must be older than the movements—whatever those movements were—which first tore up and then relaid with water enormous quantities of Tertiary clay, next deeply ploughed the upper tenacious contorted drift into that clay, and, lastly, deposited a second drift on what are now the highest hill-tops near Dunstable.

A detailed section to a larger scale is shown in Fig. 2. A represents the upper drift with Tertiary pebbles, capped with humus and abundant jet-black lustrous Neolithic implements and flakes. B is tenacious brown drift clay with ochreous Palæolithic implements.

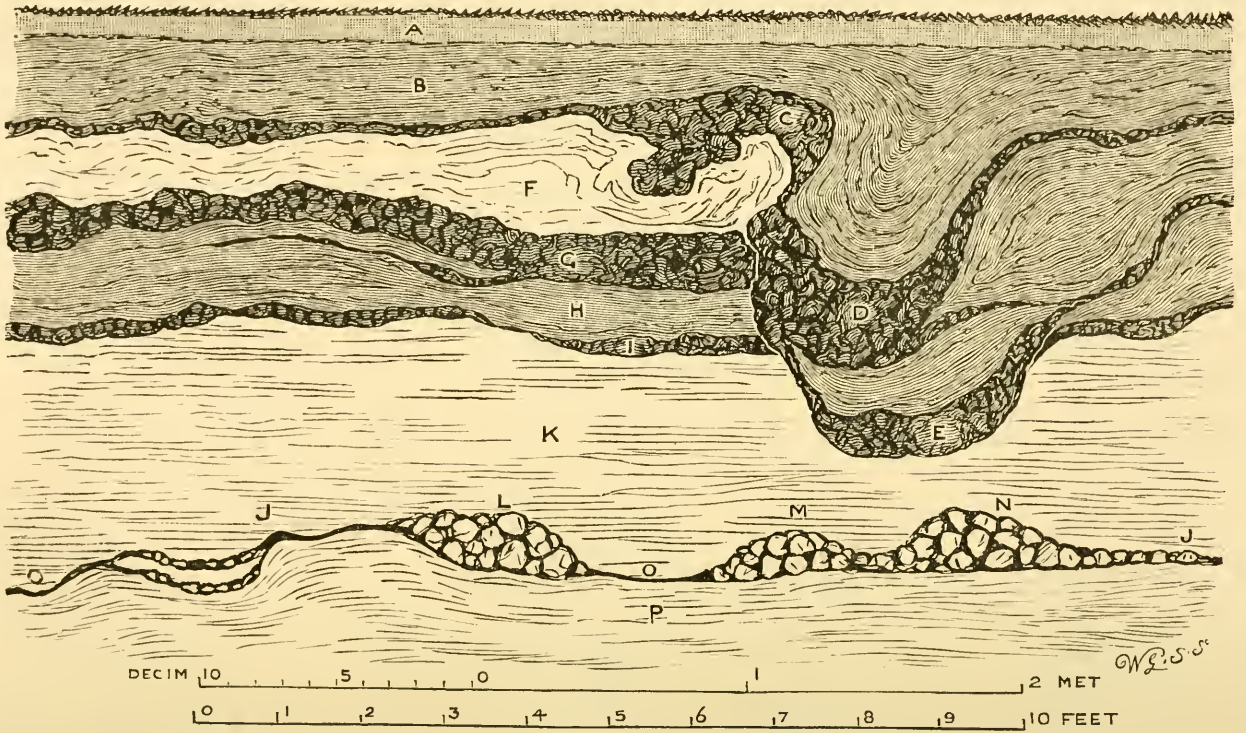


FIG. 2.—Section at Caddington of Palæolithic floor on Tertiary clay, surmounted by relaid Tertiary clay and contorted, implementiferous red drift.

C, D, and E is ochreous gravel containing similar implements; F, whitish clay with whitish Palæolithic implements; G, gravel with lustrous-whitish Palæolithic implements and flakes; some of these flakes have been replaced by me on to the cores from which they were originally struck; I, same as G; J, J, Palæolithic floor on Tertiary clay; K, relaid Tertiary clay covering up the Palæolithic floor; L, M, N, heaps of flints brought by hand in Palæolithic times from the red clay-with-flints, or chalk-with-flints, and placed upon the floor by the margin of the old lake for the purpose of flaking into implements; O, O, Palæolithic floor with sharp-edged implements and flakes; P, Tertiary clay *in situ*. Small pieces of chalk sometimes occur in the cavities of the larger flints, and similar pieces rarely occur upon the floor.

The whole Palæolithic living place on Caddington hill was in very remote times so gently covered with water holding Tertiary clay in suspension that all the objects left on the floor in Palæolithic times may be found in position now, in fact, exactly as they were left by our precursors of old. Flakes of all sizes occur in hundreds, nearly all as keen-edged as knives. The actual places where the men and women sat and worked may be fixed without doubt, as the flakes at certain spots can be replaced on the blocks from which they were originally struck, and even on the finished implements themselves. I have conjoined more than five hundred of these flakes, fitting some on tools and others on cores or other flakes; but few flints of my perhaps unique group-find resemble in colour the lustreless indigo-black flints found and replaced by my friend Mr. F. C. J. Spurrell at Crayford—my examples are white, or beautifully marbled and blotted indigo and white, or like tortoise-shell in colour, and generally lustrous.

It was remarkable to find artificially transported and piled up flint blocks taken from the chalk or red clay-with-flints, but, perhaps, still more remarkable to find, as I did, groups of the larger and better flakes obviously carefully selected and placed in small heaps by themselves. The flakes from these little piles had in some instances been struck from the same block, as they were capable of being replaced upon each other.

It goes without saying that, in a position like the one here described, the whole Palæolithic stock-in-trade—slender enough as it was—must be present on this living place.

The replacement of the flakes will be better understood by reference to the accompanying illustrations. Fig. 3 shows an edge and side view, one-half the actual size, of a finished implement, found at Caddington, and broken in two (probably by use) in Palæolithic times. The two pieces were found on the floor by me about three feet apart, and the two pieces are in a slightly different mineral condition. Fig. 4 is the reverse side of this conjoined implement, showing a large flake re-attached to one edge, a smaller flake

towards the other edge, and a small flake at the base. The thick black lines show the junctions. I have also re-attached a flake to the other side of this tool as shown in Fig. 5; Fig. 6 shows a Palæolithic implement, also from Caddington, in an unfinished or initial condition, one-half the actual size; the basal part is finished, but the upper part was never completed. Near this unfinished tool I found nearly every flake which had been struck from the original block from which the tool was made. Fig. 7 shows one side of the same implement with numerous flakes re-attached, and showing the form

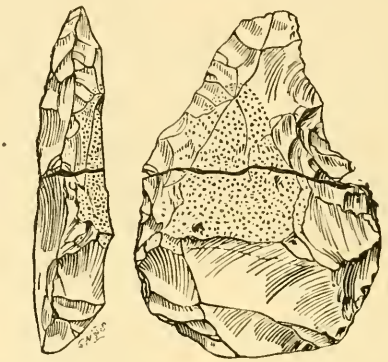


Fig. 3

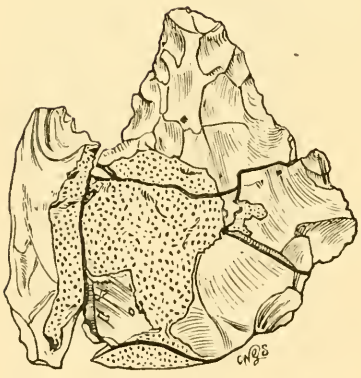


Fig. 4.

Fig. 5.

FIGS. 3—5.—Flint Implement from Caddington, made and broken in Palæolithic times; one-half nat. size. In Figs. 4, 5, some of the original flakes are shown conjoined.

of the original block of flint selected by the man of primæval times for flaking into an implement.

The facts briefly mentioned only indicate a few of the results obtained. I have had the position to myself for several years, and I have measured, drawn, and photographed various sections, and kept everything together with great care. I received permission from the owners of the pits to excavate on my own account, so that the work

done up to the present time has been under my close supervision. A good deal of the digging has been done by myself.

FIG. 6.—Unfinished Palæolithic Implement from Caddington; one-half nat. size.

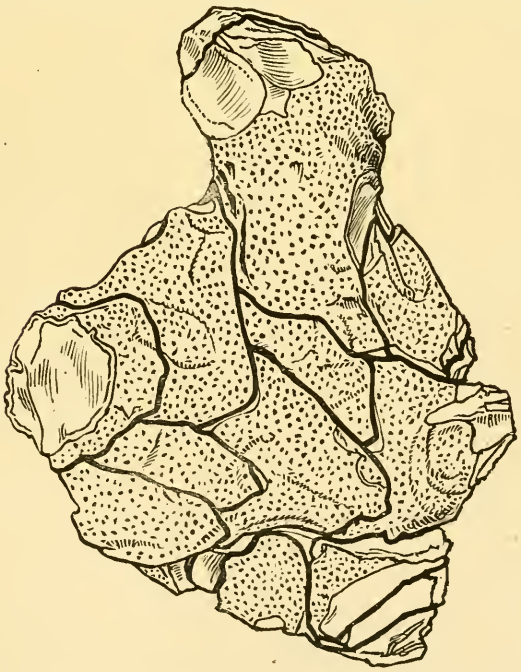


FIG. 7.—The same Implement, with the original flakes re-attached.

No work has been done since last spring, so that the banks of the Caddington pits have since then crumbled in or tumbled down, and

the fallen material is just now thickly overgrown with coltsfoot and other weeds. I hope, however, to be able to resume work soon, and in the meantime I am engraving a map of the district, with a complete set of sections, the chief implements, and the most noteworthy masses, large and small, of conjoined examples. The full account, when published, will throw light on numerous critical points of interest, which will suggest themselves to readers, but which I have not adverted to in this short note.

WORTHINGTON G. SMITH.

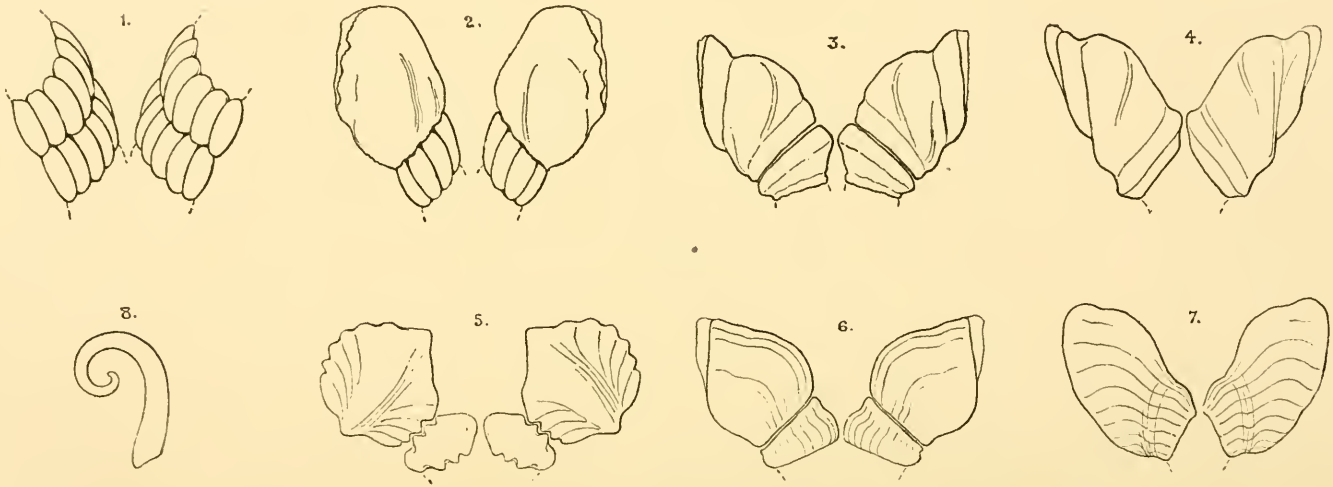
III.

The Evolution of Sharks' Teeth.

SO much has lately been written concerning the development and modes of complication of the teeth in the highest vertebrates, the Mammalia, that it may be of interest to summarise our present knowledge of the phenomena in question, as illustrated by some of the lowest vertebrates, the Elasmobranchs or Sharks and Skates. If the teeth in mammals are frequently complicated and modified by the fusion of adjacent cusps and by their splitting (as now claimed by most competent observers), it is probable that the fundamental laws governing such modifications will produce analogous results under the simplest conditions as exemplified in the dentition of the cartilaginous fishes. In each case the known palæontological history of the various groups of animals is tolerably extensive; and it is thus possible to institute satisfactory comparisons in all essential particulars.

I.—*The Fusion of Adjacent Cusps.*

It is a familiar fact that the calcified tubercles in the skin of numerous Elasmobranchs, both living and extinct, are often fused by their base into little groups, though generally without much constancy or regularity of arrangement. It is thus to be expected that the same complication of the tubercles will occur in the mouth, where their function as teeth may be deemed to specially favour this process. The fusion does indeed take place, and the only peculiar features to be noticed are the constancy, regularity, and symmetrical arrangement of the little groups of cusps or "teeth." In all the most primitive sharks, the inserted base of the cuspidate teeth is a great flattened expansion, exactly like that of each tubercle or group of tubercles in the external skin; and the polished shining cusps are at first always well separated above the base. Such teeth are well-known from Palæozoic rocks under the names of *Diplodus*, *Cladodus*, &c., and it may be remarked that they never consist of more than a single row of points, thus showing that they originate from adjacent cusps developed simultaneously in one and the same series. The relative sizes of the cusps, of course, vary much in the different sharks, as also do their numbers in the respective "teeth"; and in some cases the actual fusion of the cusps well above the base begins to occur, more especially when they assume obtuse proportions.



CRUSHING TEETH OF SHARKS.

Fig. 1. *Cestracion.* Fig. 2. *Psephodus.* Fig. 3. *Cochliodus.* Fig. 4. *Deltoptychius.* Fig. 5. *Pleuroplax.* Fig. 6. *Deltodus.* Fig. 7. *Pœcilodus.* Fig. 8. Section of Cochliodont dental plate, showing in-rolled outer border.

In short, compound cuspidate teeth in sharks are always uniserial, and owe their origin to the constant and regular fusion of primitively separate, adjacent, simple cusps.

II.—*The Fusion of Successional Teeth.*

In accounting for the complication of cuspidate teeth in mammals, however, Prof. W. Kükenthal informs the writer that he is inclined to proceed beyond stating that such teeth result from the fusion of simple adjacent cusps. He even suggests that a successional set of cuspidate teeth may fuse completely with the set of fully-formed teeth already in front—that in such primitive mammals as *Polymastodon*, for instance, each tooth may be a compound of cusps derived from both the first and second sets, which normally develop separately and one after the other.

It is of great interest to note that precisely the same phenomenon can be observed in a certain specialised group of extinct sharks, characteristic of the latter part of the Palæozoic epoch. The family of Cochliodonts, of which little is known except the dentition (1–6, 8), is so named from the great shell-shaped plates into which its principal grinding teeth have more or less completely coalesced; and all circumstances point to the conclusion that these simple plates are the equivalent of several distinct rows of successional teeth in the ordinary sharks with grinding teeth, as represented by the living *Cestracion*.

The Port Jackson Shark, or *Cestracion*, as is well known, exhibits two principal series of grinding teeth on the middle of each half of the jaw (Fig. 1), in which there are five or six successional teeth behind (or within) the foremost row. In *Psephodus* (Fig. 2) the equivalent of the hinder of these two series forms a great simple plate; and this is shown by abnormal specimens (Davis, No. 1., pl. lv., fig. 4) to consist of three rows in succession. In *Pleuroplax* or *Pleurodus* (Fig. 5), *Deltodus* (Fig. 6), and *Cochliodus* (Fig. 3), each of the two series is fused into a continuous plate, which always distinctly exhibits traces of its components in the first two genera. In *Pœcilodus* (Fig. 7) and *Deltoptychius* (Fig. 4), not only do the two plates occur, but they are once more fused together at their adjacent border into one continuous piece. The coalescence of the teeth is so complete in all cases, that their mode of growth is much changed; the once-separate outer tooth can no longer fall away from the mouth, as growth of tooth-substance takes place within, and the external border thus coils inwards like a scroll, as shown in Fig. 8.

Briefly, then, compound crushing dental plates in sharks result not only from the fusion of adjacent teeth, but also often from the coalescence of several successional teeth.

III.—*The Multiplication of Similar Cusps.*

Fusion of cusps and plates, however, will not explain all the

phenomena observed in the process of complication of sharks' teeth. There must be some other means by which cusps can be occasionally added and suppressed. In some modern Lamnidæ, for example, the development of lateral denticles in the teeth is very variable and uncertain ; and the palæontological history of the dentition of the Notidanidæ (7) is especially difficult to comprehend, except on the theory that development sometimes leads to the multiplication of similar parts when they occur in series.

The evolution of the teeth in the Notidanidæ is, indeed, so noteworthy, that the principal stages have been illustrated in succession in the accompanying Figs. 9–12. The earliest known tooth closely approaching that of *Notidanus* in shape is one from the Lower Oolite

TEETH OF NOTIDANIDÆ.

Fig. 9. "*Hybodus polyprion*." Fig. 10. *Notidanus daviesi*. Fig. 11. *N. microdon*. Fig. 12. *N. gigas*.

(Fig. 9): it is commonly referred to *Hybodus polyprion*, and only differs from the tooth of the ordinary *Hybodus* in the remarkable reduction of the cusps at one end. The next form of tooth, in the Upper Oolite, referred to *Notidanus muensteri* and *N. daviesi* (Fig. 10), has the reduced cusps absent or mere serrations, and the cusps at the other side of the principal cone somewhat enlarged; but the root or base is still depressed, as in *Hybodus*. In the Upper Cretaceous species (Fig. 11) the root is compressed and deepened, and the cusps behind the principal cone are as many as seven in number ; while in some Pliocene species (Fig. 12) the corresponding teeth have not less than ten cusps behind the principal cone, thus attaining the maximum complication. A few of the simpler types of teeth, of course, persist as usual until periods later than those at which they were dominant ; but the more complex teeth are invariably later than the earliest known examples of the simpler teeth.

To the present writer the evolution of the multi-cuspidate teeth of the Notidanidæ thus described appears analogous to that observed among mammalian teeth in those of the elephants. The principle that has admitted of the gradual multiplication of the plates (or ridges) in the grinders of elephants as they have evolved from the stage of the Miocene *Dinotherium* to that of the surviving species of India, may be assumed to have also governed the multiplication of the cusps in the sharks under consideration. In neither case is there any scope for a process of fusion of once-detached cusps or cones; in both cases it is obvious that changes have taken place in the formative tissues.

So far as the complication of teeth is concerned, the various processes thus appear to be the same in the highest as in the lowest vertebrates; and these are evidently to be regarded as the outcome of one fundamental law. It is singular, however, that there is as yet little or no evidence of the operation of this law in the intermediate groups of Amphibia and Reptilia, in which the dentition consists almost invariably of isolated, simple, or crenated cones.

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A. Smith Woodward.

IV.

The Walk of Arthropods.

MODERN naturalists are often reproached with their preference for describing dead animals rather than for observing live ones. The justice of such reproof cannot be altogether denied, and it must be admitted that the knowledge of a creature's structure needs to be supplemented with a knowledge of how it lives and moves before we can claim to know it. Some years ago, we were much startled to learn that the conventional picture of a running horse represents the animal in an attitude altogether unnatural, and Muybridge's instantaneous photographs taught us how quadrupeds really use their limbs. A study of the walk of Arthropods is no less interesting, and this neglected subject is dealt with in several recent papers, in the latest of which Dixon (1) brings instantaneous photography to bear also on the problem.

Previous observers seem to have trusted to what they could actually see of an insect's progress, or to have made it record its steps by some graphic means. The most elaborate memoirs hitherto published on the subject are those of Demoor (2, 3) who has studied in great detail the locomotion of some insects, arachnids, and crustaceans. By means of suitably arranged vertical and inclined plates of glass partially covered by black paper, he compelled the creature under observation to move in a straight line, as it sought or shunned the light, and observed it through the uncovered glass. Beetles and crustaceans were made to record their steps by walking over smoked glass, and the various feet of Arthropods were dipped in different colours, so that diagrams of their relative positions were obtained. The horizontal and vertical displacements of the body were indicated by means of a stylet attached to the abdomen.

The ordinary method of an insect's walk seems generally agreed upon; it was long ago correctly described by Kirby and Spence. The first and third legs on one side move together with the second on the other, so that the creature is supported successively on one of an alternate pair of tripods; an arrangement of the highest mechanical perfection, as Demoor points out. Graber (4), Carlet (5), Lloyd Morgan (6), and Wilkins (7), all arrived at the same diagonal scheme for the walk of an insect as that now set forth by Demoor and Dixon. Demoor lays stress on the fact that each leg of

the tripod has its special function, the foremost to draw, the middle to support, the hindmost to push the body. He also describes in detail the motions of the body, horizontal, vertical, and transverse, which are such as to draw its centre of gravity towards the apex of the triangle of support, and so facilitate the fall on to the other tripod. He concludes from this that the insect progresses by a true walk—"a series of successively arrested falls." His observations seem all to have been made on beetles—*Meloë*, *Ateuchus*, and *Oryctes*, and he states that while the three legs of the set are placed on the ground simultaneously, the hindmost is not lifted until after the other two.

The photographic method adopted by Dixon enables the exact order of the motions to be made out. He used a camera looking vertically down on the animal, which ran on a white sheet of paper floating on water. Sunlight was used so that the shadows cast showed the exact position of the legs in relation to the ground. He found that while in the cockroach and the house-fly the hindmost leg is lifted last of the three, in the blow-fly it is the first to be raised. A highly interesting result is the extension of the diagonal motion to the anterior appendages, as in the cockroach and earwig each antenna and maxillary palp moves with the front leg of its side: it will be remembered that the second pair of maxillæ (labium) comes between the legs and the maxillæ, and the mandibles between these and the antennæ. It is possible that some aid to the determination of homologies may come from such observations, but it seems likely that this rule of moving in diagonals would only be obeyed by those appendages which remain functional.

Dixon has shown that the motion of limbs in diagonals is not universal in insects. An aphis and some small flies sometimes moved the legs of the same pair together, as did also the larva of a rove-beetle. That the latter mode is general among caterpillars is already well-known, and is recorded by Carlet (8); the claspers at the hinder end of the animal are moved first, and the wave of motion travels forwards. Dixon finds this motion of limbs in pairs also sometimes takes place in a thysanure (*Tomocerus*), specially when the abdominal sucker is in use. The fact that the primitive thysanure has a similar walk to the larva of higher insects is interesting and suggestive, but the median support afforded by the abdominal sucker makes such a mode of progression very natural, and the similarity may be homoplastic rather than homologous.

With regard to myriapods, Gaubert, in the memoir already referred to in NATURAL SCIENCE (p. 527), states that, while centipedes move their legs alternately, millipedes move those of the same pair together.

The walk of arachnids is described by Gaubert as produced by the motion of alternate limbs, the first and third legs on one side moving with the second and fourth on the other. This result agrees with that obtained by Carlet and Wilkins; but they seem to have

observed only spiders. Demoor's observations have been made on a scorpion (*Buthus*), in which he finds a more complicated arrangement. The second and third legs of one side are close together at the moment when those of the other are far apart; these four legs give, therefore, a triangular base of support whose apex is alternately to the right and left of the animal; the first and fourth legs of the same side move together, each alternating with its fellow, the first pair of legs, as in insects, having a tractive, the hindermost pair a pushing function. Demoor thinks that this arrangement should hold throughout the arachnids, and tries to throw doubt on the results of Carlet, who gave the regular motion of diagonal limbs as the walk of the common wheel-web spider, *Epeira diadematа*. According to Demoor, the great relative size of the abdomen in the female *Epeira* makes it an untrustworthy subject for such researches. Gaubert, however, has observed the same walk in spiders of the genus *Tegenaria*, and we may consider the results of Carlet fully confirmed, as far as spiders are concerned, by the photographic researches of Dixon. The common house-spider, *Tegenaria derhamii*, and a hunting spider, *Tarentula pulverulenta*, were observed by him, and the rule of motion of limbs in diagonals holds in both. These species are perfectly at home running on flat surfaces, and are certainly better subjects for observation than the *Epeiræ* which are rather adapted for climbing on plants or about their own webs. The female *Epeira diadematа* can run, however, over a flat surface, and seems to carry her abdomen clear of the ground. I have observed that the proportionally larger, globular abdomen of the female *E. quadrata* is at times a drag on her movements on a flat surface, and the hind legs are often not raised from the ground but simply trailed along it. If the walk of *Buthus* as observed by Demoor is representative of what occurs among scorpions generally, we must consider their mode of progression to differ from that of spiders. If all insects do not walk alike, we may naturally expect to find differences among arachnids. Dixon's observations show that while in the *Tegenaria* the wave of motion travels from the hindmost to the foremost legs, it travels from before backwards in the *Tarentula*, though in both cases there is a pause between the action of the two sets of diagonal limbs. A reduction in the time of this pause, and an extension in the interval between the motion of the first and last legs of the same set, would produce such a walk as Demoor finds in *Buthus*.

Dixon noted that a spider often uses its palps as legs, but could not determine at what time they move. Gaubert states that a palp moves with the second leg of its side; it follows, that is, the rule of diagonals.

For the walk of crustaceans we have only the researches of Demoor. The prawn, *Palæmon serratus*, generally uses six legs for walking, which move in diagonal sets as among insects. *Gebia littoralis* sometimes shows the same walk, but when wishing to emerge

from the water, while its hinder abdominal appendages are fixed to the sea-bed, it stretches its legs forwards, those of the same pair acting together, and so draws itself onwards like a caterpillar. This mode of progression is analogous also to what Dixon observed in *Tomocerus* when using its abdominal sucker. The lobster, *Homarus vulgaris*, uses its four pairs of legs in walking, according to Demoor, in much the same way as the scorpion, *Buthus*, but its limbs act with less mechanical advantage than those of the arachnid. As might be expected, the walk of crabs is irregular and complicated; the legs of a pair always move alternately, but while the four pairs sometimes follow the rule found in spiders, sometimes the walk is like that of *Buthus*, and at other times the four legs of a side are moved successively and independently. The well-known lateral motion of a crab is caused by the legs of one side acting as tractive levers, while those of the other side push.

An interesting field for research is before us in the walk of Arthropods, and there can be no doubt that photography will give us more reliable results than any other means which can be used to determine how these creatures use their wonderfully-formed limbs.

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Geo. H. Carpenter.

V.

The Falling of Leaves.

IT is a matter of common observation that most of the trees in temperate climates like our own are deciduous, that is, shed their leaves at the approach of the unfavourable season to break out anew when it is past. The roughest examination of a leaf shows the reason; a delicate structure spread out to get as much air and sunlight as possible, and containing a high percentage of water, would fare badly in the frosts of winter. The plant, indeed, has often to resort to devices for protection against the cold of even a summer night, as witness the sleep movements of leaves; the false Acacia (*Robinia pseud-acacia*), for instance, dropping its leaflets into a vertical position, instead of, as in the day-time, spreading them almost horizontally to the sky. An early snow-storm shows the foliage to be actually a source of danger to the tree in winter, the great weight of snow, kept from falling by the leaves, often snapping huge branches, or bearing down the whole tree. Where leaves persist through the winter, as in evergreens, they are specially protected by a thick cuticle, and often much reduced in size, as in Pine "needles."

Common though the occurrence is, it is only in recent years that we have had anything beyond vague guesses as to the way in which it is brought about. Various suggestions were made from time to time, but they were not based on a careful investigation of the parts concerned, and were, therefore, generally valueless. Thus Duhamel (1), in his "Physique des Arbres" (1758), states that a layer of tissue between the leaf and stem remains in a herbaceous condition, and cannot stand the cold of winter; and this being the case, evergreen trees keep their leaves by reason of the point of insertion acquiring a maturity sufficient to resist the severe weather. He admits, however, that this explanation is not altogether satisfactory, for it does not explain why leaves which only appeared at the beginning of the autumn, resist the cold better than those developed in spring, whereas the latter ought certainly to be the less herbaceous, and accordingly suggests as a second cause that the leaves stop growing on account of their strong evaporation of water, while the stem continues to grow in thickness, thereby setting up a mechanical strain and tearing the fibres connecting the two members.

According to Mustel (2) (1781), the leaves cease to evaporate in

autumn, but the heat from the sun being often considerable, continues to raise the sap in the stem; the leaf, however, is already choked, and can admit no more, and so strong is the pressure of the ascending current against the petiole that it falls off. Murray (3) (1785) offers another mechanical explanation; he says the pressure of the growing bud in the axil of the leaf hinders more and more the flow of sap into the latter, causing it to die and fall off. Gerard Vrolik (4) (1797), though his explanation is based rather on analogy with animal structures than on observation, comes nearer the mark. Every organic part has its own life-period, which it fulfils, and then dies at the appointed time, independently of the duration of other parts. Premature death of the leaf may be brought about by excessive cold, heat, or disease. The separation of dead leaves is due not to mechanical causes but to the absorption of a layer between the dead and living parts yet belonging to the living part. This absorption occurs first in the cell-tissue, and later in the vascular bundle of the leaf-joint. Vaucher (8) (1821) imagined a parenchymatous tissue between the stem and leaf which, so long as it was living and succulent, strongly connected the two members. In the autumn it dried up or altered in such a way that the union was interrupted. He also believed that the fibres did not run uninterruptedly from the stem into the leaf, but were connected by a suture formed most probably of parenchyma in which the separation occurs when the fibres in the petiole die. Schultz (9) (1823) thought that the ends of the members of which he imagined the vessels composed were on a level at the place of separation, and that they became gradually shut off from each other, so that at last no flow of sap from stem to leaf was possible. He does not explain how the actual separation took place. Link (6) (1812) suggested that the direction of the cells was altered at the place where the separation ultimately took place. According to Treviranus (11) (1835) also, the cell-tissue at the leaf-joint differs from that of the rest of the organ in form, size, and direction; and when the vigour of the life-processes diminish, there is insufficient force to push the sap through the joint and the different cell-masses become separated. Schacht (13) (1859) makes leaf-fall the result of the stoppage of the flow of sap between stem and leaf by the gradual formation of a cork-layer across the joint.

Mettenius (12) (1856), who really seems to have made careful observations, states that in those ferns which throw off their leaves, the process is furthered and made possible by the death of a layer of delicate-walled parenchyma, which, in the same way as in dicotyledons, is formed between the leaf-cushion and the petiole.

Dr. Inman (14), in a short paper read before the Literary and Philosophical Society of Liverpool in March, 1848, describes the gradual formation of a "process of bark" across the leaf-joint, and also states "that the disruption takes place invariably through the cellular tissue external to the prolongation of the epidermis" or

process of bark, so that the tree does not suffer from the effects of an open wound. The change in the cellular tissue prior to its disruption he describes as a sort of self-disintegration; "the cells contract and become rounder, and separate their walls from each other, so as to destroy their cohesion." He lays great stress on the point that this separating process is a vital one, and there is no doubt that he gives the true explanation, but it is vaguely expressed, and though foreshadowing some of the results subsequently arrived at by Mohl, his work will not compare with the careful account of the German botanist to whom belongs the honour of the satisfactory elucidation of the problem of leaf-fall.

In 1859, Hugo v. Mohl (15) chanced, as he tells us, for the first time for many years to spend the autumn at home, and took the opportunity of investigating, in a number of plants, the organic changes which cause the fall of leaves. The result of his investigations will be found in the first two numbers of the *Botanische Zeitung* for 1860, of which paper he was then one of the editors. Taking *Gymnocladus canadensis* as an example, he describes the appearances noticed in the tissues of the leaf-joint between the 10th and 20th October, during which time many of the leaves lose their leaflets, and the common leaf-stalks on the lower nodes of the branches gradually drop, while those on the upper nodes are only beginning to prepare for their fall and cannot be removed without tearing the joint.

Separating the tissue of the twig from that of the leaf-stalk is a layer of cork. The tissue of the lower part of the leaf-stalk consists of shorter cells than those of the rest of the stalk or the cortex of the twig, while the lowest layer immediately above the cork also has its cell-walls coloured brown. Somewhat parallel with this brown layer, but separated from it by colourless polyhedral cells exactly similar to those forming the bulk of the leaf-joint, a thin cell-layer runs across the joint. If we examine a leaf near the time for falling, but which yet cannot be separated without irregular tearing, this layer is seen, in longitudinal section, to consist of a few cell-rows, somewhat more opaque than the rest of the tissue, in which treatment with iodine tincture reveals a fair amount of very small starch grains, absent from the rest of the tissue of the leaf-joint, a brown-staining, slimy content of a proteid nature, and a conspicuous primordial utricle. We are evidently dealing with a tissue which, in contrast with the rest of the leaf-joint, has all the characters of a young tissue —is, in fact, meristematic. Closer observation shows, in many of the cells, thin dividing walls parallel to the layer; a process of cell multiplication is evidently going on. If we examine a leaf on the point of falling, where the tear is more or less complete, we find that it proceeds from the separation of the walls of the cells of the starch-containing layer, the free rounded surfaces of which are seen lining the sides of the tear. This layer Mohl terms the "separating layer."

The result of this process is that the surface of the leaf-scar, directly after the fall of the leaf, is formed, not by the brown dying cell-layers, but by the colourless succulent living cells above these, which, however, rapidly dry up. According to Mohl, the formation of the "separating layer" begins only a short time before the fall; it was not present on October 4th. It is not formed simultaneously through the whole thickness of the joint, but starts from the inner side bounding the leaf-axil and proceeds towards the outer surface; it often happens that complete separation has taken place above, next the axil, while the under outer ridge is still firmly united to the cortex and cannot be separated without tearing. He is unable to say at what time the cork-layer was formed, as the researches on *Gymnocladus* did not extend back beyond the middle of September, when it was already present. The vascular bundles take no part in the changes which anticipate leaf-fall. They run unaltered and uninterrupted from the twig through the cork-layer, and the brown, round-celled layers above it. After the separation in the surrounding tissues they are broken in a purely mechanical way by the mere weight of the blade, or some external strain, such as wind or rain.

Thus for *Gymnocladus* Mohl shows conclusively that the separation of the leaf is the result of changes in a distinct "separating layer" which bear no connection with the previously formed cork-layers. The latter serve to protect the surface of the scar, which is consequently healed long before it is actually made.

Mohl also investigated the phenomenon in many other temperate plants with deciduous leaves, including shrubs and herbs as well as trees. In many cases the cork-layer was present as in *Gymnocladus*, but frequently it was absent; this was the case in all the ferns with deciduous leaves which he examined, in the beech, several oaks, the elm, the white mulberry, the lilac, belladonna, the tulip tree, and others. Hence, it is evidently not the cause of the leaf-fall as Schacht thought, and as is also seen from the fact that in plants like the poplars, which regularly lose their leaves from below upwards, the cork-layer is in exactly the same condition in those leaves which are about to fall and those still firmly connected; the difference lying in the "separating layer" which, while scarcely marked in the lower leaves, is completely formed in the upper. The brown coloration (suberisation) of the membranes of the round-celled layer has also no direct influence on the fall, as separation occurs above it in the still fresh and succulent part of the petiole; this layer varies greatly in size, and is sometimes quite inconspicuous.

The "separating layer" is universally present, though sometimes easily overlooked when only divided from the brown, round-celled layer by two rows of cells, in which case the tear seemed to take place between the dead tissue and the fresh, living leaf-stalk. The formation of this all-important layer usually begins, according to Mohl, a few weeks before the fall, in most cases not before the

beginning of October; he could not find it in many plants on October 5th, *e.g.*, ash, *Ampelopsis* and others, but it formed in these in the course of the next five to ten days. No definite time can, however, be given, as it depends on the weather, the position of the tree, and also varies, as, for instance, in the poplars mentioned above, in the different leaves of the same tree according to position on the twig, or whether shaded or exposed to the sun. It was quite absent, or faint traces only were present, in the almost completely dried leaves of the beech and several species of oak in the second half of October.

Mohl definitely states that only two rows of parenchymatous cells are used in its formation. When the leaf nears its fall, the cells separate without tearing and the free cells become rounded off; they are not by any means dead, but contain a well-defined layer of protoplasm and become turgid on addition of water. The transition from firm union to complete separation is, moreover, not sudden, but spread over a time during which the walls get weaker and are more easily separable. In none of the plants examined had the vascular bundles running through the joint undergone any visible change. There was no general rule as to the place of separation, except that it never occurred in the cortex of the stem, but always in the tissue of the leaf-stalk, usually in the lower region of the basal swelling. The place is not previously indicated; it appears in the hitherto perfectly uniform parenchyma of the part of the leaf-stalk.

Such is the account of the anatomical changes resulting in the fall of the leaf given by the German botanist two and thirty years ago. With a few modifications and additions it represents the present state of our knowledge.

In 1882 MM. Guignard and Van Tieghem (19) communicated to the Botanical Society of France the result of some researches conducted specially on *Gymnocladus canadensis*—the plant of which Mohl also made special use. They investigated the fall of the individual leaflets, as well as of the whole leaf, and their researches, moreover, showed the condition of the parts concerned as early as the middle of June.

As regards the leaflets, the formation of the separating layer was the only phenomenon observed, and that only a short time before the fall. A thin layer of active tissue (secondary meristem) is formed in which the cells of the epidermis, and the bast and wood parenchyma, also take part, only the sieve-tubes, the vessels, and the fibres being omitted. The separating layer consists of three rows of cells, of which the middle one is absorbed, while the walls of the upper and lower, relieved on one side from pressure, become rounded towards and press upon each other, and the tension thus caused snapping the vessels and fibres, the leaflet is literally thrown off by the force of the turgescence of the separated cells. The wound is left exposed; there is obviously no reason for the careful protection of scars on a leaf-stalk which will itself soon drop.

As regards this common leaf-stalk, an important point is the

early appearance of the separating layer. In the middle of June a transverse layer of parenchyma dies and becomes suberised, completely separating the parenchyma of the stem from that of the petiole, while leaving the vascular bundles free. This evidently corresponds to Mohl's round-celled layer, which takes on a brown coloration. Shortly after, the layer of cells in the stem immediately below it becomes meristematic, dividing centripetally by walls parallel to the layer. The daughter-cells become suberised from above downwards, and a layer of cork is formed which thickens more and more against the lower border of the first suberous layer. This cork layer is in touch with that of the stem. Thus a double suberised barrier separates the parenchyma of the stem and leaf. A little later there appears in the lower part of the petiole above the cork formation a layer of secondary meristem, quite similar to and behaving like that just described for the leaflet; it appears, however, much earlier, as it is so far, that is, on the lower surface only of the petiole, perfectly developed before the end of June. Leaves examined a month later were in the same state, which is probably maintained without change till the autumn, judging from Mohl's observations, which date from October 4th.

In the present case, leaf-fall was produced artificially by enclosing the twig bearing the leaf in a damp box. For the first five days no alteration was visible, but on the sixth the blue line produced in staining a longitudinal section with iodine tincture, which marks the layer of separating meristem, had progressed upwards, traversed the vascular bundles, and reached half-way to the upper epidermis. On the seventh day it had quite reached the latter as in the leaflet. The central row of cells is absorbed, those of the upper and lower layers becoming turgid, press on each other, causing the vessels and fibres to snap, and the leaf falls. The parenchymatous cells of the end of the bundles exposed on the scar become meristematic and form a cork layer and also grow into the vessels, filling them with tissue (tylose), so that the wound is soon covered by a continuous cushion of cork.

The account of MM. Guignard and Van Tieghem suggests several modifications of that given by Mohl:—

1. The very early appearance of the separating layer in the lower region of the petiole.
2. The similar early appearance of the suberous layer, the true nature of which Mohl did not appreciate, and also of the underlying cork, which he merely states to have been present at the commencement of his researches.
3. The mode of separation which, according to Mohl, occurs without cell-destruction, the layers merely separating from each other; whereas the two later observers speak of the absorption of an intervening layer.

The French botanists also investigated the process in other leaves and leaflets. They found that all the leaflets fall as in *Gymnocladus*

canadensis, while as regards the fall of leaves two methods are to be distinguished. The first, as in *Gymnocladus*, with a previous cicatrisation of the wound; the second as in the leaflets of *Gymnocladus* and leaflets in general, that is with no trace of previous cicatrisation, the wound healing only slowly, remaining for some time exposed as a nidus for spores of parasitic fungi. There are also intermediate cases.

Further researches are needed to show whether the absorption of the median layer is universal, or whether the separation may take place according to both methods. Strasburger (20), describing the leaf-fall in the horse-chestnut in his "Botanische Practicum" (1884), says, p. 241: "The separation takes place inside the separating layer, the cells of which round off against each other and become disconnected"; and Hillhouse, in an editorial note in the English translation of the same, mentions that researches undertaken in 1882 at Bonn, under Strasburger's guidance, and still continued, have led to practically the same result. Bretfeld (18) who investigated the process of leaf-fall in the tree-like monocotyledons, orchids, and aroids, finds that the separating layer in these plants is a still earlier formation, being produced at the same time as the leaf itself in the general tissue-differentiation.

In the case of evergreens, where the leaves are so protected as to be able to withstand the winter cold, the plant is always in leaf. Every season, however, some of the older leaves fall, while new ones are regularly produced. The number of seasons for which they last varies with the species in question; in *Skimmia japonica*, a shrub frequently seen in London parks, leaves will be found on the growth produced the year before last, and Mohl says he saw leaves on eleven-year old shoots of the Silver Fir in the Black Forest.

It is interesting to note that, where the function of the leaves is for some reason performed by another organ, phenomena analogous to leaf-fall may also supervene. In some species of *Rubus*, the blackberry genus, the leaves are reduced to thorns, and the function of assimilating carbonic acid from the atmosphere is performed by the green cortex of the stem. F. W. Oliver (21) has shown that in *Rubus australis* the assimilatory layer is cut off annually by a layer of cork, and that a similar case occurs in *Casuarina*, where the longitudinal ridges on the stem form the chief assimilatory tissue; these ridges are cut off every year by a cork layer and begin to scale off in the second year.

It is well known that a sudden frost in autumn often causes a copious leaf-fall. Mohl (15), in investigating this point, found a thin sheet of ice on the surface of the leaf-scar, while other leaves which were still attached to the twig were, however, quite separated from the leaf-scar by a similar ice-sheet, to the upper surface of which they were frozen. Closer examination showed the ice-sheet to be formed in the separating layer, the cells of which, being actively living, are more delicate and contain more water than the surrounding tissue,

and are hence more susceptible to frost. Examination of the leaf-scar often showed torn fragments of tissue attached thereto, evidence that the separation had been a violent one.

In a paper on the parts involved in the process of defoliation, McNab (16) argues that it is not a separation of the leaf from the axis bearing it, but of one part of the leaf from another, the "epiphyll" from the "hypophyll."

His view is based on a theory of Eichler's, that the leaf consists from its earliest appearance of an upper and a lower portion; from the upper, epiphyll, the stalk and blade are developed, while the stipules are appendages of the lower, hypophyll, which is a part of, and adherent to, the axis and ultimately forms the leaf-scar. This would explain why the stipules sometimes remain after the fall of the leaf, as in *Mimosa*; usually, however, they fall sometime before, having served their purpose as protective organs to the developing leaf. This is a question of morphological interest, but the results hitherto obtained show rather that the separating layer is a new formation appearing somewhere, but at no very definite place, in the lower part of the leaf-stalk, and not marking the line of some previous morphological division.

While the production of the separating layer is the first cause of the leaf-fall, external conditions have also great influence on the process. In still weather a very slender attachment may suffice to support the leaves, which the force of a heavy shower or a gust of wind would speedily send to the ground. In a large town like London, where the air is vitiated by smoke, the leaves will fall sooner than in the pure open country. The glorious autumn tints are also absent, or but feebly reproduced in town, the yellow, orange, or crimson colouring giving place to a dry rusty brown. In a Midland pottery town where manufacturers run riot and sneeze at anti-smoke legislation, the leaves fare worse still, and half-way through September don a jaundiced hue and drop sadly to the ground, or shrivel miserably on the branches.

Climate has naturally a great influence on the length of time during which the leaves will remain attached. Where the autumn is prolonged the foliage will also last. Thus for the neighbourhood of Naples, excepting years when the winter sets in unusually early, Tenore quotes the end of November for the defoliation of the walnut, ash, lime, oak, and poplar, and states that the leaves of the apple, beech, elm, and birch often last till the end of December. Foliage generally will keep longer in a damp, sheltered spot than in dry, sunny, exposed positions, and longer after a wet than a dry summer. All the trees do not lose their leaves at the same time; the oaks and elms are still green when the chestnuts and limes are almost bare. Apart from conditions of climate or weather, each species has its own peculiarity in this regard. This may sometimes be due to the fact that species now growing together in one country have come

originally from somewhat different climates. A plant may have contracted the habit through long generations of shedding its leaves early or late, as the case may be, and retain the habit under changed conditions. Thus De Candolle (17) explains early leaf-fall in some cases by introduction from an extreme climate where it would be necessary, and instances the walnut introduced from Armenia, and the *Catalpa* and *Gleditschia* from the eastern United States.

The question also arises whether plants which come into leaf early in the spring lose their leaves early in the autumn, and whether the late leafers are also tardy as regards defoliation; or whether some other combination obtains, and the early leafers are late to lose their leaves. The hawthorn hedges are among the first to show signs of green in the spring, and among the last to lose their foliage in the autumn. The horse-chestnut buds have burst when the oak and ash are as grey as midwinter, but the latter retain their leaves the longer; we must remember, too, that like many of our trees they get their flowering over before leafing, while in the chestnut the leaves are the first developed. However, as De Candolle remarks, we cannot draw conclusions from stray instances, but must take the mean of researches extending over some time. He accordingly takes the *résumé* of four years' observations (1841–44) given by Quetelet (22) in his report on the climate of Belgium. Thirty-eight species or good varieties were observed. De Candolle makes four lists, A, B, C, D. In A he includes the early leafers, in B those with an early leaf-fall. C comprises the late leafers, and D those where the fall is late. Each list contains about thirteen names. Two, *Spiræa sorbifolia* and the lilac, *Syringa vulgaris*, are common to A and B—that is, out of thirteen early leafers only two lose their leaves early; four are common to A and D, leaf early, but have a late fall; two, the walnut and *Gleditschia ferox*, are common to B and C, having an early leaf-fall though leafing late, and four are late in leafing, and have also a late leaf-fall. It is evident that there is no real connection between the two epochs; the leaf-fall is independent of the previous budding.

De Candolle also quotes observations on individual trees as to whether, for instance, any one chestnut which came into leaf early would be earlier or later than others in shedding its leaves.

In many species, like the horse-chestnut, beech, elm, &c., trees growing side by side, planted at the same time, and subjected to the same external conditions, neither come into leaf nor lose their leaves simultaneously. These facts of idiosyncrasy, he says, are constant. They recur always in the same manner year after year, even when the differences in time are reduced to two or three days. Are the individuals which first come into leaf also the first to lose their leaves? or is the inverse the case? Some facts are given for the horse-chestnut, where the difference between individuals is very marked. Of three old trees in the court of a country house at Frontenex, the owner stated that for years they followed the same order of succession at both

epochs, those which were the first to leaf were also the earliest to lose their leaves. A chestnut in the garden of the Tuileries, from its habit of early leafing, is known as the Vingt Mars, and M. Henri Vilmorin states that, compared with the other chestnuts in the gardens, it keeps its leaves beyond the mean time of leaf-fall. M. Vilmorin also observed thirty-four horse-chestnuts 150 years old in a plantation on his own property at Verrières in 1876 and 1877. He found that those which were the first to lose their leaves in 1876 were again in the same category next year, with slight differences as to order. Six of those which were the first to put out leaves in spring were late to lose them in autumn, while five others were early in leaf and early in losing leaf. He concludes that the two phenomena have no regular relation. A tree which is a precocious leafer in the spring may either be early or late in shedding its foliage in the autumn. He found, however, that in the majority of cases lime trees (*Tilia platyphylla*) which were early to leaf were late in losing leaf. Observations on the beech and elm, the latter at Geneva, showed, as in the case of the chestnut, no relation between the two phenomena.

In conclusion, we may notice how plants utilise the fact of their leaves being merely temporary structures. Leaves share with the bark (also transient) the functions of the excretory organs of an animal; in their cells the plant deposits those by-products and residues from the chemical life-processes of which it can make no further use, and which are thus got rid of when the leaf falls. A leaf which has run out its full time and then been dropped, will contain quantities of calcium oxalate, resinous and gummy matter, and often alkaloids, such as nicotine, thein, and others, which form the nitrogenous excreta of plants. On the other hand, useful matter like starch, sugar, and proteid has been gradually removed and passed back to the stem.

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VI.

Norwich Castle as a Museum.

THE most salient features which, from a distance, mark the site of the ancient and picturesque City of Norwich are its cathedral spire, and its still more venerable castle, the donjon-keep of which forms a striking object for many miles around. Long before the Romans had set foot in Britain this spot had been selected by the Iceni for a British village, and the elevated peninsula of land, almost encircled between the rivers Yare and Wensum, had presented to these early and warlike people a favourable site for a stronghold where, enclosed by earthworks, defended by wooden palisades, and encircled by ditches, they might well defy the incursions of neighbouring tribes as restless and as hostile as themselves.

There is no evidence of Roman masonry in the castle walls, but the neighbouring camp at Caister was doubtless maintained to overawe the district, and they may have fortified the earthworks of the British station, and even kept a part of their garrison there; for all the old Roman roads in Norfolk must have radiated from Caister or Norwich (then the *Venta Icenorum*), proving that it was a place of great military importance in the Roman period.

As the earlier occupiers, the Danes and Saxons, mostly *built in wood*, we shall be justified in concluding that nearly the whole of the present castle[1] is of Norman origin.

Uffa, first king of the East Angles, built the earliest structure here, A.D. 575, but this was destroyed by Sweyn, king of Denmark, in 1003, in revenge for the massacre of the Danes in the previous year; but in 1011 the Danes themselves rebuilt the castle and city, and Norwich, for the time, became a Danish stronghold. Canute, it is stated, erected another building here about 1018, but the present castle was probably built soon after the Norman Conquest.

The following inscription is placed on the eastern face of the keep:—

"This Royal Castle, built by William Rufus, as Knyghton testifies in his Chronicles, on the site of one much more ancient, has been used as a county gaol since the year 1345, and was finally vested in the Magistrates of Norfolk for that purpose by a Royal Grant, confirmed by Act of Parliament in 1806.

"The ornamental work and facing of the exterior having fallen

[1] Except the rubble flint-work foundations, the lower course of the keep, and the bridge over the moat, which is the largest arch extant in this country attributed to the Saxons.

into a state of extreme decay, the same were ordered to be restored, at the expense of the county, by the Court of Quarter Sessions, in April, 1834. This restoration was carried into effect, with the most careful adherence to the details of the ancient work, in Bath stone, as most resembling that of Normandy, which had been originally used, under the superintendence of the visiting Justices, and completed in 1839.

"The battlements and corbel table were designed from the best discoverable authorities, as no portion remained of the original termination to the building.

"Anthony Salvin, Esq., London, Architect.
"Mr. James Watson, of Norwich, Stonemason."

Notwithstanding this bold assertion upon the wall of the keep, on Knyghton's authority, it is very doubtful whether the building was erected by William Rufus, or in his reign. Antiquaries for the most part agree that a strong fort of some kind occupied the site before the Conquest, the inner moat and arch of the bridge being considered of Saxon date. It is also certain that William the Conqueror built a strong Norman castle on the site, which stood, under the leadership of Emma de Guader, Countess of East Anglia, a three months' siege by his own troops, aided by the best artillery of the day, and could not have been destroyed by the siege, as, when it was taken, a garrison of three hundred men was thrown into it. It seems most improbable, therefore, that a new castle should have been erected fifty years later by William Rufus; it would be more probable that the one built by the Conqueror was enlarged and added to, notably by the portion known as Bigod's Tower, &c.[2]

Whether the enclosure was surrounded by three distinct trenches has been disputed,[3] but portions of outer entrenchments have been discovered by Mr. Boardman and others, and it is interesting to notice that the innermost and deepest of these moats still remains, being planted with trees and shrubs, and converted into a very pleasant public garden, at the base of the circular and elevated plateau, upon the level summit of which the castle is built.

The great keep, which stands at the south-west angle of this plateau, is a massive quadrangular pile measuring 110 ft. in length from east to west, 92 ft. 10 ins. in breadth from north to south, and 69½ ft. in height to the top of the merlons of the battlements, the walls being from 10 to 13 ft. in thickness. The only apertures in these huge walls are loop-holes at intervals, used for purposes of defence, and also to admit light to the interior apartments; but the solid walls of the exterior were relieved by ornamentation with string courses and semicircular arches resting on small three-quarter columns.

On the east side of the keep there is a projecting tower, called

[2] For this critical and thoughtful note I am indebted to Miss M. M. Blake, of Sprowston, near Norwich, who is at present engaged on a history of Norwich Castle.

[3] Many Archæologists are inclined to adopt Mr. Harrod's view ("Castles and Convents," pp. 133–39), that a large horseshoe-shaped fosse extended to the south of the existing circular one, as in the case of Castle-Acre, Castle-Rising, and other places where Norman architects availed themselves of existing forts.

"Bigod's Tower," probably built by Hugh Bigod, third Earl of Norfolk, who succeeded his brother as High Constable of the Castle early in the 12th century. This tower, which was an open portal to the grand entrance of the castle, is of a richer kind of architecture, and in the genuine Norman style. Since 1824, it has been entirely restored so as to exhibit its pristine aspect, which certainly differs from the rest of the keep. The beautiful arch in this tower, and the galleries on the same level, giving access to the loop-holes and furnished with seats for the archers, are, together with the staircases in the angles, about the most interesting portions of the building to the archæologist, as they still bear the marks of the Norman chisels.

During many succeeding reigns, the castle was principally used by the sovereign for the reception of State prisoners, and was held by the king's Constable, and afterwards by the Sheriffs for Norfolk and Suffolk, who were expected "to keep it in repair and competently fortified with victuals and other things necessary and convenient, lest danger might threaten the king." (Edward II., 1308.)

Notwithstanding the king's commands, the citizens of Norwich having, in 1294, begun to enclose their city with walls and gates, the castle, which had up to that period been considered the defence of the city, became neglected, and in its subsequent history, from 1345, must be considered simply as that of the county gaol, and its jurisdiction vested in the Sheriffs of Norfolk and Suffolk as custodians for the sovereign for the time; and so it remained for the next 500 years, save that in 1805 his Majesty King George III., having no use for the place, "freely gave this ancient and royal demesne in the county of Norfolk, to be and remain in the possession of the high sheriff and justices of the peace of the county of Norfolk, for the administration of justice, the freedom of elections, and the confinement of the county prisoners." (S. Woodward.)

The plateau on which the castle stands is more than six times the area occupied by the keep itself, and on this plateau (once the inner bailey of the castle) the original county prison and shire-house were situated. These were replaced by new prison buildings in 1824–28, and the keep refaced and restored 1834–39. In 1887, the county prison having been removed to Mousehold, the buildings were purchased of the Government by the Corporation of the City of Norwich for the sum of £4,000.

It was about this time that Mr. E. Boardman, F.R.I.B.A., the City Architect, submitted a plan to the corporation, which he had cherished for many years, by which he proposed to utilise the old prison buildings and the Norman keep on the Castle Hill, and convert the whole into a magnificent museum of science and art for the city, preserving at the same time the most ancient building, "the keep," rescuing it from further decay, and from its former degradation as a prison, and restoring it as an ancient historical monument, to an honoured place among the chief public buildings in the city.

On the 19th October, 1886, at a meeting of the Town Council of Norwich, the late Mr. John Gurney, being then Mayor of Norwich, proposed the conversion of the keep and prison buildings into a museum, with a view to the transfer of the collections now belonging to the Norfolk and Norwich Museum. A committee, known as the Castle Museum Committee, was appointed for the purpose of raising the necessary funds, &c., and to this fund Mr. Gurney contributed £5,000.

Since that date the Castle and all the buildings attached to it have been leased to fifteen trustees, namely, five for the Corporation, five for the shareholders of the Norfolk and Norwich Museum, and five for the subscribers to the Castle Museum Fund. When the scheme is completed, the trustees are to make over the building and collections to the Corporation, who have agreed to accept the trust, and to hold the same for the permanent scientific and educational use of the citizens of Norwich, and to maintain and preserve the said collections and utilise them for the purposes of science, literature, and art, and provide out of the Corporation funds for such purpose.[4]

Various meetings have been held, under the presidency of Lord Walsingham, supported by H.R.H. the Prince of Wales, Lord Leicester, the Lord Lieutenant (Sir Edward Birkbeck, M.P.), the High Sheriff of Norfolk (S. Gurney Buxton, Esq.), the city and county members, the Mayor and Sheriff of Norfolk, and, indeed, all the most influential people in both city and county.

Some £15,000 will be required to complete the buildings, and the sale of the present museum premises, it is estimated, will provide funds sufficient for the furniture and fittings of the new museum.

The adaptation of the whole of the buildings to the purposes of the museum has now been carried into effect, and the writer had the pleasure of seeing the entire series of galleries in an advanced state towards completion in August last.[5]

To such noble proportions may a museum attain, which, in February, 1825, had its initiation in a small body of private gentlemen, naturalists and antiquaries, connected with the city and county, starting with an annual subscription of five shillings each member.

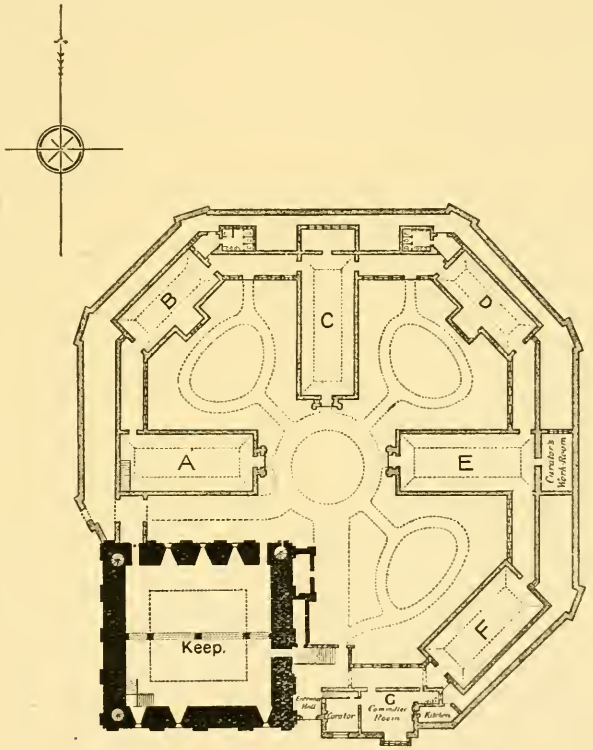
At the end of 1826 we find the Committee appealing to their friends for aid, with a debt of nearly £400, and a collection moderately valued at £850. It is to the honour of these gentlemen and their friends and successors that the Norfolk and Norwich Museum has contined (supported by voluntary contributions) for nearly seventy years, and has, at the present time, in addition to its books

[4] These particulars were kindly sent me in a letter by Mr. John Quinton, Librarian, Norfolk and Norwich Library, Norwich, to whom I am much indebted.

[5] Upon this occasion, and also in March last, I received the greatest kindness and attention from Mr. E. Boardman, Mr. James Reeve, Mr. Eaton, the Honorary Secretary, Mr. Southwell, Mr. Mottram, and other members of the Committee of the N. and N. Museum.

and pictures, its antiquities, its recent and fossil collections, a fine series of British and foreign birds, and the finest collection of raptorial birds in the world, formed and presented to the Norfolk and Norwich Museum by the late John Henry Gurney, Esq.

In the Geological Collection are preserved the grand series of Mammalian remains from the Forest Bed deposits of the Norfolk coast, formed by the late Mr. John Gunn, F.G.S.; the Chalk and Crag Collections of the late Mr. Samuel Woodward, besides those collected and presented to the Museum in recent years by Mr. James Reeve, the present indefatigable curator. The historical and figured specimens preserved in the Norwich Museum,



PLAN OF NORWICH CASTLE MUSEUM.

must be of immense value, and tend to give it a very high rank among our provincial collections.

A most valuable Report was drawn up and presented to the Castle Museum Committee by Dr. P. Lutley Sclater, F.R.S., the Secretary of the Zoological Society of London, dated June 18, 1891, on the requirements of the present collections and their proposed arrangement in the new building. As Dr. Sclater's recommendations were adopted by the Committee in July, 1891, we may conclude that they will be carried out as far as possible when the removal takes place.

The illustration is a ground plan of the inner-bailey, occupying the mound of the Castle Hill. This plateau is 350 ft. long from north to south, and 340 ft. wide from east to west. A promenade of varying width, from 20 to 30 ft. broad, surrounds the exterior of the enclosure, bordered by palisades and overlooking the Castle garden, which, as already stated, occupies the inner moat around the Castle. The keep itself stands near the south-west angle of the hill, having two of its sides exposed, but its north and east faces are enclosed within the precincts of the inner bailey courtyard. The original castellated granite wall of the prison has been preserved and is 20 ft. in height, and (save at the south-west angle, where the keep projects, forming a right angle) is roughly octagonal in form, as seen in the ground plan. The area thus enclosed is about one and a-half acres in extent.

"On entering the castle through the old prison gateway," says Mr. Boardman,[6] "we find ourselves in a vestibule, 28 ft. by 17 ft., having a handsome stone staircase six feet wide, by which entrance is gained to the keep on the left hand, and an archway leading to the enclosed gardens in the centre. The entrance-hall and all the galleries of the museum have been laid with handsome tessellated pavements, similar to that of the Great Hall of the British Natural History Museum in Cromwell Road."

Turning to the right is a corridor, 10 ft. wide and about 50 ft. long, on the right hand side of which is the Curator's room and the Committee room and library (Room G on plan). Over this latter apartment has been constructed a muniment room, with floor and roof formed of iron girders and concrete, so as to render it fire-proof, to be devoted to the preservation of all the deeds and charters belonging to the Corporation.

The first pavilion is entered at the end of the corridor. It measures 54 ft. long by 27 ft. wide (Room F on plan). This gallery is connected by a corridor 14 ft. wide by 40 ft. long, with a second pavilion 68 ft. long by 27 ft. wide and 25 ft. high (Room E on plan). A similar corridor communicates with a third pavilion (D on plan) of corresponding dimensions to the first (Room F). At the end of the second large room (Room E) is the Curator's work-room, constructed with a gallery round it, with large north lights, and fitted with wall-cases and cabinets to contain the large collection of unmounted bird skins. The fourth pavilion (Room C on plan), of similar dimensions to Room E, it is proposed to devote to a picture gallery. The fifth and sixth pavilions (Rooms B and A on plan) correspond in proportions with Rooms D and E, already noticed, and a small connecting room unites this fine series of galleries with the keep, which we enter by a doorway formed in the north wall.

The interior of the keep measures 80 ft. by 76 ft., and a gallery has been constructed around it, on a level with the principal (*ancient*) entrance from Bigod's Tower, about 12 ft. in width. Columns and

[6] *See* Report of his lecture, *Norfolk Chronicle*, November 29, 1890.

arches of stone support the roof which now encloses the keep, partly slated and partly glazed to light the interior. The pillars and arches supporting the roof are placed in accordance with the original disposition of the roof, "visible marks of the strong partition-wall running across from west to east" having existed prior to the erection of the new roof (King, Archæol., vol. iv., p. 403, 1776). Mr. Boardman also discovered (after the removal of eight feet of soil from the interior of the keep) the lower part of the ancient central wall, six and a-half feet in thickness, which ran from east to west and divided the keep into two equal parts. In the northern division he had exposed the foundations of four stone columns which formerly supported the arches or vaulting forming the main floor of the keep. The spiral stairs in three of the four angles remain, and one of them gives access to the roof, from which a grand panorama of the city and the country far and wide can be obtained; while, by descending, we reach the dungeons of the keep and the castle well, about 120 ft. deep, excavated in the chalk.

The keep will probably be devoted to the exhibition of the Ethnological collections and local antiquities, in which latter the Norfolk and Norwich Museum is very rich. Abundant floor-space will also be afforded for the holding of soirées and other scientific gatherings. The small room (leading from the keep to Room A) will, it is proposed, be devoted to the exhibition of Mr. Robert Fitch's collection (partly antiquities, and partly consisting of fossils, from the immediate vicinity of Norwich) recently presented to the Museum.[7]

The fine top-lighted Gallery A will probably be devoted to the exhibition of the geological collections, including the famous mammalian fossil remains, presented by the late Mr. John Gunn, from the Norfolk Forest Bed series. Gallery B will probably be devoted to the collections of recent Invertebrata (Insecta, Crustacea, Mollusca, Corals, &c.), and will also contain the Pisces and Reptilia.

Here the Picture Gallery (Room C) intervenes, but it has been suggested by the architect that a fine connecting corridor may be made in the rear of Block C, so that the Natural History student may pass on to Gallery D, and take up the continuous arrangement of the zoological collection without passing through the Picture Gallery. This room (D) will probably be dedicated to the stuffed specimens of recent Mammalia and their skeletons, and to other zoological specimens.

It has been suggested by Dr. Sclater that Gallery E should be entirely devoted to the magnificent and unique collection of Raptorial Birds formed by the late Mr. John Henry Gurney, and in order that the whole series may be placed in one room, a light iron gallery is to be constructed around this pavilion, which will thus afford greatly increased space for wall-cases, and enable all the specimens to be well seen and *properly arranged*.

[7] Mr. Fitch also gives £300 to provide exhibition cases for his collection

Dr. Sclater mentions in his report that the "Gurney Collection" of Raptorial Birds, at the present time, consists of 3,259 specimens mounted, and 1,345 unmounted specimens, and, with the possible exception of the series in the British Museum, is the finest and most nearly complete collection of its kind at present existing. He recommends that the Diurnal Birds of Prey should be so arranged as to commence on the right-hand side of the corridor as we enter from Room F, be continued all round the ground floor of Room E, and finish on the right-hand side of the corridor leading to Room D. The Nocturnal Accipitres, which are only one-third of the number of the Diurnal Birds of Prey, may be conveniently arranged in the gallery of Room E. "This room," he adds, "I consider to be excellently lighted, and generally well adapted for the exhibition of the great Gurney collection."

Accommodation will also be afforded in the adjoining Curator's room for the Bird skins and duplicates preserved for study purposes, and for the Taxidermist. The connecting corridors to the north and south are to be devoted to birds'-nests and eggs, and to special groups, as, for instance, the fine series of examples of the now extinct Norfolk Bustard (*Otis tarda*), once a denizen of West Norfolk, Wilts, Dorset, and Yorkshire. Room F, it is suggested, should be devoted to as complete an exhibition as possible of the Norfolk fauna generally, and probably a cabinet for the flora also.

This sketch of the general arrangement of the Museum collections is of course *only tentative*, and has been partly pointed out to the writer by Mr. James Reeve, the curator of the Norfolk and Norwich Museum, and in part gleaned from Dr. Sclater's valuable report already referred to. Much must, of course, remain to be fully considered and worked out in detail when the actual time for the removal of the collections arrives; but the Committee will have to very speedily decide as to the exact appropriation of the galleries, because the making of the cases must be put in hand, both for walls and floor spaces; and although the table cases can be afterwards shifted about and transferred to suit change of plan, the wall cases, *being fixtures*, must be designed for the several rooms, and with suitable regard to the size and nature of the objects to be exhibited therein. The greatest praise is due to Mr. E. Boardman, the City Architect, for having initiated the idea of converting the series of prison-blocks into Museum Galleries, whereby hundreds of pounds have been saved, and dry, well-seasoned, and strong walls secured for the galleries, which are certainly not liable to crack and settle, as has frequently happened in many modern buildings. Admirable work-rooms and studies are also provided, and, between the blocks and the outer enclosing wall, ample unoccupied space remains for any number of additional private rooms to be constructed at a small cost. The inner area of the courtyard is being laid out as a very pleasant garden, where, instead of the "Gallows-tree," sweet flowers and shrubs may grow, sheltered

from the wind, yet open to the sun and air. The Castle Museum Committee are also happy in having the Lord Walsingham for their Chairman, one who is himself a Norfolk man, and an ardent naturalist, and who has, by his princely contributions to the British Museum of Natural History, of which he is a Trustee, abundantly proved his generosity and disinterestedness in public matters.

The Committee of the Norfolk and Norwich Museum have acquired their new building at a fortunate time, for they have no need to make expensive experiments in furniture and fittings. All such trials have been made for them in the new Natural History Museum, Cromwell Road. They may there see, not only what is good to imitate, but also what to avoid in the fitting up of exhibition cases; and by careful study of work already executed they may make the Norfolk and Norwich Museum not only a grand historical museum, which it is certain to become, but also the finest and most approved provincial museum of Natural History in the kingdom.

To the Worshipful the Mayor and Corporation it may also be desirable to mention that although they have acquired the finest historical building and the most commanding site within the ancient city of Norwich for a museum, yet that museum, when completed, cannot be maintained creditably unless the Corporation are prepared by a *liberal provision* from the funds at their disposal (and under the Libraries and Museums Act) to provide it with a proper staff of officers, men who by their training and qualifications are fitted efficiently to carry on the work of curating the treasures which have occupied so many years in collecting. Nor should they ever allow the museum of their venerable and historical city to become a mere "Old Curiosity Shop," but rather strive to make it a grand educational centre, so that, like the beacon-light which shone from its "keep" in days of old, its light and culture should radiate over the whole city and county, and many names be added to the list of Norfolk and Norwich worthies among the future, as there have been in the past generation of its citizens.

HENRY WOODWARD.

REFERENCE.

For some account of this ancient building, see the "History and Antiquities of Norwich Castle," by Samuel Woodward, of Norwich, 4to, 1836 (edited by B. B. Woodward). London: 1847. Nichols & Son, Parliament Street, and Charles Muskett, Norwich.

SOME NEW BOOKS.

Animal Coloration. By Frank E. Beddard, M.A., F.R.S. Pp. 288, with four coloured plates and woodcuts. London; Swan Sonnenschein & Co., 1892. Price, 10s. 6d.

The author of the above work resembles the enterprising advertiser, in that he supplies a long-felt want, and differs from him by not having any unfailing nostrum to inflict on suffering and scientific humanity; for Mr. Beddard aims in his book rather at an impartial *résumé* of the facts for the benefit of the intelligent lay reader, than at the promulgation of any new scientific views on this difficult subject. It must not be supposed that because the aim of the book is popular it is not of serious scientific value, for in addition to many new facts it gives an all-round account of the facts of coloration as at present known, which is invaluable for reference. A general review of these opens the book, and occupies a long introductory chapter. The direct influence of environment of coloration is next dealt with at considerable length, and many interesting facts are adduced to show that this, and not Natural Selection, is the cause to be assigned for some groups of colour phenomena. Such are the loss of colour in the Scarlet Ibis in confinement, and the heightening of it in the cayenne-fed canary; the influence of humidity in producing dark colours in insects and reptiles; the existence of brilliantly-coloured animals out of the tropics; and so forth. It is well remarked, with reference to some colour changes, which appear to correspond to the seasons, that these may have some connection with the renewal and decadence of sexual activity. It has been stated with regard to male ducks, which, curiously enough, *lose* their bright plumage at the commencement of summer, that this change does not take place in unpaired birds. In the next chapter, which deals with protective coloration, comes an instance which might, perhaps, be to some extent placed under the former category, namely, the general tawny colour of desert animals, which might possibly be to some extent due to bleaching; pigeons of a tawny colour, for instance, being much affected by exposure to the sun.

It is well said that one would rather expect brilliant coloration to be the rule among nocturnal insects, seeing that all colours would be invisible at night, and the most glaring do their possessors no harm. Yet the dull colours of moths, as distinguished from butterflies, are among the tritest of zoological commonplaces, and Mr. Beddard's suggestion that their reference is to protection in the daytime seems very reasonable. While admitting the wonderful special resemblances exhibited by many animals, such as the giraffe and the Lappet moth, to their surroundings, Mr. Beddard very pertinently asks whether, as a matter of fact, animals are concealed by their protective resemblances from their foes, and points out that a complete answer to this cannot yet be given; for, as he shows, there is always too great a tendency to endow animals with senses exactly similar

to those possessed by ourselves; and yet in many cases we do not anthropomorphise sufficiently. As Mr. Beddard, a little further on, asks: "Judging of birds by our own standard—which is the way in which nearly all the problems relating to colour have been approached—does it seem likely that we should fail to see a caterpillar, perhaps as long as, or longer than, the arm, of an obviously different texture from the branches, and displaying in many cases through its transparent skin the pulsations of the heart, for which we were particularly searching?"

It is also important to notice that certain usually protectively-coloured insects select certain circumscribed situations on trees, while many birds habitually seek their prey on different parts of a tree; and with regard to arboreal animals, it is certainly striking that green is not so universal among these as tawny among those which dwell among sand.

The heading "Protection often due to multiplicity of surrounding objects" is distinctly suggestive, attention being drawn to the difficulty of finding an object on a carpet of complicated pattern, as all know who have hunted the almost proverbially evasive shirt-stud. Elsewhere Mr. Beddard states that he has seen twenty people pass so conspicuous an insect as the common Tiger Moth. The chapter on warning coloration contains records of some new and interesting experiments, which certainly do not always bear out orthodox views of this subject. Most important, too, is the reference to the uncertainty as to the degree of perception of form by insects. The extraordinary indifference to enemies displayed by these animals must have struck everyone who has watched them, and is, perhaps, attributable to their small opportunities of acquiring experience. In speaking of Protective Mimicry, Mr. Beddard suggests that it possibly originated between forms much alike to start with, and the striking resemblances frequently observable where mimicry can hardly be possible would tend to confirm this view. Neither does mimicry appear to be always deceptive, it certainly is not so to the average collector.

There is an interesting closing chapter on Sexual Coloration, and a sentence in it with reference to the work of sexual selection in settling this, is at once characteristic and important. "The question, however, cannot either stand or fall upon its probability or improbability. Actual observation can alone settle the matter."

This may well be taken as the keynote of the book, which is thus of a type eminently desirable in days when theory so far outruns observation, which, like the patient goddesses of Homeric story, has such hard work to do in repairing the breaches left by a rapid predecessor. It remains to be said that the book is in typographical and illustrative execution most attractive, while the coloured plates may well make us wonder "how they do it at the price."

Essay on Life and Death. [Essai sur la Vie et la Mort.] Bibliothèque Evolutioniste, vol. iv. By Armand Sabatier, Professor of the Faculty of Sciences at Montpellier. Paris: Ve. Babé et Cie., 1892.

This volume owes its existence to an attempt on the part of the Professors of Montpellier to bring themselves into closer relations with their city. It is based on a series of popular lectures—parallel in many ways to English University Extension lectures—given by Professor Sabatier to the people of his university town.

The book reveals its origin in many excellent features. It is clear, orderly, historical. The problems are stated in striking ways; the expositions of current views, and of any modifications of them, are simple and direct.

The first and smaller half of the book is on life. It is a detailed comparison between the so-called "organic" and "inorganic." The general point of view is summed up by the author:—"La matière dite brute est vivante. La vie de la matière brute et la vie de la matière vivante sont deux aspects, deux moments différénts de la vie. La vie est partout, l'esprit est partout." "So-called inorganic matter is living. The life of inorganic matter and the life of organic matter are two faces of life, two kinds of its energy. Life is everywhere; mind is everywhere." It is an exact and careful attempt to prove and illustrate a theory closely resembling W. K. Clifford's double-faced unity. Most of the barriers between organic and inorganic substances have now been broken down. Substance after substance naturally formed by animals or plants have been built up in the laboratories of chemists. Sensitiveness, movement, growth, individuality, are all questions of degree, and are not the exclusive privilege of living matter. Professor Sabatier makes a detailed examination of the phenomena of life, and finds strict parallels for them all in inorganic substances. The second and larger part of the book is devoted to death; for the author, although he has minimised the differences between living and dead matter, by no means assumes that he has removed the sting from death.

He gives a careful and exceedingly useful account of the contributions to the question of all the most important writers, and makes very pretty critical play among their theories. His own theory is elaborately and somewhat discursively treated. Shortly, it seems to be this: Primitive, unspecialised protoplasm is immortal—at least potentially. So long as conditions are favourable, it would continue to feed, grow, and reproduce. But protoplasm has not remained primitive. Even in the Protozoa it has become specialised, and in the tissues of higher animals becomes increasingly specialised. One result of this is that the power of reproduction becomes limited, and death finally appears. In the Protozoa it may be periodically staved off by conjugation. In the Metazoa certain tissues—notably epithelia—retain for a time the embryonic facility of reproduction and for a time are able to repair the organism. In connection with such tissues the germinal cells arise, and thus reproduction of the whole animal is closely connected with reparation of its parts. But the germinal cells having been formed, the germinal tissues, like all other tissues, ultimately become senescent. Death is the price of specialisation, the penalty paid for progress.

So far, there is nothing very novel or suggestive in Professor Sabatier's book. If there were nothing more, it might be left as an admirable and able review of the subjects by a learned biologist. I have purposely left for separate treatment what seems his most important idea, an idea which has no doubt occurred to most biologists, but which certainly never before has received so striking and suggestive consideration.

It has long been held that the most peculiar feature of protoplasm, the very abstraction of life, is its production only in the presence of already formed protoplasm. *Omne vivum e vivo*: there may be in a flask or a ditch all the constituents of protoplasm, but even under the most suitable conditions these constituents do not come together

unless there is already present a speck of living matter. Professor Sabatier calls this power, by which a cell converts into protoplasm substances which are not protoplasm, "*amorce*"—the power of inducing combination. It is this that is possessed in full force by primitive protoplasm, and so confers on it conditional immortality. It is this that gives embryonic tissues their reparative and reproductive powers. But as cells become more and more specialised they gradually lose "*amorce*," and ultimately die.

But Professor Sabatier collects many instances to show that this—the apparently peculiar feature of life—is really shared with inorganic chemical reactions. Again and again it happens that all the conditions for the production of an inorganic substance may be present, but the reaction will not take place, or takes place only slowly and with difficulty, unless a small fragment of the desired compound be placed in the mixture. It is in the working out of this idea that the most valuable part of the book consists, and it seems, without question, that this line of research will carry biologists almost into the confines of physical chemistry. The outposts from the two sides already see each other. P. C. M.

HORN MEASUREMENTS AND WEIGHTS OF THE GREAT GAME OF THE WORLD. By Rowland Ward, F.Z.S. Square 8vo. Pp. viii. and 264, illustrated. London: Published by the Author, 1892. Price £1 10s.

IN this handsome and well-illustrated square 8vo volume, Mr. Rowland Ward treats of a subject which forms a kind of border-land between sport and science; and the author has thus done well in stating on the title-page that his work is intended as a record for both sportsmen and naturalists. With the mere ambition of the sportsman to beat his rival by obtaining the finest pair of horns or antlers of any species of "large game" on record, the true zoologist has, of course, no sympathy; but, on the other hand, every student of mammals must be interested in learning the maximum development which these appendages are capable of attaining, and from this point of view he thus is clearly indebted to the rivalry among sportsmen.

As its title implies, the greater part of the work is devoted to the Pecora, among which the Bovidæ, from their numerical superiority, claim by far the larger share of space. The horns of the various species of Rhinoceroses likewise come within the scope of the title of the volume. When we find, however, that the dimensions attained by the tusks of elephants, hippopotami, and wild boars, as well as the bodily measurements of lions and tigers, are also given, we feel that the author has scarcely done himself justice in selecting a sufficiently comprehensive title for his work.

The especial value of Mr. Rowland Ward's volume is that it covers a practically new field, there being, so far as we are aware, no other which treats of the whole subject; such records of measurements which exist being, as a rule, scattered through various zoological and sporting works, the publications of zoological and natural history societies, and the columns of sporting newspapers. In the main, the work consists of a series of tables of measurements arranged under the headings of the various species recorded; but in the case of some of the less known or recently discovered forms, more or less detailed descriptions of the animals themselves are given. The tables, in most cases, comprise measurements of a very large

number of specimens, so that the naturalist will be able not only to learn the maximum dimensions of the horns or antlers of the various species, but likewise their average size. Wherever it has been practicable to do so, the locality whence each specimen was obtained, as well as its present ownership, is also recorded. The illustrations, many of which have been specially executed for the work, are in the main good, and render the volume much more attractive than it would otherwise have been.

In most cases Mr. Rowland Ward is well up to date in regard to scientific nomenclature, though he has unfortunately retained a few names which have long ago been consigned to the oblivion of synonyms; and he might, we think, in regard to Indian mammals, have been better advised had he followed Mr. Blanford's recently completed monograph of that subject. Among the figures we notice that the one given on page 239 as that of the head of the common African Rhinoceros (*R. bicornis*), is really one of Burchell's Rhinoceros (*R. simus*); clearly showing (although not altogether satisfactory) the truncated upper lip, long nasal orifice, and tall tubular ears of the latter. In the table of measurements given on the same page we also notice that the second longest Rhinoceros horn known (54 inches) is put down as belonging to *R. bicornis*; whereas, on turning to page 194 of Mr. Selous' "Hunter's Wanderings in Africa," we find that it really pertained to *R. simus*. Indeed, no known horn of the former comes anywhere near these dimensions. We have also observed certain inaccuracies with regard to distribution, which would have been better avoided.

Much as we may regret them as tending to mislead the non-scientific sportsmen, these little inaccuracies scarcely, however, detract from the zoological value of the work, as those versed in the study of mammals will detect most of them without great difficulty.

Since the amount of labour involved in taking the various measurements, most of which were made by the author himself, must have been enormous, Mr. Rowland Ward is to be congratulated on the energy and perseverance which have enabled him to complete his arduous task, and thus to produce a book which, while invaluable to the sportsman, is at the same time of the highest importance to the working zoologist. R. L.

The Sportsman in South Africa. By J. A. Nicolls and W. Eglington. 8vo. Pp. vi. and 147. Plates. London: The British and Colonial Publications Co., 1892. Price, 10s.

The stream of works on South African animals and sport seems endless, but the one before us is decidedly above the average, and is worthy to rank alongside Mr. Selous' well-known volume. It is stated on the title page that the object of this work is to describe the haunts, habits, form, and pursuit of all the so-called game animals—both mammalian and avian—which are to be found in the country lying to the southward of the Zambesi; and, so far as we can judge, this task has been carried out very creditably. The most striking feature of the book is the excellence of the illustrations, which comprise upwards of 68 figures, contained in twelve plates. The figures are executed by some process with which we are unfamiliar, but which, from an artistic point of view, is deserving of the highest praise, and is far above the level of the ordinary woodcut. This artistic excellence is, moreover, obtained without any loss of fidelity to nature; and some

of the illustrations, like the one of the head of the Eland (which we owe to the courtesy of the publishers) are the most spirited and at the same time the most faithful likenesses of the animals they portray that we have come across for a long time.

The work commences with a chapter on the equipment necessary for a sporting trip in South Africa, which is followed by others relating to the fearful destruction of game which has there taken place, and the efforts which have been made to preserve the small remnant ere it is too late. The authors state that, apart from the question whether such enactments as were made were ineffectual in themselves, or were too late in their appearance, there is no doubt

Head of Eland (*Oreas canna*).

"that, with the exception of a few wild elephants, and, perhaps, buffalo, which still eke out a harried existence, although protected in the Colonial Government's forests, and also an odd troop of zebras, koodoos, and probably hartebeest, which serve as ornaments in a semi-domesticated condition on some of the out-of-the-way farms, the remnant of the noble game which once roamed in countless thousands all over the country, for which Southern Africa was pre-eminently renowned, has been, by wanton and ruthless slaughter, decimated or driven far beyond the outermost bounds of civilisation into the pathless veldt of the Kalahari, or the inhospitable territories of the aborigines of the Interior." While participating in the authors' regrets, the naturalist cannot but rejoice that there are still such

havens where the remnants of many of the species can find a refuge, instead of sharing the fate which there is only too much reason to fear has befallen the Quagga and Burchell's Rhinoceros.

In the descriptive part of the work each species referred to has a brief diagnosis, followed by a notice of its habits, and its present distribution; the latter being especially valuable to the zoologist. Perhaps the most interesting passage in the volume relates to the pair of horns on the evidence of which Dr. Günther gave the name *Antilope triangularis*; the supposed new animal being subsequently raised to generic rank under the title of *Doratoceros*. The authors of this volume are, however, of opinion that these horns are really nothing more than abnormal examples of those of a Kudu. From a specimen that has recently been brought to our notice, we are, however, inclined to endorse the view of a recent writer in the *Field*, that they belong to a Cow Eland.

This work ought to be useful to the naturalist in general, and of especial value to all those interested in the natural history and sport of South Africa, to whose attention it may be cordially recommended. R. L.

A Vertebrate Fauna of Lakeland, including Cumberland and Westmoreland, with Lancashire North of the Sands. By Rev. H. A. Macpherson, M.A., with a Preface by R. S. Ferguson, F.S.A. Pp. 650, with 6 etchings, 2 coloured plates, map, and woodcuts. Edinburgh: David Douglas, 1892. Price, £1 10s.

Chancellor Ferguson prefaces this bulky work with a lucid and terse description of the chief physical features of the English Lake District. The Prolegomena include essays on a variety of subjects, and are illustrated by some fine woodcuts, drawn by Miss E. C. Woodward. The author acknowledges in the Prolegomena his obligations to Mr. R. Lydekker, who favoured him with assistance in determining some animal remains from the cave-deposits. The mass of the book deals with the present vertebrate fauna of this fascinating region.

The author, being interested in ornithology as a specialist, the second book, dealing with Aves, is naturally the fullest, and describes at great length the habits of wild Swans, of Sand Grouse, and many other species of birds, combining all available historical information with the results of ten years' work at field ornithology. Of the other books, the first contains really important articles on the Pine Marten and the Red Deer. The fourth book, which deals with the fishes, is full of antiquarian lore. The chief local names of animals are supplied in a glossary. Six full-page etchings of local scenery, and two coloured plates of birds lately added to the British list, accompany the text, which is further explained by a carefully drawn map, showing the various elevations in different colours. Both author and publisher are to be congratulated on having produced a most useful and artistic compendium.

The Birds of Devon. By W. S. M. D'Urban, F.L.S., F.E.S., and Rev. M. A. Mathew, M.A., F.L.S., M.B.O.U. Pp. lxxxvii., 459. Illustrated with maps, photos, and coloured plates. London: R. H. Porter, 1892. Price, 21s.

The south-west peninsula of England possesses an avifauna only second in interest to that of Norfolk, and has been the scene of much valuable field work during the last half century. The present volume

not only summarises the long labours of the authors, both of whom are veteran ornithologists whose names carry weight, but it further supplies reference to almost all the rarest birds that have ever straggled to Dorset, Somerset, Devon, and Cornwall. The text is massive and full of detail. We should, however, have been glad if the authors had excised certain portions of their book, such as the long and somewhat superfluous dissertations on the families of birds, which occupy space and tend to obscure much more interesting details. The authors also do not always succeed in bringing the most important facts at their disposal into prominence.

Mr. Murray Mathew is one of the few professed ornithologists who have seen Sabine's Gull alive on the coast of Britain; but he relates his experiences thereon with such excessive modesty, and such an absence of feeling, that one would imagine he felt no emotion at all in such a remarkable position. Other instances in which the writers have failed to do justice to their own researches could easily be cited; but the work, taken as a whole, is an admirable monument to the patient and protracted labours of two English gentlemen, who study birds as born naturalists, and not as collectors.

Not the least attractive feature of this fine volume is furnished by four beautiful plates from the brush of that skilled artist, Mr. Keulemans, representing the Black Redstart, Montagu's Harrier (black variety), Rough-legged Buzzard (black variety), and Great Black-headed Gull.

The frontispiece is a sketch of "Yes Tor," the home of the Ring Ouzel, executed by the late W. H. M. D'Urban. The volume is tastefully bound, printed in good type, and supplied with a useful working index. We trust that it will find a place in the library of every British ornithologist.

THE BIRDS OF LANCASHIRE. By F. S. Mitchell, Member of the British Ornithologists' Union. Second edition. Revised and edited by Howard Saunders, F.L.S., F.Z.S. With additions by R. J. Howard, M.B.O.U., and other local Authorities. 8vo. Pp. xxvi., 271. London: Gurney and Jackson, 1892. Price, 10s. 6d.

THE second edition of this excellent county fauna will be heartily welcomed by ornithologists. It is true we should have preferred that the author himself should bring the text up to date, but, in his absence, this task has been admirably performed by Mr. Howard Saunders.

The editor has left the text almost untouched, with the exception of some literary blemishes, which he has taken great pains to remove. He has, however, added numerous notes in brackets, many of them supplied by a most competent local naturalist, Mr. R. J. Howard, of Blackburn, who undoubtedly knows the avifauna of his county extremely well. It is pleasant to read such notes as those which Mr. Howard has contributed on hybrid Pochards (*see* p. 173); or upon the Grey Geese which frequent the marshes of the Ribble estuary. Mr. Mitchell's list of the birds of the county is now supplemented by five additional species—the Purple Heron, Sociable Plover, Eared Grebe, Wilson's Petrel, and Frigate Petrel; the last of these has just been figured as a new bird to Britain in Macpherson's "Fauna of Lakeland," the proof-sheets of which were placed at Mr. Saunders' disposal.

The great merit of this book consists in the fact that ornithologists can see at a glance the salient points about almost every species

under consideration. A serious defect is the scanty information supplied about some few species. For example, all that we are told about the Red-necked Phalarope is this: "A rare visitor on passage; it is said to have been shot on the Ribble." We only mention this, however, as a clear proof that there is still work to be accomplished by Lancashire ornithologists in mapping out the visits they receive from some of their rarer species. We hope that the appearance of this admirable edition, with its improved index, will spur them on to renewed exertion. The coloured plates of the Wall Creeper and Black-throated Wheatear, which accompanied the first edition, are now replaced by two of the choicest of Mr. G. E. Lodge's woodcuts. We regret that the Wall Creeper obtained in Lancashire, the only British specimen extant, was fated to pass into a private collection, when Mr. Mitchell left England. It should certainly have been secured for the national collection.

The Conchological Society's List of British Land and Freshwater Mollusca, 1892. Compiled by W. Nelson, W. Denison Roebuck, F.L.S., and J. W. Taylor, F.L.S.

Such a list as this comes eminently within the province of a body like the "Conchological Society of Great Britain and Ireland," and the present new edition should have been made thoroughly complete and reliable that it might serve as a guide for the multitude of votaries of the Science, youthful and otherwise, whom we are glad to think abound over the country.

Numerous changes and some improvements have been introduced into the present issue. The old artificial division into Land and Freshwater forms has been abandoned in favour of a more scientific classification, which, however, is capable of yet further emendation as regards the slugs. The Arionidæ, which head the list, should come nearer the Helicidæ, and their place be taken by the Testacellidæ.

The nomenclature has been revised, and several changes made in conformity with the inexorable law of priority. *Helix sericea*, Müll., becomes *H. granulata*, Ald.; *H. ericetorum*, Müll., is now admitted to be Linné's *H. itala*, as long ago shown by Hanley. Whether the *Planorbis marginatus*, Drap., is Müller's *P. umbilicatus*, as now advanced, is very questionable. The genus *Bulimus* disappears: *B. acutus* being rightly transferred to *Helix*, and the other two British species referred to *Buliminus*. *Cyclostoma*, on the other hand, we are glad to see, is retained. The genera *Amphipeplea*, *Segmentina* and *Velletia* are revived for their appropriate species of Limnæidæ; but this being so, it would have been more consistent had *Patula* likewise been allowed to stand as a genus. *Paludina* gives place to *Viviparus*: surely the feminine form would have been more appropriate. *Anodonta anatina* still masquerades as a species, despite the fact that even Isaac Lea would have none of it. *Testacella scutulum*, Sow., is re-established, although further confirmation of its specific distinctness is really desirable. The sectional names of *Trichia* and *Zenobia* have by some oversight been omitted under *Helix*, with the result that the *Fruticola* group is unduly rich in species.

Synonyms are only given where a well-known name has had to give way to an earlier and comparatively unknown one, which they follow in each case in parentheses. Here a confusion arises, owing to the way in which these are expressed. Thus we find "(=*L. margina-*

tus, Jeffr.)" and "(=*H. concinna*, Jeffreys)"; the latter is correct, since it was Jeffreys' own species, but the former is misleading, inasmuch as Jeffreys did not describe any such species. He held (and we believe quite correctly) that the *Limax sowerbyi*, Fér., was a synonym of *L. marginatus* Müll., and accordingly adopted the last name. This could have been made quite clear by putting "(=*L. marginatus*, Müll. of Jeffr.)."

It should also be pointed out that *Cochlicopa*, Fér., was a mixture of very distinct forms, and to Risso belongs the credit of first defining it accurately; hence it is *Cochlicopa*, Risso, or cannot stand at all. There are other small errors in the authorities quoted for specific names, such as *Hyalinia glabra*, attributed to Studer, who, we believe, never described it, while Müller is throughout quoted as Mull., instead of Müll. The compilers also persist in identifying *Planorbis glaber*, Jeff., with the American form *P. parvus*, Say, although the distinctness of the two has been pointed out (*Proc. Geol. Assoc.*, vol. xi., p. 381).

The most objectionable feature of the list, however, is the wholesale introduction of the so-called varietal names. It is true that these trashy designations (which in most cases are applied to mere variations and not to varieties in the strict zoological sense of the term) are not so abundant as in a certain other list recently published, where the array of names appended as "vars." to each species sometimes leads off with "typicus" or "typus"; all the same, they are, scientifically speaking, worthless save in instances, rare, indeed, in this limited area, of forms well marked zoologically or geographically. It is sad to think what an amount of time is wasted in the attempt to identify these delusive "vars." that might be spent in the accomplishment of work of real scientific value. Alas that the variety-monger should be as blind to higher glory as the character depicted by Bunyan intently gathering to himself not overclean straws! The space these varietal names consume might have been turned to account for valuable notes or for synonymy, but this we fear we must wait for till a certain work, long advertised as in preparation, shall appear. Unless, perhaps, since the authors of that work are still struggling with the first genus, *Testacella*, the Conchological Society may meantime arouse itself and vindicate its high-sounding title by appointing a committee of its competent members, who shall produce a correct and reliable list, not of a section merely, but of the whole of the molluscan fauna of these Islands and their surrounding waters. (BV).[2]

Contributions to the Study of the Sponges of the North Atlantic. [Contributions à l'étude des Spongiaires de l'Atlantique Nord.] By E. Topsent. Results of the Scientific Explorations of Albert I., Prince of Monaco. 4to. Pp. 166, pls. xi. Fascicule ii. Monaco: 1892.

In this beautiful monograph of 166 pages and 11 plates are described the sponges collected by the Prince of Monaco, on board the "Hirondelle," in the North Atlantic. The work is almost entirely systematic, but, in an introduction, the author gives a short account of his views on sponge histology—a subject to which he has himself made important contributions in recent years. The whole number of species described amounts to 167, of all orders, of which 58 are new, with 8 new genera. With regard to these new species, we may notice incidentally, for the benefit of that most necessary, but often unappreciated person, the recorder, that though the author writes n. sp. after the species, many of them have already been named

and enumerated by him in preliminary accounts, and will be found recorded in the *Zoological Record* for 1890. Hence, they are not all of them, strictly speaking, new species, though here described for the first time. The nomenclature of spicules published in the "Challenger" Reports is adopted, with the addition of a few names for some new forms of spicules. In classification, the following modifications are proposed:—The order Carnosa, of Carter, is revived, to include the suborders Microsclerophora (Sollas), Oligosilicina (Vosmaer *pars*, *Chondrilla*), and Myxospongida (*Chondrosia* and *Oscarella*). The Monaxonida are classified into Spintharophora (Sollas emend.) and Halichondrina. The latter name is used in the same sense as by Ridley and Dendy in the "Challenger" Reports (vol. xx.). The Spintharophora are divided into Aciculida, with diactinal spicules (Epallacidæ, Stylocordyla, and Tethyadæ), and Clavulida with monactinal spicules (Spirastrellidæ, Suberitidæ, and Clionidæ). The monograph is got up in splendid style, and well printed, and the illustrations are excellent. We may specially draw attention to plate i., containing beautiful coloured pictures of living sponges depicted on board the yacht. M. Topsent must certainly be congratulated on having given us a valuable contribution to the study of an interesting group. E. A. M.

THE FUTURE OF SILVER. [Die Zukunft des Silbers.] By Professor Edward Suess, 8vo. Pp. iv. and 227. Vienna and Leipzig: 1892.

THE Silver Question in general, and the theory of Bimetallism in particular, in the main lie outside the scope of NATURAL SCIENCE; both, however, ultimately depend on the relative abundance of the supply of gold and silver, and this is a purely geological question. We cannot, therefore, follow Professor Suess into the interesting questions of economics and currency raised in his latest work, but as the main part of the volume is devoted to a sketch of the present condition and future prospects of the gold and silver mining fields, of the mode of occurrence of the ores, and of the factors which govern the possibility of their profitable exploitation, the book cannot be allowed to pass unnoticed. Professor Suess, the Professor of Geology in the University of Vienna, is one of the foremost of living geologists, and his "Antlitz der Erde" inspired two of the Presidential Addresses at the last meeting of the British Association; he has special qualifications for the treatment of this subject, being equally eminent as an economist, and his work "Die Zukunft des Goldes," which appeared in 1877, has taken high rank in the literature of the Silver Question. To this, the present volume is a sequel, written to trace the history of the production of gold and silver during the past fifteen years, and to show that the predictions then made have been verified by subsequent events. The book is divided into ten chapters, of which the first five deal with the questions of the occurrence and supply of gold and silver, and the last five with the economic consequences that result therefrom.

In the introductory chapter Professor Suess deals briefly with the Californian and Australian gold discoveries of 1849, and with the visions that these encouraged of a universal international gold currency. These hopes were, however, strongly discouraged by the geologists of the time, and quotations are made from a paper by Murchison in 1854 warning politicians that the enormous gold supply of that period was quite temporary. In the second chapter the author summarises the history of the variations in the gold supply since 1877, and points out that

gold occurs in three ways: as sulphides in deep veins, as metallic gold in superficial "gossans" that result from the weathering of veins, and as alluvial gold. Statistics are quoted of the recent productions of the Californian, Dakotan, Australian, Russian, Siberian, and South African mines; in a later chapter the figures are also added of the Chinese, Indian, and South American workings. The general results show a steady decrease in the proportion yielded by the alluvial deposits: thus from 1848-1875 these yielded 87·7 per cent., in 1876, 65 per cent., and in 1890 only 44·2 per cent. In the face of the difficulties in the working of the deep sulphide deposits, Professor Suess concludes that we shall always be practically dependent for gold for currency on the alluvial workings; mine after mine has been closed as soon as the gossan was removed and the sulphide vein reached. Nor does Professor Suess regard the Transvaal conglomerates as a durable source of supply. During the last few years the total gold supply has declined but slightly, and last year has witnessed an actual increase; but this is shown to be due to the extraordinary efforts of the miners with such tremendous works as the Amador mining canal, 100 miles in length, and the draining of the Feather River by the Big Bend Tunnel nearly three miles long. On pages 98-102 the author has collected the statistics of the amount of gold used in the arts, and concludes that it is now closely equal or slightly in excess of the annual supply. The recent history of silver production is similarly treated and the enormous increase in the output shown. One of the most interesting chapters in the volume is that which records the tragic history of the Comstock lode, from its discovery in 1857 to its final abandonment in 1891. The mines were worked first for gold and then for silver, and the greatest yield for any one year was 37,000,000 dollars in 1876. The story of the mine as Professor Suess tells it us is a singularly varied one: the early settlers were rough even for the Western States, and from murder, famine, and sudden death the romantic records of this lode have not been delivered.

The first part of the volume shows the approaching exhaustion of gold, and the apparently limitless nature of the supply of silver. The ratios of the production of the two were—1 : 5·47 in 1860, 1 : 14 in 1880, and 1 : 23·8 in 1890; the value ratios have consequently gradually changed till they stand at 1 : 22·10 and not 1 : 15·5, which the older schools of bimetallists declared would be permanent. There is too little gold and too much silver, and with the contraction of the gold currency serious commercial difficulties have arisen; prices have fallen, bank reserves have only been maintained at the cost of frequent changes in the bank rate; and, as Professor Suess remarks, any alteration in this immediately affects prices all the world over. Industrial crises have resulted, and it is maintained that they are inevitable until some means be discovered of increasing the medium of exchange. Professor Suess is not a bimetallist, and probably the most valuable part of his book is that in which he emphatically protests that any hope of fixing a definite ratio between gold and silver is quite vain. "This divergence [that between their relative values] lies in the nature of the metals themselves," he tells us on p. 193, "and no statesman and no law can alter their natural relations." Professor Suess, therefore, predicts that the time is coming when silver will be used as the medium of the world's currency. This is not a solution that English financiers are likely to welcome; but while they remain as hopelessly divided as they are at present, it is difficult to conclude whether the truth lies more with the advocates of bimetallism or the champions of the opposite school.

GEOLOGICAL MAP OF SCOTLAND. By Sir Archibald Geikie, D.Sc., LL.D., F.R.S., Director-General of the Geological Survey. Edinburgh: John Bartholomew and Co., 1892. Price 6s.

STUDENTS of British Geology and travellers interested in the relations between scenery and rock-structure, will welcome this new Geological Map of Scotland; for the scale is sufficiently large to show the distribution of the principal groups of rocks, and the topography is based on the latest Ordnance Survey. Moreover, the map is well printed, and the colours, numerous as they are, clearly show the various subdivisions of the rocks.

It was thirty-one years ago when the "First Sketch of a New Geological Map of Scotland, with Explanatory Notes," was issued by Sir Roderick Murchison and Archibald Geikie; and this map was on a scale of 25 miles to an inch. It summarised our knowledge at that date, recording work done by Macculloch, Nicol, Harkness, and others, and that done in a portion of the south of Scotland by the Geological Survey. Much original work was likewise incorporated, for Skye and Raasay, and parts of Islay and Jura, and the Highlands, had been personally surveyed by A. Geikie; while Sutherland and Caithness, the Shetland and Orkney islands, and other parts were coloured according to the work of Murchison. A larger Geological Map of Scotland, by A. Geikie, was published in 1876.

Now, the Geological Survey has mapped in detail the southern half of Scotland, and a large part of the central and north-western Highlands, while Professor Lapworth, Professor Judd, and others, have contributed largely to our knowledge of particular strata, so that it is appropriate that a new and larger map should picture the progress that has been made. This map is accompanied by Explanatory Notes (pp. 23), in which is given a concise account of the leading features in Scottish Geology.

In the older map two colours were used to denote Laurentian and Cambrian rocks. Now the Pre-cambrian rocks are taken (in the Explanatory Notes) to include Lewisian, Dalradian, and Torridonian; and no less than 10 coloured tablets serve to indicate divisions that can be made in the Metamorphic rocks of the Highlands (grouped as Dalradian), and two tablets include other divisions of Serpentine and Lewisian Gneiss. The relations of the "Eastern or younger Schists" (to which the name Dalradian has been applied by Sir A. Geikie) are still so obscure that they cannot be placed in a definite position in the stratigraphical series. The Torridon Sandstone, now known to be Pre-cambrian, is followed unconformably by the Cambrian strata; and these again can nowhere be traced in sequence with the Silurian rocks, of which the lowest portion is correlated with the Arenig group. Succeeding strata of Old Red Sandstone, Carboniferous, Permian, Triassic, Jurassic, and Cretaceous are duly indicated; there being 19 coloured tablets to represent the main divisions from the Torridon Sandstone to the top of the Mesozoic strata. Four other tablets serve to indicate the Tertiary and Post-tertiary deposits, and eight tablets are given to various Igneous rocks.

The scale of the map is 10 miles to an inch, so that in size it is a little over 2 ft. wide by 2 ft. 6 in. in length; and it should be mentioned that the Orkney and Shetland Isles, and also the adjoining tracts of Cumberland and Northumberland, and a small part of the north-east of Ireland, are coloured geologically. There are likewise seven longitudinal sections. Altogether, the map is an excellent one, and the modest price of six shillings will ensure for it a wide circulation.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

UNDER the will of the late Mr. Hodgkin, of Long Island, the Royal Institution of Great Britain benefits to the extent of £20,000.

MR. WALTER E. COLLINGE has been appointed Demonstrator in Biology in the Mason College, Birmingham, and Mr. W. G. Ridewood, B.Sc., has been elected to the new Lectureship in Biology in St. Mary's Hospital Medical School, London.

DR. B. L. ROBINSON has been appointed Curator of the Herbarium of Harvard University, Cambridge, U.S.A., in succession to the late Dr. Sereno Watson. Dr. Robinson has hitherto occupied the position of Assistant Curator. According to the *Botanical Gazette* arrangements have been made by which he will be enabled to devote some time to the completion of the "Synoptical Flora," as it is intended to proceed with this great work as rapidly as possible.

WE learn that Messrs. Iddings and Penrose have accepted appointments on the staff ot the new Chicago University under Professor Chamberlin. The former will lecture on Petrology and Mineralogy. Dr. Walcott has been offered the position of Palæontologist in the same University. It is now arranged that the Academic year shall be divided into four quarters, each of two terms, and that no student shall be required to attend more than six terms each year.

LORD WALSINGHAM, High Steward of Cambridge University, has arranged to present a gold medal annually for a period of three years to the member of the University who shall send in to the Board for Biological and Geological Studies the best original monograph on some department of Natural History research. The date fixed for the first competition is October 1, 1893.

PROFESSOR CLAUS has issued the first part of vol. x. of the *Arbeiten aus dem Zoologischen Institute der Universität Wien und der Zoologischen Station in Triest*. The Professor contributes the second part of his memoir on the development of the Scyphomedusæ, concluding with a discussion of their systematic relationships. Dr. B. Haller describes the anatomy of a gastropod mollusc, *Siphonaria gigas*, which he believes to be one of the most primitive known types of Opisthobranchiata, peculiarly modified by its surroundings. Each memoir is illustrated by three plates.

WE are glad to announce that the condition of Sir Richard Owen has considerably improved, and except for the resulting loss of strength his health, according to Sir James Paget, is as good as it was before the last attack. Sir Richard Owen derived considerable pleasure from a visit of the Prince of Wales, who with characteristic kindness called on Sunday, the 11th, with the Duke of Teck and the Princess May. Sir Richard Owen is now in his 89th year, and were it not

for weakness in the limbs and the fear of cold, might be able to dress daily and sit in the library adjoining his bedroom. His great deafness, however, prevents him from seeing others than his intimate friends.

Mr. Charles Andrews, B.A., B.Sc., is the successful candidate in the recent examination for the position of Assistant in the Geological Department of the British Museum. As soon as the subsequent formalities have been completed, he will commence work on the fossil Vertebrata.

The Botanical Department of the British Museum has lately benefited by the acquisition of two cryptogamic collections of great value. The one is the large herbarium of the late Mr. George Davies, of Brighton, and consists of some 20,000 specimens. It was presented by his widow. The deceased gentleman devoted more than thirty years of his life to a keen study of the mosses, hepatics and lichens, which he partly himself gathered in the British Isles and the south of Europe, and partly received from correspondents in distant parts of the world. The other acquisition is a very complete set of 494 Hepatics, collected by the distinguished traveller and botanist, Dr. Richard Spruce, in the River Amazon and the Andes of South America, during the years 1849-60, and ably monographed by his masterly hand in the *Transactions and Proceedings of the Botanical Society of Edinburgh*, vol. xv. (1885).

The second part of Mr. George Murray's *Phycological Memoirs*, embodying the results of researches on Algæ conducted in the Botanical Department of the British Museum, is nearly ready for issue. Besides papers on the West Indian Algæ and a comparison of the marine floras of the warm Atlantic and Indian Ocean by the Editor, there will also be notes on the Fucaceæ by Miss A. L. Smith, Miss Barton, and Miss F. G. Whitting.

The Smithsonian Institution has lately issued vol. xxviii. of its "Contributions to Knowledge," an elaborate work entitled "Life Histories of North American Birds, with special reference to their Breeding Habits and Eggs," by Captain Charles Bendire, Hon. Curator of the Department of Oology in the U.S. National Museum. The volume deals only with land-birds, and is illustrated by twelve fine coloured plates of eggs. It is based on the author's field notes and the collection in the U.S. National Museum, and there is a reference to every published observation of importance in regard to the habits and distribution of each species.

The Curator of the Kingston Museum, Mr. T. D. A. Cockerell, is displaying praiseworthy activity in his new sphere, and contributes to the current number of the *Journal of the Institute of Jamaica* (vol. i., no. 4) a series of original notes on the local Mollusca, Insects, Myriapods, and Fungi, several in co-operation with well-known specialists. Some new genera and species of insects are described, but without figures. The description and a quaint picture of the earthquake at Port Royal on June 7, 1692, are reprinted from the originals in the British Museum and form an interesting contribution to the Journal. The Institute is evidently doing good work in the promotion of scientific studies in Jamaica.

Two new parts of the *Records of the Australian Museum* (vol. ii., nos. 2, 3) were issued in August. Professor Liversidge contributes the first paper on some bismuth minerals, illustrated by three plates. Mr. A. G. North records some additions to the known Avifauna of Tasmania and Norfolk and Lord Howe Islands, and also has a note on the nesting of the Manucode (*Manucodus comrii*) with figures of the eggs. Mr. J. Douglas Ogilby describes some new reptiles and fishes from Australia, and Mr. C. Hedley discusses the structure and affinities of a land snail, *Panda atomata*, with several illustrations.

THE movement for the Sunday opening of Museums still progresses in England. The Town Council of Sunderland has just resolved to open the Public Museum and Art Gallery each Sunday afternoon from two until six o'clock, while the Reference Library will be open from six until nine in the evening. It is estimated that the additional expenditure incurred will not exceed £60 per annum.

PROFESSOR JAMES GEIKIE'S Address to the Geographical Section of the British Association at Edinburgh (1892) is printed in the *Scottish Geographical Magazine* for September, and is accompanied by a Bathy-hypsometrical Map of the World, coloured to show the surface-relief of the globe without water in the Ocean Basins.

AT the recent meeting of the American Association at Rochester, New York, a resolution was passed in favour of the scheme for a series of International Scientific Congresses to be held next year at the Chicago Exhibition. Committees were appointed to co-operate with the local organisations in making arrangements, and it is expected that reports of the proceedings will be published.

A BRIEF report of the recent International Zoological Congress at Moscow is contributed by Baron Jules de Guerne to the *Revue Scientifique* of October 8. Count Kapnist presided, and there were eleven general meetings, extending over a period of nine days. The French naturalists were represented by Drs. Milne-Edwards, R. Blanchard, and C. Schlumberger, with Baron de Guerne and a deputy for the Prince of Monaco; and among other foreign zoologists present were Professor Virchow and Dr. H. Virchow, of Berlin, Dr. Brusina, of Agram, Dr. Th. Studer, of Berne, and Dr. Jentink, of Leyden. Britain seems to have been unrepresented. Among the subjects discussed were the Northern Asiatic fauna, the Black Sea and Baltic Sea faunas, the classification of the variations of animals according to their causes, the arrangement of the Animal Kingdom in phyla, the value of embryology in classification, the scientific aspects of zoological gardens, and zoological nomenclature. The discussion of the latter subject occupied two ordinary meetings, and was continued from the Paris Congress. The Czar and Czarewitch contributed to the funds of the Congress, and the Grand Duke Sergius Alexandrowitch, Governor of Moscow, personally attended some of the assemblies. The next Congress will be held at Leyden in 1895, Dr. Jentink being president-elect.

THE rules of zoological nomenclature adopted by the Moscow Congress are also published in the same number of the *Revue Scientifique*. The more striking articles are as follows:—

1. The tenth edition of Linnæus' *Systema Naturæ* (1758) to be recognised as the basis of zoological nomenclature.

2. The law of priority to be applicable to the names of families and higher groups exactly as in the case of generic and specific names.

3. The law of priority to prevail, and thus the oldest name to be retained even when given to an identifiable fragment of an extinct animal or to the larva of a living animal.

4 A generic or specific name once published cannot be rejected, even by its author, on account of inapplicability.

5. Hybrid names not to be altered.

6. Capital initials to be employed in all specific names derived from proper names, and the original orthography to be preserved, even retaining accents.

7. When a species is removed from one genus to another, the name of the original author of the specific name to be placed in parentheses, thus:—*Ponto della muricata* (Linn).

8. The name of a family to be changed when the name of the type-genus of the family proves to be a synonym and disappears from nomenclature.

9. Every generic name preoccupied in the same kingdom (règne) to be rejected.

On October 5 a number of members of the British Ornithologists' Union dined together and formed a British Ornithologists' Club, which will meet monthly in London from June to October in each year, and issue a monthly Bulletin. The object of the Club is to promote social intercourse between members of the Union, and a small annual subscription will be levied to cover publication and other expenses. The first committee consists of the Earl of Gainsborough, Dr. Sclater (Editor of *The Ibis*), Dr. Bowdler Sharpe (Secretary and Editor of the Bulletin), and Messrs. Bidwell, Seebohm, and Howard Saunders (Treasurer). At the opening meeting, Mr. Edward Degen read a paper on "Some of the Main Features in the Evolution of the Bird's Wing," which will appear *in extenso* in an early number of *The Ibis*.

Colonel Bailey, R.E., Lecturer on Forestry in the University of Edinburgh, has been appointed Secretary of the Royal Scottish Geographical Society, in succession to Mr. Arthur Silva White. At the anniversary dinner of the Society, held in Edinburgh on October 19, under the Presidency of the Marquis of Lorne, Mr. White was presented with a sum of £400 and an honorary diploma of fellowship.

The Société Scientifique de Chile has issued the first part of the second volume of its *Actes*. It comprises one elaborate memoir of 174 pages by Professor F. P. Borne on a Chilian spider, *Latrodectus formidabilis*, and a record of the Proceedings of Meetings. The first Annual Meeting was held on April 4th, when the Society was reported to be in a flourishing condition both as regards membership and finances. The *Actes* are published in bi-monthly parts and freely distributed abroad.

The last (June) part of *Timehri*, the Journal of the Royal Agricultural and Commercial Society of British Guiana, contains among other matter valuable papers by Mr. J. J. Quelch, on the Bats and Birds of Prey of British Guiana, and numerous short notes on the Natural History of the Colony. There is also a brief article by Mr. T. S. Hargreaves on Guiana Gold.

The Woolhope Naturalists' Field Club is somewhat dilatory in its publications, the volume of *Transactions* for the years 1886-89 having only been issued last month. In addition to the ordinary reports of meetings and excursions, the new volume contains many brief but valuable contributions to the Natural History of the Club's district, and there are several plates, illustrating papers on Archæology.

Part VII. (1891-92) of the *Transactions of the Leeds Geological Association* has just appeared. The frontispiece is a photograph of the base of the trunk of a fine Carboniferous tree, found in the Gannister at Meanwood; and the principal contributions to local geology are those of Mr. B. Holgate on Glacial Deposits in Teesdale, and Mr. J. E. Bedford on Glacial Action in the Leeds District. Many general questions of great interest are also briefly treated, and there are the usual reports of excursions.

OBSERVATIONS AND CORRESPONDENCE.

The Permanence of Ocean Basins.

It seems desirable that I should say a few words in reply to Dr. Blanford's letter in the last issue (p. 639), and to Mr. Jukes-Browne's article in the preceding number of Natural Science (pp. 508-513).

Both these writers lay stress upon a supposed considerable modification of my views in my late paper. I reply that there is no real alteration; because the 1,000-fathom line was never adduced by me as an absolute and rigid boundary between the oceanic and continental areas, but as a general indication of their respective limits according to the best obtainable evidence. In proof of this, I may point out that even in the first edition of "Island Life" (p. 444) I refer to a submarine plateau at a depth of between 1,000 and 2,000 fathoms, which stretches southward from New Zealand towards the Antarctic continent, and suggest an ancient connection "with the Great Southern Continent by means of intervening lands and islands"; and in my "Darwinism" (p. 346) I define the permanence of oceanic and continental areas as meaning that, "while all of them have been undergoing changes of outline and extent from age to age, they have yet maintained substantially the same positions, and have never actually changed places with each other"; and again, at p. 347, after stating that the 1,000-fathom line "marks out, approximately, the continental area," I add: "There may, of course, have been some extensions of land beyond this limit, while some areas within it may always have been ocean; but so far as we have any direct evidence, this line may be taken to mark out, approximately, the most probable boundary between the continental areas and the great oceanic basins."

In my recent paper I have merely repeated and enforced these statements by showing how little real difference is made by carrying the possibilities of Continental extension, in rare cases, as far as the 1,500- or 2,000-fathom line. There is, therefore, no ground for alleging any departure from supposed "extreme views" which I formerly held, since the fresh arguments I have adduced show any great extension beyond the 1,000-fathom line to be in the highest degree improbable.

Dr. Blanford objects to my statement that the theory of the permanence of the ocean basins was "attacked" by him. Perhaps the word was not well chosen, and I should have said "criticised," but it was held by Mr. Jukes-Browne to be such a damaging criticism that (in private correspondence) he expressed surprise that I had not replied to its arguments in the new edition of "Island Life." But though the passage quoted by Dr. Blanford is not very antagonistic, there is much in the "Address" itself that is altogether opposed to my views. For instance, the writer argues in favour of a former land-connection between South America and Africa, which he says is "chiefly shown by tropical forms," though adding, "but these may have migrated far southward during warm periods;" and, later on, he speaks of a possible "girdle of land, chiefly in low latitudes, round nearly three-quarters of the globe, from Peru to New Zealand, and the Fiji Islands." But any such land-extension as this is so wholly inconsistent with the permanence of the great ocean basin of the Pacific, that to argue in favour of it is certainly to attack the theory of permanence. Again, a direct land-connection between South Africa, Madagascar, and the Peninsula of India is very strongly advocated, but the 1,000-fathom line shows us an actual though slightly circuitous connection by means of existing continental areas between Africa and India, and to hold that this would have been insufficient—why, I cannot

possibly understand—and to prefer to bridge across an ocean between 2,000 and 3,000 fathoms deep, in order to reduce the distance to about three-quarters of the other route, is again to hold views totally inconsistent with the theory of ocean-permanence.

Even in the passage from his "Address" quoted by Dr. Blanford, there is more antagonism than agreement; for I cannot admit that "there is no evidence whatever . . . that every ocean-bed now more than 1,000 fathoms deep, has always been ocean." If by "every ocean-bed" we mean the great oceanic basins as distinguished from deep seas within continental areas, and if we admit those very rare and limited encroachments of old continents on the margins of these oceans, which, as I have shown, I have always admitted, then I maintain that there is very strong evidence indeed of the permanence of all the great ocean-basins, and that the 1,000-fathom line still gives us the best indication of the general limits of the old continental areas. This is so, because the facts adduced in my last paper show that any extension of the continents into the oceanic areas much beyond the 1,000-fathom line would necessitate the submergence to great depths of many times their area of existing land.

Before concluding, I must briefly notice a very extraordinary claim of Mr. Jukes-Browne. He says ("Evolution of Oceans and Continents," p. 510):—"Those who oppose the doctrine of permanence say that the present continents are the outcome of a long series of geographical mutations," each phase being "an episode in a long process of geographical evolution."

But this is exactly what those who *uphold* permanence have always said. Dana taught the evolution of the American continent nearly forty years ago; Sir Archibald Geikie has discussed "Geographical Evolution" with permanence of continental areas; and I myself, following these great masters, have endeavoured to sketch out the process of this evolution and its results. This "process of evolution" necessarily implies permanence of position of the continental as regards the adjacent oceanic areas—the only permanence that has ever been postulated by myself or others, and it is simply amazing to find this very process now claimed as if it were the discovery and the distinctive teaching of those who oppose permanence!

In conclusion, I may say that, while admitting with pleasure the growing approximation of views on this subject, I cannot forget that it has been, and still is with many writers, the practice to assume former continental extensions across the great oceans in order to explain difficulties in the distribution of single genera or families; that geologists of repute have claimed the Dolphin bank in the Atlantic trough as the relic of a chain of mountains comparable with the Andes; that oceanic islands have been recently claimed to be merely the tops of submerged mountains, which can only be properly compared with the highest points of continents, and that a geological critic so late as 1879 considered the idea that the oceans had always been in their present positions "a funny one." If such extreme views are now less common than they were, I hope that I may, without presumption, claim to have had some share in bringing about the change in scientific opinion now in progress.

ALFRED R. WALLACE

MUSEUMS AND SPECIALISTS.

I DESIRE to raise a slight objection to the kindly notice of my account of the Fossil Crinoids in the British Museum (NATURAL SCIENCE, vol. i., p. 474), a notice that has just been reprinted verbatim in the *American Geologist* for October. It is stated that I accept as my rule the American maxim, that a museum is a collection of labels illustrated by specimens; but this gives an entirely erroneous impression. The paper referred to began with Professor Flower's words, "a well-arranged *educational* Museum has been defined as a collection, &c.," and it further stated in so many words that it dealt solely with the specimens exhibited for the diffusion of scientific knowledge.

The excuse for this protest is that the comments appeared shortly after a leading article on a similar subject, in which comparisons were instituted between the Storehouse-type of museum, disarranged and thick in dust, with its keys half

lost, and the Text-book type of Museum, with little exhibited but that little in perfect order, to the great disadvantage of the latter. Now, that article was certainly entertaining and contained a few pointed truths, but at the same time its bearing is not quite clear.

No museum yet built can possibly exhibit every specimen it contains, or ought to contain; and no rational zoologist, when he visits a foreign museum, expects to see all the individuals of his particular group set out (in dust or otherwise) under glass cases. Selection is a necessity, and this being so, it may as well be done on the best principles, and with the intention of interesting or instructing the majority of visitors to the museum, by whom, in many cases, and not by the "poor zoologist," the museum is, in fact, supported. As for the specialist, surely it is more to his convenience that the collection he wishes to examine should be kept in drawers, in perfect order and not dusty, so that the specimens can at once be taken out without disarranging the show-cases, and without giving more than five minutes' trouble to an attendant. Even when specimens are set out under glass, it appears, from the article in question, that the visitor must have the cases unlocked to be able to see them. Were the specimens in drawers and properly catalogued they could be found in less time than the unaided visitor would take to find them in his walk round. The article was obviously written by one who had suffered; let me remind him that the specialist visitors to a museum are in a very great minority, and let me suggest that next time he purposes visiting a museum with only one curator he should write and make arrangements beforehand.

F. A. Bather.

The Parasites of Cut Corks.

I shall be extremely grateful if some reader will kindly inform me what parasites, including fungi (if any), specially attack and destroy *cut* corks, as used, for example, in the manufacture of floats for fishing nets, for life-belts, and life-buoys, of course often immersed for long periods in fresh and in salt waters.

Many of these life-saving appliances are covered with painted coarse canvas.

The cork floats of fishing nets, with the nets, are boiled together in a solution of oak bark at intervals of about every three weeks. Other fishermen use catechu, tar, tannin, &c., and in Japan the unripe juice of persimmons (*Diospyrus virginiana*).

I am not aware that these corks are usually *superficially waterproofed*, for which process I am anxious to learn what materials are best suited. It is evident that such waterproof materials should be as light as possible, having a specific gravity less than that of fresh water, so as not to diminish the buoyancy of the cork used.

Both the Board of Trade and the Lifeboat Institution recommend that only *solid* cork should be employed for this work in the rescue of the drowning.

I should be further indebted for a complete list of aquatic animals and plants which attack corks long immersed or sunk in salt water and in fresh water.

I am also anxious to obtain specimens of old or "rotten" corks which have lost their powers of buoyancy in salt water, especially any which may sink more or less in water, with a view to exhibit these at the Royal Cornish Fisheries, (1893) Exhibition, of which I am one of the vice-presidents.

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Second Wind.

Second wind is due to the power of the respiratory centre to suddenly adapt itself to modified or changed circumstances and conditions. The respiratory centre is the nervous mechanism which presides over and guides the movements of respiration It is acted upon or influenced by stimuli reaching it from the respiratory muscles, the lung substance, and the blood circulating in and around the respiratory centre in the *medulla oblongata* (or the basal portion of the brain, which adjoins the spinal cord).

Any alteration in the composition of the blood will naturally react upon

this respiratory centre. Hence, by some physiologists breathlessness has been attributed to an undue proportion or excess of carbonic acid in the blood. To explain the phenomena of Second Wind, some writers have alleged that the respiratory centre can become accustomed to the increased amount of carbonic acid present—a condition which has been observed in many human diseases.

As regards deep-sea fishing, rushing the animal upwards and making the fish thus breathless, is due partly to fright, but still more to altered "atmospheric" and aquatic pressures, which cause the contained gases, including the carbonic acid, within the fish to expand and escape in increasing proportions. Evidently the nearer the hooked and suddenly-raised fish is drawn to the surface, the less is the amount of carbonic acid included within the animal's blood; its breathlessness is nevertheless augmented, thus clearly illustrating that extreme breathlessness may exist with a diminished and diminishing supply of carbonic acid. This fact seems to suggest that some cases of breathlessness may be explained on other grounds than those due to a supposed excess of carbonic acid in the blood.

I am anxious to learn whether domesticated and wild animals, especially those which are chased or hunted, have been observed to suffer from phenomena similar to those experienced in the so-styled second wind of man, which appears to be analogous to the so-called "blowing" of horses and hounds.

J. Lawrence Hamilton, M.R.C.S.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

M. Suchetet (*Bréauté, near Goderville, Seine-Inférieure, France*) is studying the facts and principles of hybridisation, and invites the co-operation of naturalists who can favour him with observations.

W. James (Liverpool).—The feathers of *Dinornis* were first described by W. S. Dallas, *Proc. Zool. Soc.*, 1865, p. 265, and *Ann. Mag. Nat. Hist.*, vol. xvi. (1865), p. 66.

J. L. R. (Northampton).—The references are unnecessary. Professor Cope's views on Evolution are summarised in his work "The Origin of the Fittest," published by Appleton, 1887.

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NOTES AND COMMENTS.

The Interior of the Earth.

A MORE or less interesting article, by the Rev. O. Fisher, entitled "The Hypothesis of a Liquid Condition of the Earth's Interior, considered in connection with Professor Darwin's Theory of the Genesis of the Moon," appears in the last number of the *Proceedings of the Cambridge Philosophical Society* (vol. vii., pt. 6, 1892). We say more or less interesting, because mathematical formulæ occupy a considerable portion of the paper, and these do not, as a rule, in themselves, interest students of Natural Science. Indeed, a serious regard for the published results of the application of rigid mathematics to the study of the earth, does not seem hitherto to have penetrated deeper than the superficial strata of the magazine article and the student's text-book. The dry flour of the mathematical mill makes excellent paste for holding together the *disjecta membra* of a popular lecture; but it never seems to have commended itself as digestible pabulum to the taste of the everyday geologist, who has his day's work to do, and needs something substantial and nourishing to do it upon.

In his "Physics of the Earth's Crust" (edition 2), Mr. Fisher claims to have proved that if the crust is as thin as some geologists suppose, and if the age of the world is anything approaching to what geological phenomena appear to indicate, then there must exist convection currents in the interior, which prevent the crust from growing thicker, by melting off the bottom of that crust nearly as fast as it solidifies. He made no suggestion in that work to account for such currents being maintained. At present, however, he thinks that the observations of Professor G. H. Darwin on bodily tides may furnish an explanation; for a certain amount of heat is generated in the interior of the earth—greater, in proportion, as we approach its centre—by

the action of the tidal couple upon the substance of that interior. "The whole heat generated, from first to last," says Professor Darwin, "gives a supply of heat, at the present rate of loss, for 3,560 millions of years."

Re-investigating, mathematically, "the effects of the tidal yielding of the earth, on a tide of short period, according to the canal theory," Mr. Fisher reaches the conclusion that on an earth with a liquid interior and a solid crust, ocean tides would exist, of the same type as those upon a solid earth. The amount of tide in the former case would be about two-fifths of the amount of what it would be in the latter. This he considers to be "a sufficient answer to the objection brought against the theory of internal liquidity, that in such a case there could be no measurable ocean tides."

He points out that the conclusion, that in a solid globe nothing short of a high degree of rigidity could sustain the weight of continents and mountains, disappears wholly in the light of Airy's hypothesis that the crust is supported in a state of approximate hydrostatic equilibrium on a yielding nucleus. Hence, all conclusions drawn from the supposed necessity of a high degree of viscosity, vanish with it; so that all numerical estimates founded upon this must be wholly inapplicable in the case of a liquid interior.

Mr. Fisher next goes on to show that on the tidal theory the whole estimated amount of heat generated in the earth remains the same, whether the question of time be considered or not. He holds that if we take this total amount of heat, as given above, it is difficult to understand how it can possibly have been dissipated. Under present conditions only a very small portion of the heat generated in this manner could be taken up by the ocean waters and radiated into space. "Unless ocean tides have been in operation for a length of time exceeding any estimate hitherto suggested, it does not appear probable that any considerable portion of the heat generated, according to the hypothesis of the moon being shed from the earth, can be got rid of by this means." Unless this heat is accumulating, it must have been got rid of by (1) "Conduction through the solidified crust; (2) by transference of internal heat to the surface by volcanic action; or (3) by conversion of heat into work against gravity, &c."

He then calculates that the amount of heat carried off by conduction through the earth's crust, even in 100 millions of years, must be quite inconsiderable compared to the full amount presumably generated in the interior. Next, as an extreme case of volcanic action, he shows that presuming the ocean to be four miles deep, and covering the entire globe, and that the whole of its waters were originally in solution with the magma of the earth, and are now cooled down to the freezing point, the total reduction of heat by this process would only use up 8° F. of the 3,000° F. to which the interior is estimated to have been heated; and, lastly, supposing heat has

been expended in producing elevations of strata, in inducing molecular changes in rocks, in shearing, &c., still the amount thus expended cannot represent more than a fraction of the whole, for the work of elevating a layer of granite-rock ten miles in thickness for a height of five miles, would represent the heat equivalent to a fall of temperature of only 1° F. throughout the globe.

Mr. Fisher concludes that there appear to be only two possible replies to the question as to what becomes of the estimated heat generated by this tidal action. Either the earth's crust solidified a very long while subsequent to the genesis of the moon, and so was able to radiate heat directly from its liquid surface into space; or else the moon was not thrown off at all, but was left behind, according to the nebular hypothesis. In this latter case, the whole amount of tidal action would not have been so great, but still quite sufficient to generate centrically an amount capable of maintaining those internal currents which the theory of a thin crust and a liquid interior appear to necessitate.

The Thames as an Agent in the Dispersal of Plants.

In the recently issued number of the Linnean Society's *Journal* ("Botany," vol. xxix., pp. 333—346), Mr. H. B. Guppy gives the results of his observations on the agency of the Thames and its tributaries, the Lea and Roding, in the dispersal of plants. Not only in autumn, but during winter and spring, seeds, seed-vessels, and other vegetable drift are constantly carried sea-wards, the months of January and February yielding the richest collections—a fact to be explained by the numerous checks to their progress. A constant wind, blowing at right angles to the river course, will cause a surface-flow across the stream, carrying out of the current much of the drift which is thus often caught in the flags near the side, or embayed for weeks in a sheltered hollow of the bank. In the tortuous channels of the lower Lea, where the current is slow, large collections may be found in the spring in some of the bends, especially after a long spell of east wind. A flood will leave much material stranded until the next one picks it up again. Eddies will detain the drift for days, and, when the tidal part is reached, there is no knowing how many times it may get carried to and fro; when a seed-vessel arrives off Gravesend, it has probably traversed the distance below Teddington weir many times over.

Mr. Guppy has worked the Thames from Richmond up to Sunbury weir. By using the tow-net, he says, opposite the weirs, and collecting the drift in other places where it had become embayed or caught in the flags, he has gathered in a day materials for months of work; not only seeds and seed-vessels are captured, but bits of stem and branches, from which, if provided with a node, the plant may often be reproduced.. Plants of *Scutellaria galericulata* and *Nasturtium* were raised in this way from small fragments. Shoots of forget-

me-not (*Myosotis palustris*) and other plants are present in numbers with shoots and buds of several of the Potamogetons and the arrow-head. Many grub-cases are found, and numerous other minute forms of life.

Collections from the drift of the Thames and Lea have much the same composition. From October, when seeds and fruits first begin to accumulate in any quantity, through the winter to February and March, there are always to be found floating in these rivers, usually in numbers, the achenes of *Ranunculus repens* and *R. sceleratus*; the nuts of *Lycopus europæus* and *Scutellaria galericulata*; fruits of *Atriplex patula*, and several species of *Rumex*; the mericarps of *Angelica*; the nuts of the alder and birch; the drupes of *Sparganium ramosum*, the carpels of the water plantain, and the fruits of different species of *Carex*. Less frequent are the black carpels of *Galium palustre*, and the seeds of the yellow flag. The Thames is distinguished from the Lea by the number of seeds of *Impatiens fulva* and the yellow-rattle (*Rhinanthus crista-galli*), while fruits of *Bidens* are more frequent in the Lea. Most of the seeds and fruits in the spring drift have floated through the winter; nuts of alder and birch, however, are continually dropping from September to June; those of the birch only float a few days, and yet are plentiful in the drift during February and March. Except the birch-nuts, all these seeds of the river drift germinate in the water in March and April, or even before, and in nearly every case the germination is completed at the surface, where the seedlings, as a general rule, float and thrive, developing the first few leaves, but never attaining a greater length than an inch or two, though if stranded on a mud-bank they readily strike and develop the full-sized plant.

The ice of an English winter was found by Mr. Guppy not to affect the germinating power of the seeds in our ponds and rivers. In the case of several of the above-mentioned seeds which had been cased in ice for twenty days, as large a proportion germinated as when they were not thus enclosed. In the shallow water of the Lea when the water was frozen to the bottom, the underlying mud was also frozen for an inch or so, coming up in a continuous mass with the ice; and shoots of forget-me-not taken up in this manner are still alive. Though the power of germination is unaffected, great numbers of the seeds and fruits sink to the bottom after the thaw, the ice affecting the vitality of the outer coverings or buoyant parts of the fruit, a process which is a direct aid to germination in the case of *Sparganium ramosum* and *Potamogeton natans*. The ice also sends to the bottom about half the fronds of the floating duck-weed; in fact, it may be generally stated that after a thaw the river is cleared of a large proportion of the drift.

When a germinating seed gets enclosed in ice, germination is arrested; but after the thaw the process is often rapidly completed, and a diminutive seedling prematurely discharged. This occurs, for

instance, in *Rumex conglomeratus*, but in the case of *Ranunculus repens* and *aquatilis* the process is often permanently checked. Some seedlings will develop the first leaves during the daily thaw, while locked in the ice at night.

The seeds and fruits of the river drift will, almost without exception, float for months in sea-water, and afterwards germinate. Fruits of *Sparganium ramosum* germinated after being afloat in sea-water for a year, and those also of many others after periods of from three to five months, the limit not of their buoyancy but of the experiment. The sinking of a seed-vessel in sea-water by no means involves the loss of germinating power of the seed ; on the contrary, it sometimes favours the process, as the sinking is due to loss of vitality of the outer coverings on which the buoyancy of the fruit nearly always depends, and the decay of these coverings is often a necessary step to germination. Thus nuts of *Potamogeton natans*, which float for months in fresh-water, sink after a week or two in sea-water ; until they are ready to sink they cannot germinate, so that sea-water quickly accomplishes what may take months in fresh-water.

As regards seeds already germinating that may reach the sea, in those of *Rumex* the process is arrested, and the protruding part of the radicle rots off ; but if after a week or two fresh-water is again reached, the process is recommenced, and an undersized seedling prematurely discharged, which, however, soon recovers, and develops the plumule. In the yellow flag the radicle soon rots off, and germination is not continued when the seed is restored to fresh-water. Rarely is the germinating seed but little affected, as in *Ranunculus sceleratus*, which will germinate in sea-water.

As regards seedlings, some such as the yellow water-lily, the water-plantain, and *Mentha aquatica*, are killed by sea-water in a day or two and sink. Others will float for a week or ten days and yet recover, though often much mutilated. The seeds of *Salicornia herbacea* sink even after a winter's drying, yet they germinate freely, and the seedling rises to the surface and thrives.

Mr. Guppy remarks on the absence in the river-drift of the seeds and seed-vessels of most of the familiar water-plants ; in fact, we find represented the plants that live on the banks rather than those of the water. With two or three exceptions the nuts of the pond-weeds are absent, as are the seeds of the yellow and white water-lilies, the fruits of the forget-me-not (*Myosotis palustris*), the water ranunculus (*R. aquatilis*), and even the nuts of *Polygonum amphibium*. Nor do we find the fruits of the hornwort (*Ceratophyllum demersum*) or the nuts of the bulrush (*Scirpus lacustris*). The missing seeds and fruits, however, have little or no floating power either in river or sea, and must be sought in the river mud. Now these plants have a very wide distribution, many being universal in fresh-water ponds and rivers, and yet they cannot of themselves cross the sea. It is to birds that we must look for the dispersal of many of our water-plants,

either by conveying them long distances in their crops or digestive canals, or less frequently in the mud sticking to their feet or plumage.

In conclusion, the author mentions some interesting experiments on a few plants. The frog-bit (*Hydrocharis morsus-ranae*) is largely propagated in this climate by buds, many of which float through the winter till spring, when they develop into the characteristic plant. Plants were raised not only from these, but from buds that had been enclosed some weeks in ice, or had sunk after floating a week or more in sea-water. The seeds sink both in fresh and sea-water, but would readily adhere to the plumage of a water bird by means of the gelatinous pulp in which they are surrounded when discharged from the fruit; they could thus be easily transported. The detached leaflets of *Cardamine hirsuta* are found floating in numbers in the drift of the Thames and Lea, and the ponds of Epping Forest from autumn to spring. From some which had been kept afloat all the winter plants were reared, and also from leaflets that had been several weeks in ice; but a week of sea-water destroys the reproductive power. Finally, as regards the duckweeds. Fronds of *Lemna minor* collected in the Lea in October remained afloat through the winter, and are now thriving; in fact the fronds were found during every month of the year, sometimes in quantity enclosed in ice. The seeds will germinate after floating a day or two in sea-water, but a week's immersion kills them. Sea-water kills most of the fronds, even after a day's floating; some, however, recover and in rare instances survive a week in the sea. In rainy weather they can withstand an exposure of one or two days, and might thus be carried some distance on a bird's plumage. *Lemna gibba* apparently disappears in the winter, and *L. polyrhiza* is represented only by small rootless fronds. *L. trisulca* seems to live through the winter in the Thames and Lea; and it was found enclosed in quantity in the ice of an Epping Forest pond after weeks of frost. The fronds of the three last species behave like those of *L. minor* in sea-water.

An Ancient Lake-Village in Somersetshire.

We have already had occasion to refer to the scarcity of remains of Lake-dwellings in England (*suprà*, pp. 40–43), and it is gratifying to be able to record a newly-discovered example in Somersetshire. In the *Times* of October 24, Dr. R. Munro calls attention to this discovery, which was made by Mr. Arthur Bulleid, of Glastonbury, and his observations are of so much interest as to be worthy of special quotation. The site is about a mile north of Glastonbury, on the road to the village of Godney; and, before excavation, the remains consisted of a number of low mounds, rising from one to two feet above the surrounding soil, and extending from 20 to 30 feet across.

Dr. Munro, who visited the spot in October, observed that close by a trench through one of the mounds, there were three oak beams, lying over a bed of decayed brushwood, and evidently *in situ*. Each of these beams contained either two or three mortise holes, and through some of them the top of an upright pile still protruded. Their resemblance to the well-known palisades of the crannogs of Scotland and Ireland was too striking to admit of any doubt. They were evidently part of the marginal structures of a large area of woodwork, but as yet of undetermined limits, over which several cottages with their respective hearths had been placed.

The composition of the mound was disclosed by a succession of well-defined strata of clay, charcoal, ashes, and decayed wood. The total thickness of these beds was 3 ft. 6 in., and within them were detected three separate hearths superimposed one above the other. These hearths were generally formed of large stone slabs placed over a bed of clay, or of small stones embedded in it in the form of a pavement. Underneath the whole was the common woodwork, whose surface here presented a neatly formed layer of round timbers laid close together and still retaining their bark.

So far as ascertained by trial diggings here and there, all these mounds appear to contain a fireplace, thus probably indicating separate dwellings or perhaps workshops; and as their total number amounts to between 60 and 70, extending over an area of some five acres, one can form some idea of the extent of the settlement.

The surrounding district is now richly cultivated, but, in looking over an old map of the date of 1668, Dr. Munro found that it contained a lake called the "Meare Poole," into which three streams debouched, and from which the site of the present discovery could not be far distant. That this lake had larger dimensions in earlier times is probable, so that when this settlement was founded the locality would have been a shallow lake or marsh. Strange to say, this map represents the district lying immediately on the north-west borders of the "Meare Poole," as inhabited by the Belgæ.

The articles at present found include bronze rings, fibulæ and one brooch; also a few decayed iron objects. Other articles formed of bone and horn are numerous. Pottery is abundant, but much broken. Remains of a quern were discovered, and a few other stone articles, including some flakes and cores of flint. Beans, wheat, rye, nut-shells, &c., were found, as well as bones, presumably of domestic animals. So far, however, as the excavations have yet disclosed the nature of its buried treasures, the result is of special interest, owing to the predominance among them of articles unquestionably belonging to the period known as "Late Celtic." Hitherto nothing indicative of Roman influence has come to light, neither coins nor Samian ware being among the relics; and, should this pre-Roman character be maintained, Dr. Munro considers that the complete excavation of the entire village becomes a national duty.

The Study of Living Protoplasm.

In Biology the study of life in its simplest manifestations continues to attract the attention of many investigators, and important progress is being made. Dr. W. Flemming, of Kiel, has a short paper in the *Anatomischer Anzeiger* (vol. vii., pp. 758—764), in which he combats the idea upheld by some authors, that the well-known network observed in the nucleus of certain cells is a false appearance due to accidental and secondary changes in the material during preparation. Dr. E. G. Balbiani (*Ann. Micrographie*, vol. iv., pp. 449–489, pl. iii.) has studied the nucleus from another point of view, namely, in reference to its influence on the growth and reproduction of the cell. Dr. E. B. Wilson (*Anat. Anzeig.*, vol. vii., pp. 732–740) has been experimenting on the developing egg of *Amphioxus*, tearing asunder the segments at various stages in the subdivision, and trying the effect of allowing these parts to continue their development separately.

Dr. Balbiani's investigations were conducted by cutting up the protozoan *Stentor cœruleus*, and observing the process of regeneration in the various pieces. He finds that wounds are soon obliterated, and that each fragment becomes a new *Stentor* whether or not a trace of the nucleus happens to be preserved. The merest trace of a nucleus accelerates repair, but, at the same time, the effect of its total absence is not appreciable until this repair is tolerably advanced. Even when present, indeed, the reconstitution of the nucleus is the last act in the process of regeneration. For the complete restoration of all the parts, however, Dr. Balbiani finds that the constant presence of a nucleus is essential: and he has often observed the death of incomplete specimens, marked by the spongy character of the protoplasm with water forming numerous small vacuoles. He has also sometimes noticed a curious tendency, during regeneration, to a multiplication of the organism by division: and he believes that the nucleoli are exclusively concerned in sexual reproduction.

Dr. Wilson's results are very curious. If an egg of *Amphioxus* that has just segmented into four parts be taken and subdivided into its components, each will usually continue to develop, and will eventually give rise to an oval free-swimming larva one-fourth the normal size, but otherwise quite ordinary in appearance. If this 4-celled stage be allowed to fall into two pieces, each half not infrequently develops successfully, and gives rise to an embryo of half the normal size. Experiments, however, seem to show that if the egg be split into pieces at its 8 celled stage, none of these segments do more than acquire cilia, and swim about actively in their simple state; while, sooner or later, all of them die. The same phenomenon has been observed by Dr. Driesch in Echinoderms.

Rabbits.

Mr. Miller Christy, in a communication "On the Extermination of the Rabbit in Australasia" (*Zoologist*, Nov.), refers to the remarkable periodical variation in numbers exhibited by the *Lepus americanus* in the Canadian North-West. He says that for several years in succession the animal becomes so scarce as to be quite difficult to obtain, increasing in the next few years to so extraordinary an extent as to become the most abundant mammal in the country. After the maximum of increase has been attained, the rabbits commence to die off, and, before many weeks are over, their dead bodies strew the woods in all directions, while a live rabbit is scarcely to be met with. Mr. Christy agrees with Sir John Richardson (1837) that the disappearance is due to an epidemic comparable to the "mouse-typhus" of Löffler. Professor Hind (1860) thought exhaustion following a severe winter accounted for the fact. But the numerous extracts Mr. Christy quotes from many writers who have paid attention to the subject, and more especially a brief newspaper paragraph which professes to diagnose the cases—swollen throat, followed by diarrhœa and death—seem to prove that the disappearance of the rabbits is due to disease. If this be so, and if the bacteriologists can find some virulent disease peculiar to the rabbit, by all means let experiments be tried for the wholesale extermination of the pest in Australia, where, judging from all accounts, the second source of the wealth of the colony, the sheep farm, is rapidly becoming a thing of the past.

Fossil Rhinoceroses.

In a recent memoir published in the *Bull. Soc. Imp. Nat. Moscou* (vol. vi., pp. 146–231, pls. iii.–v.), Madame M. Pavlow continues her elaborate researches on the phylogeny of the Ungulates, treating, in this fasciculus, first of the fossil Rhinoceroses of Russia, and then of the mutual relationships of all the members of the family. The species found in Russia are comparatively few, and as they are all identical with Western European forms, call for no particular remark.

In treating of the phylogeny of the family, the authoress publishes a genealogical table, in which the whole of the Rhinoceroses, both fossil and extinct, are derived from the genus *Systemodon* of the Lower Eocene of North America. That such a characteristically Old World group should have had a Transatlantic origin appears startling, although it is paralleled in the case of the Camels: and we are not at present prepared to admit that the case is proved. Madame Pavlow's genealogical table is open to a considerable amount of criticism, but since Professor H. F. Osborn informs us that he is at present engaged on working out the phylogeny of this group, we think it better to defer such criticism until his memoir shall have appeared. We may state, however, that in attempting to trace the origin of the

living Sumatran and Javan Rhinoceroses through *R. deccanensis* and *R. karnuliensis* of the Indian Pleistocene, the authoress is clearly in error, since, if these species had any lower cutting-teeth at all, it is quite certain that they must have been rudimental ones. It may further be added that, in so widely separating the two-horned Pleistocene Rhinoceroses of Europe from the existing African species, which they resemble in the absence of front teeth, we think that the authoress has decidedly carried the doctrine of "parallelism" to an extreme.

Late Flowering Plants.

While we write, the ivy is in flower, and bees, wasps, and flies are jostling each other and struggling to find standing-room on the sweet-smelling plant. How great must be the advantage obtained by this plant through its exceptional habit of flowering in the late autumn, and ripening its fruit in the spring. To anyone who has watched the struggle to approach the ivy-blossoms at a time when nearly all other plants are bare, it is evident that as far as transport of pollen and cross-fertilisation go, the plant could not flower at a more suitable time. The season is so late that most other plants are out of flower, but yet it is not too late for many insects to be brought out by each sunny day, and each insect, judging by its behaviour, must be exceptionally hungry.

Not only has the ivy the world to itself during its flowering season, but it delays to ripen its fruit till the spring, a time when most other plants have shed their seed, and most edible fruits have been picked by the birds. Thus birds wanting fruit in the spring can obtain little but ivy, and how they appreciate the ivy berry is evident by the purple stains everywhere visible within a short distance of the bush.

The ivy berry, however, has a soft seed with a large unprotected embryo, which is totally destroyed by digestion; yet the conspicuous glossy blackness is evidently employed to attract birds. How can birds be of use to a plant thus endowed? It seems probable that in this case, as with many other abundantly-fruiting plants, a large proportion of the fruit acts merely as bait and thus allows by accident a small proportion of the seeds to be transported uninjured. Anyone who has watched birds feeding from a superabundant supply will have noticed that a considerable number of seeds slip and drop uninjured, the bird not taking the trouble to search for them while there are plenty more on the bush. Thus probably are ivy berries dropped at the foot of the surrounding trees and walls on which the birds perch.

The ivy is therefore a plant which will always tend to occur abundantly as far north as the climate will permit it, for the further it travels north the less the competition from other plants, and the greater the competition of the insects and birds for its favours.

The butcher's broom—that singular exotic-looking plant whose flowers seem to be unvisited by any insect, and whose conspicuous scarlet berries are apparently untouched by any of our native birds—is also now in bloom. But most of our other winter-flowering plants are only exceptional individuals of various hardy species, which can flower wherever and whenever a sheltered nook or glimpse of sunshine can be found.

Tea.

There are various ways of preparing and using the leaves of the tea-plant (*Camellia theifera*). In the *Kew Bulletin* for October Mr. Stringer, Acting Vice-Consul at Chiengmai, describes the mode in vogue among the Laos of Upper Siam. The preparation is known as "Mieng."

The "Pa mieng," or tea forest visited, was on the north-western slope of the mountain range near Chiengmai, where the trees were growing in abundance and close together. The tea-pickers stated that new plants were obtained from seed. There are four seasons for picking the leaves, the first in the eighth Lao month (April–May), the second in the ninth (May–June), the third in the twelfth (September–October), and the fourth in the second month (November–December). The leaves of the first picking are considered the best; the third and fourth pickings yield smaller crops. The young leaves only are taken, the upper part being nipped off with the fingers so as to leave about a quarter still attached to the tree, as if the whole leaf is picked the young branches die.

The picking is done in the early morning, each handful is tied tightly together as it is gathered, and about mid-day the leaves are steamed. The little bundles are placed in a wooden cylinder 2 feet long by 15 inches diameter, at the bottom of which is a network of strips of bamboo. The cylinder is hung over an earthen pot or chatty holding water, and heated over a furnace which consists of a hole in the ground. A wet cloth is placed round the edge of the pot, so that all the steam goes up through the cylinder. When thoroughly steamed it is left to cool, and then taken out and re-tied, and the "Mieng" is ready for use. If, however, it is to be kept for any length of time, it is well trodden down in small pits dug in the ground, and lined with large leaves, to press out as much of the liquor as possible. It is then covered with leaves, and large stones are put on top, and it is kept thus buried for fifteen days. "Mieng" thus prepared is said to last for two years. If wanted for immediate consumption, it need not be buried, but is packed in large baskets, and heavy stones put on it to press out the liquor. The dark reddish-brown liquor left in the pot after steaming is used as food by the Laos and Siamese. The habit of chewing "Mieng" is almost universal among

the Laos; a quid of it seems indispensable for men engaged in hard work such as poling or rowing.

The price of the prepared article at the village visited was one rupee (1s. 3d.) for twelve packages, a package containing ten bundles or handfuls. In Chiengmai it was dearer, seven to ten packages being sold for the same money.

Animals once supposed to be extinct are still being discovered in Australia, the refuge for so many ancient forms of life. The latest announcement is the fact that all the freshwater herrings observed by Mr. Ogilby in certain rivers of New South Wales are doubly armoured, *i.e.*, they have a row of scutes on the back resembling the ordinary armature on the ventral border. In the last number of the *Ann. Mag. Nat. Hist.* (ser. 6, vol. x., p. 413), Mr. Smith Woodward points out that this is a peculiarity exhibited by most of the extinct herrings of the latter part of the Cretaceous and the early part of the Tertiary period. They are assigned to the genus *Diplomystus*, and have been found fossil in Brazil, Wyoming, the Isle of Wight, and Syria. Such herrings do not appear to have hitherto been detected in any part of the world in rocks of later date than the Oligocene, and Mr. Ogilby's discovery in the freshwaters of New South Wales is thus of great interest.

We are glad to find that there are still some anatomists who read Sir Richard Owen's works. In the *American Naturalist* for November, Professor Victor Carus points out that Owen described the cervical vertebræ in *Ornithorhynchus* as having no zygapophyses, while in *Echidna* only the atlas has these processes. The reason for the Professor's letter is that Dr. .Georg Baur has recorded this observation as new (*Amer. Nat.*, 1892, p. 72), and commented on the fact that Flower and Lydekker have not noticed it. There are fashions in "authorities" as in more commonplace affairs, and the time will yet come when the great pioneers in Natural Science are duly respected.

In the November number of the *Ann. and Mag. Nat. Hist.*, the Rev. F. O. Pickard Cambridge describes two new forms of British Spiders. The one, *Tmeticus simplex*, was found in abundance in a damp cellar at Cannock, Staffordshire, and the other, *Leptyphantes plumiger*, most probably came from Hyde Bog, near Wareham, Dorset. Figures of both species are given.

A brief memoir of the late Thomas Roberts appeared in Natural Science for March (p. 76). We have now the satisfaction to announce that the Essay on "The Jurassic Rocks of the Neighbourhood of Cambridge," for which he gained the Sedgwick Prize in

1886, has just been published (Cambridge: C. J. Clay & Sons, price 3s. 6d.). This Essay gives a very full account of the Oxford Clay, the Corallian Rocks, and the Kimmeridge Clay of Cambridgeshire and Huntingdonshire. The beds are compared also with their equivalents in other parts of England and on the Continent. The great value of this work results from the care with which the various palæontological horizons have been worked out, so that the lists of fossils and other particulars will be of great service to workers in Jurassic geology.

Those of our readers who have from time to time read with interest reports of the researches of Dr. F. A. Forel on the Lake of Geneva—researches dealing not merely with the geographical details, but with every problem of its physical history—will be interested to hear that he has now collected and issued in book form the results of his labours for upwards of fifty years. The book, of which volume I. has quite recently been issued, is entitled "Lac Leman, Monographie Limnologique," a term which Dr. Forel explains to be equivalent to oceanography, and which he has coined for use when speaking of fresh-waters. The work is illustrated with numerous charts and maps. Dr. Forel cannot believe that the basin of the Lake of Geneva was excavated by the old Rhone glacier: he thinks it is merely an ordinary valley affected by earth-movements.

M. J. Girard, in the *Revue de Géographie* for October, concludes his series of interesting papers, entitled "Études de géographie littorale." In his first paper (August) he treated of "The Movements of Sands," and showed the changes constantly taking place in bays and harbours. He alludes to the effect of small island obstructions in shifting currents and thereby altering sandbanks. In his second paper, "The Genesis of Deltas," the examples are drawn from the Rhone, the Mississippi, and the Hoang-Ho, and two very interesting plans are given of the Rhone delta. In the third and concluding paper M. Girard deals with "Fjords and the Glacial Epoch," and again illustrates his remarks with plans and pictures which are pleasantly new to the reader, not the well-worn figures one is so accustomed to see.

In the *Naturwissenschaftliche Wochenschrift* for November 6, Professor A. Nehring describes the fruit of an extinct plant found in certain peaty deposits at Klinge, near Cottbus. This "sausage-shaped" fruit he names *Paradoxocarpus carinatus;* it will be of interest to English botanists, for the same plant occurs in the Cromer Forest-bed. The other plants found at Klinge are all, with the exception of *Cratopleura helvetica*, common species living in Germany at the present day.

BLACK Burmese rice, according to Professor Church's analysis, published in the *Kew Bulletin*, is unusually rich in the flesh-forming albuminoids and in oil. Compared with other Indian rices, the percentage of true albuminoid in the Black Burmese is as 8·5 to 6·7, while the percentage of oil reaches 2·2 as compared with ·6, the average of the other Indian forms. The nutrient value of the Burmese rice is greater, and is much nearer to that of a complete food than is the case with the common kinds of Indian rice, which are also rather poorer in phosphoric acid. The dark appearance of the grain is due to a pigment contained in the integument, which proves to be identical with that to which is ascribed the red colour of grapes, black currants, the florets of the crimson dahlia, the leaves of the copper beech, and many other leaves, flowers, and fruits. If a few grains of the black rice are covered with slightly acidulated spirits of wine, the pigment dissolves with a magnificent crimson colour.

A NEW mineral named Geikielite, in compliment to Sir Archibald Geikie, has lately been described before the Mineralogical Society by Mr. Allan Dick. In composition the mineral is a magnesian titanite, and it was brought from Rakwana, in Ceylon, by Mr. J. Baddeley. It was found in the form of a pebble, and was taken, with a number of other pebbles, to the Museum at Jermyn Street, and submitted to Mr. A. Pringle for examination. The particular specimen, now named Geikielite, was found by Mr. Pringle to differ in specific gravity and other "external characters" from any known mineral, and it was consequently handed to Mr. Dick for chemical investigation.

Among the store of pebbles collected by Mr. Baddeley, another new mineral was afterwards recognised by Mr. L. Fletcher, and this (as he announces in *Nature* for October 27) proves to be crystallised zirconia. He proposes for it the name of Baddeleyite, as the natural occurrence of this oxide of zirconium has not previously been noticed.

THE "Dictionary of National Biography," of which the first volume was published in 1885, has now reached the thirty-second volume, and the noteworthy men and women have been recorded as far as Leigh. In the volume last issued (1892) we note that there are, among many others, short memoirs of Daniel Lambert, Edwin Lankester, Dionysius Lardner, R. G. Latham, John Lee (of Hartwell), and John Edward Lee. In glancing through the previous thirty-one volumes, we come to the conclusion that the work has been very thoroughly done; and that very few names of any importance will be found wanting. We miss, indeed, a reference to Captain Thomas Brown (the well-known Conchologist); no mention is made of John Billingsley (of Agricultural fame), or of C. R. Bone, a lithographic artist (of Palæontographical fame), but a reference to these omissions will perhaps serve to indicate the general completeness

of the work. Commenced under the editorship of Mr. Leslie Stephen, this great work is now under the direction of Mr. Sidney Lee, aided by a large number of contributors; and it is calculated that the series will be completed in about 50 volumes. Up to the present time the volumes have appeared at quarterly intervals with unbroken punctuality; and the completed work will, it is estimated, contain at least thirty thousand articles. Lists of the names intended to appear are inserted from time to time in the *Athenæum*, so that the Editors have spared no pains to render their undertaking as complete as possible.

Islington, although the most thickly-populated parish in the United Kingdom, remains far behind many smaller parishes and provincial towns in the matter of educational institutions. It possesses no Free Library, no Public Museum, and no Technical Institute. Efforts, however, are being made to establish a Northern Polytechnic Institute, and a meeting was held on November 10, in the Highbury Athenæum, to further the project. The Charity Commissioners have promised an endowment of £1,500 per annum, and the Trustees of the Parochial Charities a further sum of £500 per annum; but these endowments, for maintenance, will not of course be paid until the buildings are erected. A freehold site of nearly two acres has been acquired near the Holloway railway-station, and upon this site it is proposed to erect a large hall, to be available for lectures, concerts, meetings, industrial exhibitions, &c.; a gymnasium, museum, club-rooms, library and reading-rooms, class-rooms, laboratory and workshops. To complete the work £15,000 is still wanted, but surely so important a parish will not be content to remain any longer "behind the times," and will heartily contribute the sum, which amounts to less than a shilling per head of the population.

There are some excellent cautions from the pen of Mr. W. F. Kirby in *Science* for October 28 on "Type-specimens," and "Type-figures" in Entomology. Mr. Kirby points out the difficulties to be encountered in identifying the specimen on which the original description was based, and remarks on the probability of the replacement of a damaged type by a better specimen, not always of the same species. The possible interchange of labels, so likely to occur in the insecta, where labels must always be more or less loose and unfixed, is also referred to, and some words of praise for the general accuracy of Fabricius' work close a too brief article.

Apropos of Mr. Britten's article in the October number of Natural Science, those who wish to follow the subject further may be glad to know that Botanical Nomenclature formed the principal

topic of the meeting of the Biological Society of Washington on October 22. Three papers were read and will no doubt soon appear in the publication of the Society:—The present status of Botanical Nomenclature, by F. C. Coville; Report on the Botanical Congress at Genoa, by George Vasey; and some Controversial Points in Botanical Nomenclature, by George B. Sudworth.

We are sorry to have to include our sparkling contemporary, *The Idler*, among the curiosities of journalism incautiously handling scientific subjects. In the November number (p. 381) the Huckleberry Finn rattlesnake story is revived, the scene being laid in India. Unfortunately, the author has either misunderstood his artist, or the artist has been jesting at the author; for while Mr. Jerome has used a python as his murderous subject, the artist has carefully drawn a cobra, which bites, but does not constrict. The confusion between the picture and the text is most amusing to anyone acquainted with the rudiments of natural history.

We have had many of these sad examples of popular science lately, but a recently-started boys' paper contains one which is calculated to make one ask whether cheap Natural History be not a failure, and popularising played out. For when "a lover of animals" "saunters" round the Zoo, and in one short page hopelessly mixes the hippopotamus and rhinoceros, and speaks in terms of warm admiration of the "otter's" skill in felling trees, one pities the unfortunate boys who are expected to be elevated and instructed thereby. A little more confusion of dentitions and they will think that that of the Primates is calculated pre-eminently for attack, and settle their differences with fangs instead of fisticuffs.

Professor Valentine Ball, of the Science and Art Museum, Dublin, is preparing for publication a map showing the distribution of the fossil mammalia in Ireland.

An excellent portrait of the Rev. Octavius Pickard Cambridge, F.R.S., appears in the November number of the *British Naturalist*.

There are at present to be seen in the Western Aviary at the Zoological Gardens a pair of doves (*Macropygia*) which are interesting from the point of view of mimicry. Their brown-and-black-barred plumage, long graduated tails, and short legs, with a tendency to fulness of feather on the thigh, immediately suggest a cuckoo; only it is very difficult to see what could cause a cuckoo to mimic a dove, or *vice versâ*.

I.

Further Observations on the Permanence of Oceans and Continents.

I.

THE literature relating to the much-debated permanence of ocean basins increases fast, and two fresh contributions have lately been made to it. In the *American Journal of Science* for September (vol. xliv., p. 183), Mr. McGee writes on "The Gulf of Mexico as a Measure of Isostasy"; while in the *Quarterly Journal of the Geological Society* for November (vol. xlviii., p. 519) Mr. R. J. Lechmere Guppy treats of "The Tertiary Microzoic Formations of Trinidad, West Indies." The authors, however, approach the subject from totally different points of view, and it may be of interest to compare their results. The subject is an interesting one, but the unbiased geologist will be much perplexed with the conflicting opinions expressed by the various authorities.

Mr. McGee starts on the assumption that the earth's surface is in a state of nearly stable equilibrium—hydrostatic equilibrium he calls it, but we do not like the term to be applied to so imperfect a fluid as the earth's crust. This equilibrium is indicated by the fact that the crust is densest under the sea and lightest under mountain ranges; therefore, as long as there is no transfer of matter from place to place the dense sea-bottom will balance the lighter mountains. Rivers, however, gradually transfer the land to the bottom of the sea; consequently the loaded sea-bottom will continually subside, perhaps carrying with it the adjacent flat shore, while the unloaded continental areas will slowly rise.

As an abstract proposition no fault can be found with this argument; but no quantitative measurements of the earth's density sufficient to prove any near approach to a hydrostatic equilibrium have yet been made. When, also, Mr. McGee points to estuary after estuary as showing recent subsidence, we cannot help observing that he does not always clearly distinguish between loss of land from marine erosion and true subsidence. He also makes no allowance for the slow compression that estuarine deposits are always undergoing, as the organic matter decays and the particles pack closer. The historical evidence for the post-Roman subsidence in Holland is

scarcely satisfactory, and sand-dunes do not necessarily indicate subsidence. So much of Mr. McGee's paper is taken up with the attempt to strengthen his case by the appeal to evidence of doubtful value, that we cannot ignore it; the Gulf of Mexico, however, is the region with which he is primarily concerned. To that area we will now turn.

The Gulf of Mexico is a nearly land-locked sea, which receives the detritus brought by the Mississippi and other rivers and derived from the erosion of about half the North American continent. This detritus, however, is nearly all deposited in the northern half of the Gulf, so the area of degradation is about 6½ times the area of deposition. If the theory be correct, that the rise or fall of an area depends on the weight removed or piled upon it, then the continent of North America must be a rising area, and the northern half of the Gulf of Mexico a subsiding one.

Such is probably the case, but there is some danger of confusion between cause and effect. It is quite as logical to argue that the land is denuded because it rises, as that it rises because it is denuded. We may also observe that if from any cause the land sinks beneath the sea-level, the part that subsides most rapidly necessarily becomes first a bay and then probably an estuary, for river-valleys will tend also to follow lines of depression. Suppose, on the other hand, that the floor of the Gulf of Mexico were to rise; the Mississippi would discharge further to the east, beyond the West Indies, and we should be told that the depression beyond was caused by the accumulation of sediment. Unless the theory of stable equilibrium be of merely local application, it is difficult to understand how land once beneath the sea-level can ever rise again; yet we know that extensive marine deposits often occur high above the present sea-level.

Mr. Guppy, writing of a district similar in many respects to that dealt with by Mr. McGee, treats the subject from a point of view altogether different. He is concerned mainly with the geological and biological evidence, which go to indicate that what is now dry land was once a deep sea, and the facts relied on are these:—Trinidad possesses in its southern half certain Tertiary formations, which from the character of their fauna seem to indicate a depth of at least 1,000 fathoms. These deep-water deposits are absent from surrounding areas; and from their partial occurrence, combined with the evidence obtained from the geology of the surrounding islands and coast of South America, Mr. Guppy arrives at these conclusions. During the Cretaceous and Eocene periods there was a sea having a depth of 1,000 fathoms or more in the area now occupied by the microzoic rocks of Trinidad. During the same period the northern mountains formed probably an unbroken range with the littoral Cordillera of Venezuela, and constituted the southern boundary of a Caribbean Continent. These mountains may be regarded as one of those "stable areas" which have never been submerged since Palæozoic

times. The Orinoco was then in existence, but brought no appreciable sediment so far from what was then the mouth of the river.

The Orinoco seems, therefore, to have discharged its sediment into a land-locked sea greatly resembling the existing Gulf of Mexico. If Mr. Guppy's account of the geology be correct, the estuary of the Orinoco and the Gulf of Paria ought, on the theory adopted by Mr. McGee, to have formed a subsiding area ever since early Tertiary times, for it is over that area that the sediment has been deposited. Such may have been the case, though we have as yet no direct evidence of the fact. When, however, we read that the southern half of Trinidad is occupied by deposits formed at the great depth of 1,000 fathoms, while the northern half has never been submerged since Palæozoic times, it is not easy to reconcile this rise of the sea-bottom with Mr. McGee's theory.

The views of Messrs. McGee and Guppy, as we remarked, have only quite lately been propounded. If we refer to literature a little earlier, however, there are numerous papers by other authors with a direct bearing on the same subject; and we may allude especially to four papers by Professor James D. Dana,[1] in which the permanence of the continental area of North America since the beginning of Palæozoic times, at least, seems to be satisfactorily proved. "The facts illustrate strikingly the great truth that the earth's features, even to many minor details, were defined in Archæan time, and, consequently, that Archæan conditions exercised a special and even detailed control over future continental growth. The extension of North America to the most eastern point of Newfoundland, and beyond it, was determined in this beginning time; and likewise, that of the European Continent to the Hebrides, in front of the Scandinavian Archæan area." The main result is briefly summed up by Professor Dana in the following letter, while Dr. Blanford and Mr. Jukes-Browne add some further comments from their point of view.

II.

With regard to the discussion on the Permanence of Oceans and Continents now proceeding in Natural Science, I may say that the argument from North America is brief: that the successive formations, from the Archæan onward, follow one another quite regularly over the surface that lies outside of the northern Archæan area, and bear

[1] "Areas of Continental Progress in North America, and the Influence of the Conditions of these Areas on the work carried forward within them," *Bull. Geol. Soc. America*, vol. i., 1889, pp. 36–48.

"Archæan Axes of Eastern North America," *Amer. Journ. Sci.*, vol. xxxix., pp. 378–383, 1890.

"Rocky Mountain Protaxis and the Post-Cretaceous Mountain-making along its Course," *ibid.*, vol. xl., pp. 181–196, 1890.

"On Subdivisions in Archæan History," *ibid.*, vol. xliii., pp. 455–462, 1892.

evidence throughout of shallow water origin; and that in this way the continent was extended from the Archæan southward, south-eastward, and south-westward.

The evidence as far as the eastern half of the continent is concerned was clearly manfest before 1845 (my first paper on the origin of continents was published in 1846 in the *American Journ. Sci.*, 2nd ser., vol. ii.), by which time the States of Connecticut, Massachusetts and New York had been geologically surveyed, and their final reports published, including a general coloured geological chart of the country by James Hall; and, besides, incomplete surveys had also been made of some other States. Further, the Professors W. B. and H. D. Rogers had so far completed the surveys of Pennsylvania and Virginia that they brought out in 1842 (*Trans. Assoc. Amer. Geol. and Naturalists*) their admirable paper on the Appalachian Mountain system, their flexures, &c. They showed that the flexing, faulting and uplifting for the 1,000 miles of their length was a sequel to the long series of Palæozoic deposition, a folding of the strata from the bottom (the Cambrian, as now called) to the top, confirming all other evidence that the strata were continental formations, and the work, regular continental growth.

Afterwards the earlier Mesozoic (that is, the Jura-Trias), added only brackish water or fresh-water beds, along some portions of the Atlantic border; and later the Cretaceous and Tertiary followed, finishing the continent to its eastern limit.

Similar facts complete the argument for continental permanence as regards the western half. By permanence, as I have always said, is meant, not the absence of deep subsidences, or of deep continental waters at times—say 2,000 or 3,000 feet in the interior, or on the borders, but the non-occurrence of oceanic depths and alternations with oceanic conditions.

JAMES D. DANA.

New Haven, Oct. 14, 1892.

III.

I DO not wish to continue the discussion on this subject, but I think that Dr. Wallace has not, in two instances at least, in his letter published in the November number of NATURAL SCIENCE, quite understood my arguments.

Dr. Wallace states that he cannot understand why the connection between Africa and India by means of existing continental areas would not suffice for the explanation of certain relations between the faunas and floras of India and the Mascarene Islands, and why I should prefer to bridge an ocean between 2,000 and 3,000 fathoms deep, in order to reduce the distance slightly. I may say that I do not regard the question of distance as material; and the majority

of the assumed zoological and botanical migrations, though not all, might have taken place equally well by either route, but the geological evidence appears to me only consistent with the former existence of a belt of land across what is now in places deep sea.

This geological evidence is, briefly: (1) That South Africa, India, and Australia, in late Palæozoic and early Mesozoic times, formed parts of one continental area, and that this area was separated by a wide sea, probably of considerable depth, from Northern Asia, Europe, and North America, which were also connected by land; (2) that in Cretaceous times part of Western India, a little north of Bombay, and also the South Coast of Arabia, were part of a sea that extended over a wide area in South-western Asia and in Europe, and that the Khasi Hills in North-eastern India, Trichinopoly, south of Madras, and Natal, in South Africa, were on the shores of another sea, divided from the first by a land barrier; (3) that, as shown by Neumayr, there are indications of this same land barrier in Jurassic and Neocomian times. I do not see how these facts can be explained by land connections within the present 1,000-fathom limit, and as it has been shown that a depression of part of the old barrier, the Mozambique Channel, to a depth of upwards of 1,000 fathoms, has taken place within comparatively recent times, almost certainly since the Miocene, and probably since the Pliocene period, it is not improbable that another part of the old barrier may have undergone depression to a greater depth, even to 2,000 or 3,000 fathoms in places, since the Cretaceous or Eocene. It must not be forgotten that the connecting barrier, which I believe to have once existed, is represented by a tract of ocean still dotted over with islands and shoals; that the soundings are far from sufficient to show the true contour of the ocean basin; and that there is, so far as I am aware, no geological or biological indication of probable land connection in Palæozoic or Mesozoic times in the direction preferred by Dr. Wallace.

The other point is of no great importance. Dr. Wallace says that my suggestion of a possible Mesozoic girdle of land, chiefly in low latitudes, from Peru to New Zealand and the Fiji Islands is inconsistent with the permanence of the great ocean basin of the Pacific. He must, I think, have overlooked the circumstance that the supposed girdle in question was explained as passing through Australia, India, Madagascar and Africa, and not across the Pacific Ocean (*Proc. Geol. Soc.* 1890, p. 106).

The real difference between Dr. Wallace's views and mine is, I think, this, that Dr. Wallace regards ocean permanence as an established law, only to be disregarded on the clearest evidence, while I look upon it as a theory supported by some important and valuable data, but by no means proved; and I contend that there is ample proof that even if the law of permanence prevails it is not universal. As a result of this, I think that every questionable case must be judged upon its merits, and if the evidence tends to show that land formerly

existed where deep sea is now, or *vice versâ*, that evidence must not be summarily dismissed because it is opposed to a theory that, as it appears to me, is very far from being satisfactorily established.

W. T. BLANFORD.

IV.

PERMIT me to explain the passage in my article on this subject which Dr. Wallace characterises as "an extraordinary claim." When I wrote that "the present continents are the outcome of a long series of mutations," each phase being "an episode in a long process of geographical evolution," I had in my mind a very different kind of evolution from that assumed by Dana with respect to North America. He regards the surface exposure of the Pre-cambrian rocks in Canada as the nucleus of the continent, and believes North America to have been built up by additions to this nucleus. In the case of America, there does seem to have been a process of building up by additions during Neozoic times; but I maintain that neither America nor any other continent dates back as a distinct continental plateau to Palæozoic times.

The evolution I referred to was the gradual evolution of continents and oceans together out of a more generalised state of geography. I look back to a time when the physical features of the earth's surface were less accentuated than they are now; when there were neither oceans nor continents, but a more equal distribution of land and sea all over the globe.

I cannot see that Dr. Wallace's argument about the relative displacement-capacity of continental masses and oceanic waters proves the continents to have maintained the same positions from the earliest times. Dr. Wallace evidently fails to see the force of my reply; all his argument proves is that, *if the volume of water has been always the same*, the total area of land at any period cannot have been much larger than it is now, but in my article I showed that it was unsafe to assume that the volume of ocean water is a constant quantity.

Again, what does Dr. Wallace mean by the great ocean-basins? and why does he object to the view that the Dolphin Ridge has once been land? To me it does not seem rational to speak of the Atlantic as a single *ocean-basin*; it is distinctly a double basin, and I can see no reason why a large part of the Dolphin Bank should not have been land, say in Triassic or Permian times.

Dr. Wallace must remember that the geologist deals with a length of time that reaches back far beyond the age of the modern genera and families of terrestrial animals, and even if the distribution of such animals can be explained by means of comparatively small geographical changes, it is because most of these families do not date

back beyond the beginning of Tertiary times, and are no guide whatever to the geography of Palæozoic times. Geologists must claim a free hand to draw conclusions from the geological evidence as regards Pre-tertiary epochs untrammelled by any inferences derived from the distribution of modern animals.

On the subject of oceanic islands I shall have something to say elsewhere.

A. J. Jukes-Browne.

II.

An Evolutionist in East Africa.

IT was the late Richard Jefferies, I think, who said that one of the greatest charms of Gilbert White was that he had no theory: he was not full, while he observed, of evolution, degeneration, and the like. Yet under the present circumstances, it is hardly possible to observe at all without ultimately testing the bearing of some of one's observations on the problems that constantly agitate biology, and during my recent short stay in East Africa I came across some facts which seemed to me worthy of record in this connection.

Shakespeare has observed "how use doth breed a habit in a man"; and certain observations I was enabled to make during my voyage out would seem to suggest that it has a similar effect upon lower bipeds. When arriving on board, the ship's ducks looked miserable enough to give rise to a proverb; they were unclean, and had not yet accommodated themselves to the resting-place afforded by the raised barred floor of their coop. Their squalor disappeared by washing pretty frequently; and before the end of the voyage, I noticed that when they were let loose on deck after this operation several of them, being so used by this time to perching, selected anything handy, like a rope or a pipe, as a standing-place. On reaching Zanzibar, I had a further instance of the versatility of the mallard. Domesticated here, in a climate unnatural to it, and where almost all the water accessible is extremely salt, this bird has adopted terrestrial habits, and runs actively about on the mud of a tidal swamp under a tropical sun at mid-day, retreating from the water instead of taking refuge in it when approached. Even on the sandy ground among the huts where there was no sea-water, these ducks appeared comfortable and healthy enough, so that it is quite possible that *Anas boschas* will supplant the Muscovy duck (*Cairina moschata*), the usual domestic duck on this coast, which is naturally less aquatic and a tropical bird. What modification may take place in consequence of this change of habit should be an interesting question. All that can be said at present is that the Zanzibar mallard seems, if anything, slighter and more erect than the European domestic duck, and that the males were, in the first week in August, mostly in full plumage in Zanzibar, while at home at that time one might vainly seek in fen or farmyard for a drake in complete

masculine attire. The Muscovy duck itself varies somewhat from the type we are accustomed to in England. I have seen specimens with the bare red face enormously developed into warty caruncles rising on each side over the eye above the level of the crown, like the eye wattle of a Barb pigeon, and extending to the base of the lower mandible. Yet others, apparently adult, had the red confined to a small area at the base of the beak, and showing little, if at all, round the eyes. With these there were specimens with an average development of red skin, so that the variation in such different directions is difficult to account for.

The fowls met with were equally interesting. In Naples, at Port Said and Suez, and in East Africa, they showed signs of complete panmixis in the various colours they displayed; yet the form and comb were in every case characteristic, suggesting forcibly that many of our breeds have in the first instance made themselves, as it were. In Naples, most of the hens to be seen about the street were of the Leghorn and Minorca type, carrying, whatever their colour, a large "single" comb, lopping over to one side. The fowls from Port Said and Suez were barely of the size of Hamburgs, meagre and skinny, with erect tails, and combs which were bifid either posteriorly or altogether, while of moderate size. The hind toe in these fowls was also often bifid. In Zanzibar the prevalent breed of fowl was the Malay, tall and gaunt, with a neck so closely feathered as to suggest, in the hens, a heron's, and the comb small in the cock and a mere rudiment in the hen. Elsewhere on the African coast there are fowls quite European in type, except for the hens' rudimentary combs; I therefore think the Malays are of Indian origin. A very ridiculous-looking variety which occasionally occurred, has the long neck of the ordinary breed without the long legs, these members being unusually short. Tailless fowls were common, but the tail seemed to be merely very short or soft, the feathers being defective, not the caudal vertebræ, as in the rumpless breed. Frizzled specimens, with the feathers reversed, might also be seen. The fowls appear to be very scantily fed, and no doubt become somewhat predaceous, as I saw one carrying off a snake about its own length, dead, and so mangled as to be useless as a specimen. With the freedom of breeding that exists among the poultry here, one would expect reversion, but as a matter of fact the colour of *Gallus bankiva* is not by any means that most commonly seen.

The goats are as interesting as the poultry, on account of the great variety in colour and form they exhibit. Some are extremely antelopine in colour and markings; and as to form, I once saw white specimens with a head and neck so gazelle-like, that I at first mistook one for an albino gazelle. On board the "Juba" there was one of the smooth-haired African sheep, which in its close brown coat also recalled an antelope. A collection of the horns of the Zanzibar goats

could not fail to be instructive, as exemplifying the variations of a domestic species, since they present astonishing differences. The pariah dogs of this coast much resemble a smooth-coated Dingo which once lived in the Zoological Society's Gardens, but they are smaller. The normal colour appears to be yellow, but others occur; I have seen partially drooped ears and a curled tail, but, of course, mixture with European dogs now and then occurs.

A common pet with the Swahilis is a small turtle-dove (*Turtur damarensis*) which presents an interesting instance of commencing divergence. All the Zanzibar specimens of this bird noticed had a black line from the bill to the eye, usually wanting, or not readily perceptible, in Mombasa specimens. The Zanzibar specimens, too, of the frog *Xenopus lævis* are largely yellow on the under side of the thighs—a character which is not apparent in Cape specimens in the Zoological Gardens.

This curious frog is very common in Zanzibar, inhabiting the small and shallow wells whence the natives procure water, and presumably subsisting on insects which fall in. It is usually seen in the water with its head out, but I have seen them just at the edge, on the mud. On land they move along in jerks rather than hop, and this possibly explains the presence of claws in so aquatic a species. I gave a specimen of this frog to a mongoose (*Crossarchus fasciatus*) and the animal mortally wounded and left it. Next day, however, it killed outright and partly ate another specimen, which appeared to be distasteful when bitten, and was soon vomited. This latter incident also happened with a lizard (*Mabuia striata*) which the mongoose had had the day before, but the lizard was, nevertheless, entirely eaten. This mongoose, curiously enough, appeared to be unwilling to attack birds, though it did not seem to find them unpalatable. The lizard above mentioned is extremely common both in Zanzibar and Mombasa, though it appeared to me wilder and more difficult to capture in the latter locality. In Zanzibar its great haunt is the wall of an old burial-ground, just outside the town, in the crevices of which they retire to rest, and can be gently drawn out by the tail, if caution be used to prevent its breaking; this calamity often happens, though the tail appears much less brittle in *Mabuia* than in *Lacerta vivipara*, in which it parts with the slightest struggle. A gecko (*Hemidactylus mabuia*) was also very common in Zanzibar, and I was able, both in that town and in Mombasa, to carry out some more experiments with regard to the palatability of various animals, both with *Mabuia* and *Hemidactylus*, specimens of which I had in captivity. In the back yard of the hotel grew some castor-oil plants, infested by a large number of black-and-yellow, sparsely haired caterpillars, which were greedily devoured by one or two glossy cuckoos. These larvæ ultimately developed into a moth (probably an *Euproctis*) almost exactly resembling *Porthesia auriflua*. These moths were readily eaten by the geckos, though

refused by fowls. I offered the geckos some "fireflies" (a small luminous beetle, not an *Elater*) which, though examined, were not eaten, so far as I saw. At Mombasa I offered my geckos two moths closely resembling the Buff Ermine, and presumably of the same genus. One was seized, and it did not appear to be rejected. A glossy-black *Blaps*-like beetle was not touched either by chamæleon or gecko, nor was the moth *Egybolia vaillantina*, a specimen of which I knocked down and gave to them. This moth is dark metallic blue, marked slightly with yellow, and looks black and very conspicuous in its slow heavy flight. The chamæleons on this occasion were probably not feeding, and I shortly afterwards found the moth dead—possibly injured in its capture. Wood-lice also were not touched by my small lizards, though *Gerrhosaurus major*, of which I had living specimens, readily ate a large *Armadillo*. Yet I once saw one chewed and rejected by one of these large lizards. Both geckos and a *Mabuia* seized and refused a red, black-legged Hemipteron common at Mombasa. The large milliped *Spirobolus*, already mentioned in NATURAL SCIENCE, is common near Mombasa, and Dr. Baxter of the Mission informed me that the odour it emitted when attacked by a mongoose was so pungent as not only to baffle this enemy, but to make his eyes smart as he stood by. A specimen procured at Wasin was offered to my own mongoose, which refused to attack it. As this milliped is dead black, with red legs and antennæ, it would appear to be a good instance of warning coloration. On offering the large dark-brown species, *Spirostreptus gigas*, to a *Galago* which readily ate grasshoppers, however, it was untouched. Possibly, therefore, this also is unpalatable, though not marked with warning colours.

I regret that I have not been able to make any noteworthy observations bearing on sexual selection. I once, however, was witness to what Mr. Boulenger thinks was the courtship of two lacertilians, though I interrupted it, under the impression that it was a fight, by attempting to catch them as they circled round and round with their heads down and tails up, occasionally closing, biting, and rolling over. The species was *Mabuia striata*, which, though handsome above, is quite plain beneath. One is at first very much tempted to put down to sexual coloration the red patches at the back of the thigh which are found in the toad, *Bufo regularis*, in this locality, and about the same region and under the arms in a tree-frog, *Hylambates maculatus* As these tints are concealed when the animal is in its normal squatting position, one naturally thinks of some special reason for their existence. Mr. Boulenger considers that they cannot be sexual, as they exist in both sexes, and as the pairing in the Anura is not calculated for the display of beauty on the part of the animals. The tendency to red marking in widely-separated genera of Anura in certain local areas is also exemplified in the Malay Archipelago[1]; in a collection

[1] *See* Boulenger, P.Z.S., 1890, pp. 31–40.

containing 12 species from Sumatra, Mr. Boulenger found no less than half marked with carmine, while a similar proportion obtained in a collection from Malacca.

Of the Oligochaete worms, the objects of my expedition, I have little to say. The dry sandy character of the East African soil is not suited for these animals (though it must be remembered that I was there in the dry season), and they only occur round pools, or in similar damp situations, as where water from houses has been constantly poured. The earthworms are prevailingly "thin and hungerly" in appearance, and of smallish size. I found numbers of a bluish earthworm in a bog on the mainland not far from Mombasa, in mud so wet that the water ran into the hollows made in digging out the worms. Similarly semi-aquatic is a mole-cricket (*Gryllotalpa africana*) which burrows, often in quite wet mud, without contracting a stain, and swims ashore quickly and composedly when thrown into the water. As our own species is an inhabitant of light and dry soils, this tolerance of wet in the African form is curious, and may help to explain its wide distribution, as it occurs in the Ethiopian, Indian, Australian, and even Palæarctic regions.

In conclusion, I must express my thanks to the British Museum officials for their kindness in assisting me to identify many of the animals herein mentioned, and to the residents in East Africa for their hospitality and assistance during my stay among them.

FRANK FINN.

III.

Note on Sexual Selection.

IN Mr. Cunningham's review of Mr. Romanes' new work, "Darwin, and after Darwin,"[1] the writer gives an independent and thoughtful criticism of my views on that subject, and I should like to be permitted to make a few observations in reply thereto. Mr. Cunningham says:—"Mr. Wallace argues as though the superior male, *facile princeps*, in the competition for a living, could found a line of descendants inheriting his own health and vigour, without female assistance." And again: "A male that excels in the struggle for existence is a complete failure, so far as the species is concerned, unless he can succeed also in finding mates." These passages seem to me to involve suppositions against all probability and all evidence. They imply that the better organised male, in all respects, except in ornament, is rejected by the females in favour of the worse organised in every respect, except in ornament. There are here two improbable assumptions—the first, that the most ornament is usually, or frequently, dissociated from the best general organisation; and the second, that any such less perfectly organised male would be preferred by the female on account of his slightly superior ornament. For the difference, it must be always remembered, *is* slight. Out of a hundred male pheasants or peacocks of the same age, the difference in length of plumes or shade of colour is rarely very conspicuous or even perceptible, except by close comparison; and if whatever difference there is were *not* usually associated with vigour and health, then the two forms of sexual selection—by combat and by display of ornament—would lead to different results; and, as males with ornamental appendages usually *do* fight for the females, the most ornamented would *not*, in their case, be the parents of the next generation.

We are, therefore, forced to conclude that the two qualities—general vigour and ornament—*are* not independent of each other, but are developed *pari passu*, and the problem then becomes, does the female determine her choice by the latter rather than by the former? I quite agree with Mr. Cunningham when he says: "The sexual desire of the female has a hereditary association with certain sensory

[1] NATURAL SCIENCE, vol. i., p. 546.

stimuli, and the means of furnishing these stimuli are constantly reproduced and improved by inheritance in the males." I further admit that the display of ornament by the male is one of the means of exciting this desire; but mainly because it is an indication of sex, of sexual maturity, and of sexual vigour, probably not at all on account of details of colour or pattern.

There is, however, another consideration which Mr. Cunningham appears to have overlooked, and that is, the necessary weakness, comparatively, of female selection, owing to the very limited range of her choice. The law of survival of the fittest has such enormous selecting power because of the overwhelming odds against the less fit. A species which has two or three broods a year, or one large brood, and which lives, say, ten or twenty years, as do many of the vertebrata, produces from 50 to 100 successors of each pair, from which one or two only are selected to take the place of their parents. But in the case of sexual selection, it is a question of probably not more than two or three to one in most species, and in many even less, for there is no evidence and little probability that the number of healthy and competent males that fail to find mates bears any large proportion to those that do find them. Much of the success of particular males must depend on early chance encounters with a mate, while the competition can only be among small groups in each locality. If we add to this the consideration that in almost every case combat, or agility, or bodily vigour must have great influence, the part that remains to be played by ornament alone will be very small, even if it were proved, which it is not, that a slight superiority in ornament alone usually determines the choice of a mate.

This, however, is a matter that admits of experiment, and I would suggest that either some Zoological Society or any person having the means, should try such experiments. A dozen male birds of the same age—domestic fowls, common pheasants, or gold pheasants, for instance—should be chosen, all known to be acceptable to the hen birds. Half of these should have one or two tail plumes cut off, or the neck plumes a little shortened, just enough to produce such a difference as occurs by variation in nature, but not enough to disfigure the bird, and then observe whether the hens take any notice of the deficiency, and whether they uniformly reject the less ornamented males. Such experiments, carefully made and judiciously varied for a few seasons, would give most valuable information on this interesting question. Till this is done, suppositions as to what determines the choice of the female can have but little value.

ALFRED R. WALLACE.

IV.

An Advance in our Knowledge of Seedlings.[1]

THE study of the germination of seeds and the resulting seedlings is not a new one. In 1693, Homberg, in the Memoirs of the Paris Royal Academy of Science, published his "Expériences sur la Germination des Plantes," and from that time to the present day there has been accumulating a large amount of literature in the publications of the various Scientific Societies, and also in the form of treatises dealing with individuals, groups, or some special aspect of the subject. It was not till the beginning of the present century that much attention was given to the early stages of a plant's history; but then several important works appeared. Mirbel's "Nouvelles Recherches sur les Caractères Anatomiques et Physiologiques qui Distinguent les Plantes Monocotylédones des Plantes Dicotylédones," in the *Ann. Mus. d'Hist. Nat.*, vol. xiii. (1809), contains one hundred and forty figures of seedlings; and this was soon followed by his "Considérations sur la Graine et la Germination." About the same time De Petit Thouars, Poiteau, and L. C. Richard were also busy; the "Analyse Botanique des Embryons Endorhizes ou Monocotylédones et particulièrement de celui des Graminées" (*Ann. Mus. d'Hist. Nat.*, t. xvii. (1811)) of Richard is one of the best accounts and gives very good figures of the germination of grasses. They were closely followed by Tittmann, whose study of the embryo and its development into the plant appeared at Dresden in 1817, and who shortly after produced several other kindred papers. We must not stay to review the copious but often exceedingly fragmentary and very widely-scattered literature of the last seventy years; among the most useful are the numerous papers of Irmisch and Winkler in Germany, and Warming of Copenhagen.

In all this literature, however, we find no satisfactory general account of the whole subject. In 1884 Klebs' "Beiträge zur Morphologie u. Biologie der Keimung" appeared in Pfeffer's "Untersuchungen d. Botan. Institut, Tübingen"; but this deals purely with the process of germination and does not consider the resulting seedlings. Tubeuf,

[1] A Contribution to Our Knowledge of Seedlings. By the Right Hon. Sir John Lubbock, Bart., M.P., F.R.S., &c., &c. London: Kegan Paul, Trench, Trübner & Co., 1892. 8vo. Vol. i., pp. viii. and 608; vol. ii., pp. 646. With 684 figures in the text. Price £1 16s.

in his "Samen, Früchte u. Keimlinge" (Berlin, 1891), describes the seed and seedlings of some of the endemic German shrubs and trees; but up to the present date seeds and seedlings have never been studied in the same systematic way as the adult plants. This deficiency Sir John Lubbock seeks to supply in his "Contribution to Our Knowledge of Seedlings."

The two good-sized volumes contain the results of a vast amount of work. Seeds of almost every natural order have been germinated, and the seeds and seedlings, and process of germination, described. In the case of small orders, or those exclusively tropical and not found in cultivation, it has been possible to give only a few illustrations; but the large and important temperate or world-wide families have been very thoroughly studied. The arrangement adopted is that of Bentham and Hooker's "Genera Plantarum"; preceding the technical descriptions of the plants of each family is a general account of the fruit and seed considered as bearing on the form of the embryo, and also of the seedlings, while an attempt is made at a classification of the chief types.

The systematic portion of the work is prefaced by an introduction of nearly 80 pages, in which the author discusses some of the leading points arising from the subject-matter of the twelve hundred pages which follow. The contents of this introductory chapter have already appeared in several papers published in the Journal of the Linnean Society; they are reproduced here in a revised and connected form.

The introduction shows what to look out for in the rest of the work, and the explanations proferred of the points here briefly touched upon will suggest much to those who read the book with any intelligence, and show, moreover, what a mine of wealth lies hidden in the mass of technical descriptions. The author lays stress on the variety in form of the seed-leaves or cotyledons, which though not quite so great as that obtaining in the foliage, is yet surprising; and also on the striking contrast between the cotyledons and not only the final leaves, but even those by which they are immediately followed. "So far, indeed, are the cotyledons from agreeing with the forms of the leaves, that the difficulty is to find any which have been clearly influenced by them." In some cases, however, it is evident that external conditions acting similarly on both have had a similar result, as, for instance, in the Crassulaceæ, where we find the cotyledons observed were all succulent, like the adult leaves and the plants themselves, those of *Crassula quadrifida* being also similarly covered with glands secreting a white substance; in both cases the same protection against loss of moisture has been adopted.

We may contrast these with the drought-loving cactus family, where, as in *Opuntia*, the cotyledons are fleshy, while the leaves have been reduced to spines or bristles (Fig. 1). In *Echinocactus viridescens* (Fig. 2), however, it is interesting to find the cotyledons also reduced,

forming minute, scarcely elevated green teeth at the junction of the fleshy hypocotyl and stem.

In some of the marsh species of *Ranunculus* the cotyledons are narrow and elongated, like the leaves.

Where the seed contains no endosperm but is filled by the embryo, the shape of the cotyledon must often largely depend on the way in which it is packed, and its relation to the leaves subsequently produced will not be evident; but if we compare cases where the embryo is so embedded in endosperm as to be apparently uninfluenced by the shape of the seeds, and where, moreover, the seed-coat splits widely on germination, allowing a free exit, we may sometimes find some rough resemblance in the relation between the seed-leaves and those of the adult.

Thus, in the Cistineæ, where these conditions hold, two leading types of cotyledons are represented by the two leading genera *Cistus* and *Helianthemum*; in the former they are comparatively narrow, in the latter broad, and the ultimate leaves, at any rate in the case of the species considered, will be found to bear a similar relation, those of *Helianthemum* being the broader. This relation may sometimes be observed also, other conditions being similar, where the seeds are exalbuminous. Thus, two species of *Matthiola* are described, in one of which, *M. incana* (Fig. 3), the cotyledons, the primary and ultimate leaves are respectively broader than in the other, *M. bicornis* (Fig. 4).

Such relations are, however, quite exceptional, and the contrast between the seed-leaf and the ultimate form is certainly striking. Sometimes the adult form is assumed abruptly, as in the cactus family above-mentioned, and this may happen even when the leaf is a highly developed compound one, as in some Leguminosæ, *e.g.*, *Æschynomene aspera* (Fig. 5). Very often, however, in species with compound leaves, the two following the cotyledons are simple and opposite, as in *Indigofera australis* (Fig. 6), the scarlet-runner (*Phaseolus vulgaris*), *Pueraria* (Fig. 7), and other Phaseoleæ. Frequently we find a more or less perfect gradation to the adult form, as in *Ceratocephalus* (Fig. 8) or *Ranunculus arvensis* (Fig. 9).

Where the cotyledons are thick and fleshy, serving merely as a store-house of reserve material for the growing seedlings, and remaining in the seeds usually beneath the ground, the first few leaves are reduced to small scales, as in *Sapindus inæqualis* (Fig. 10). Green assimilating leaves are unnecessary while the store in the seed is inexhausted, and the whole energy of the seedling can be more profitably spent in raising it as high as possible into the light and air, and out of the influence of surrounding vegetation.

In a few rare cases the cotyledons by growth after germination do come to resemble the ordinary foliage leaves. The Onagrarieæ supply most of the examples. The cotyledon at first bears no relation to the subsequent form, but a new growth takes place at its base, so that the whole organ finally resembles in form and appearance the

other leaves of the plant, though still bearing the original cotyledon at its apex. The accompanying figures of a species of *Clarkia* will illustrate this (Fig. 11).

The cotyledons, immediately after germination, are oblong-orbicular, slightly auricled at the base and sessile; they then enlarge and become orbicular or broadly ovate, somewhat cuneate at the base, and shortly petiolate, as seen in Fig. A, which represents a specimen three days after germination. The new basal portion grows as represented in B and C, five and ten days after germination respectively, until it ultimately forms the greater part of the leaf from which it is easily distinguished by its irregular ciliate margin, as seen in D. *Streptocarpus* (Fig. 12) (Gesneraceæ) presents a somewhat similar case, but here one of the cotyledons only shows an extraordinary development, and becomes the first foliage leaf.

In the introduction, the author also discusses the shapes of cotyledons, showing how the length, breadth, symmetry, equality, crenation, emargination, lobing or fission, and presence of auricles may often be explained by their having to be packed into a seed of a given shape or successfully to make their exit from the same in germination. It is, of course, open for one to say that the seed is made to fit the embryo, not the embryo the seed, but, as Sir John points out, the shape of the seed is often largely predetermined by that of the fruit or the way in which they are packed; and, moreover, the devices for distribution or protection against injury by beast or climate must often have had considerable influence. This last consideration is well exemplified in two species of *Galium*. The shape of the seeds is the same, but those of *G. aparine* have only a thin coat, as there are no conditions of extraordinary severity to be endured, while the seeds of *G. saccharatum* are provided with a tough corky coat, to prevent drying up in the hot climate of North Africa. The thin coat of *aparine* will tear easily in germination to allow the exit of the cotyledons, which can therefore broaden out in the endosperm, but the thick coat of *saccharatum* is too tough to be thus easily torn, and the cotyledons therefore remain narrow, so that they can be drawn out through a much smaller opening (Figs. 13 and 14).

Narrow cotyledons are frequently a device for easily getting clear of the seed, thus in Santalaceæ "their narrowness seems to be due to a difficulty of exit from the small opening of the woody fruit, for there is plenty of space inside to allow of their attaining a much greater width," and even then they often become torn in the process. Compare also the acicular cotyledons of the pines (Fig. 15) and firs, which seem to shake off the seed with difficulty. Broad, flattened seeds often contain narrow cotyledons from the fact that the embryo lies transversely, often curving round the edge as in *Menispermum* or many Chenopodiaceæ (Fig. 16) and Amarantaceæ. In the case of the sycamore, the strap-shaped cotyledon can be easily rolled up into a ball and packed in the round seed (Fig. 17).

The walls of the seed are sometimes thickened at the chalaza, or point where the vascular bundles enter the seed-coat, and this may cause a notch in the apex of the seed-leaf; similarly, auricles at the base are explained by the effort of the cotyledon to fill up the space round the radicle. A section of the seed of *Cuphea silenoides* illustrates both these phenomena. In the convolvulus and its tropical allies, the ipomœas, a ridge at the chalaza is sometimes so prominent that the cotyledons cannot grow in length, and are consequently divided almost to the base.

Sometimes, however, as in *Berberis aquifolium*, the cotyledons are notched though lying quite free in the endosperm.

Inequality of the two cotyledons may be due to folding in the seed, as in the radish or cabbage (Fig. 18), where they are applied face to face and then doubled longitudinally; the outer one, having more space, is the larger. In the geranium there is a want of symmetry in each cotyledon, due to one-half of each being folded inside one-half of the other, the two inner halves being the smaller (Fig. 19).

The curiously divided cotyledons of the lime are explained by their manner of growth in the seed, where they meet the wall and then curve back, following its general outline; to fit the walls neatly without crumpling they must be cut into lobes (Fig. 20).

These are only a few of the considerations arising from the descriptions and illustrations, which form the bulk of the work.

It may be of interest to notice a few of the many other points of interest revealed in a perusal of the systematic portion.

In the Ranunculaceæ the embryo is minute and embedded in a copious endosperm, and we do not find the great variety in shape which usually obtains where the cotyledons have to be packed in a given space owing to the embryo occupying the whole seed.

The prevalent type of cotyledon is broad and bluntly ovate or oval; *Ranunculus arvensis* quoted above (Fig. 9) is an example; *Ceratocephalus falcatus* (Fig. 8), with its linear cotyledons, is a marked exception. The genus *Anemone* shows the greatest amount of modification; the petioles of the cotyledons are more or less connate in most species at the base, but in *A. coronaria* half-way up, in *A. rupicola* nearly to the top, in *A. polyantha* quite to the top with the blades also somewhat connate at the base; in *A. nemorosa* the petioles are quite suppressed. *Delphinium* affords an instance of an interesting relation between length of hypocotyl and presence or absence of petioles in the cotyledons. Thus in *D. staphysagria* the cotyledons are almost sessile, but the hypocotyl is three centimetres long, in *D. elatum*, on the contrary, the hypocotyl is very short, but the cotyledons have long petioles; in both cases the elevation of the cotyledon is assured.

This relation is also noticed between two species of *Vitis*, and again between *Eucalyptus marginata* and *E. calophylla*.

Clematis may be cited as an instance of the occurrence of aërial

and subterranean cotyledons in the same genus; *Clematis graveolens* has aërial cotyledons like all the other specimens of Ranunculaceæ described, but in *C. recta* (Fig. 21) they are fleshy and do not leave the seed, and as usual in such cases the first leaves are small and soon drop, in the figure they have already fallen. The same occurs in the genus *Phaseolus*, where *P. vulgaris* has its cotyledons borne up on a long hypocotyl, while in *P. multiflorus* they remain below ground in the seed. In *Clematis*, however, the aërial cotyledons are foliaceous, but in *Phaseolus* they are thick and fleshy, wrinkled and turned to one side of the stem, and only a pale green, looking as if they ought never to have left the seed, and the hypogæal were the normal state. In *Erythrina monosperma*, also a member of the tribe Phaseoleæ, the cotyledons are aërial, but thick, fleshy, and pale yellow, while in *E. suberosa* and *E. vespertilio* they are subterranean.

In *Chimonanthus*, belonging to the Calycanthaceæ, a small order closely allied to the Ranunculaceæ, the fleshy cotyledons fill the seed, but are aërial, becoming foliaceous and persisting for more than a year; the two following pairs of leaves are small and perish early.

The cotyledons of *Anona palustris* (Anonaceæ) are aërial and closely resemble the true leaves, but in several species of the genus they never leave the seed, but are torn from the axis during germination.

In spite of the great diversity of the fruits and seeds in the Cruciferæ, the cotyledons may be grouped under a few leading types. They may be broad and entire, as in *Matthiola incana* (Fig. 3), or broad and emarginate, as in the radish, cabbage (Fig. 18), or mustard. The linear form of *Heliophila* is unusual, while the spathulate type represented by *Bunias orientalis* seems to be due to growth after germination, as the cotyledons are narrow and spirally coiled in the seed. *Schizopetalon walkeri* and the common cress (*Lepidium sativum*) are exceptional in having divided cotyledons (Fig. 22). In the former they are bipartite, looking like four narrow cotyledons, and it is suggested that this may be a device for getting rid of the seed, as there seems to be some difficulty, the narrow arms having to struggle out through a small orifice. In the cress, on the other hand, the two lobes of each cotyledon help to fill up the extra large seed.

The cotyledons of *Viola tricolor* (Fig. 23) closely resemble those of *V. palustris* (Fig. 24), but the shape of the leaves is very different in the two cases.

Hibiscus phœniceus (Malvaceæ) has dimorphic seedlings, one form retaining the simpler form of leaf much longer than the other.

In *Linum monogynum* (Lineæ) (Fig. 25), the cotyledons are much longer and broader than the following leaves, an unusual circumstance, but recurring occasionally, as in *Olea cuspidata* (Fig. 26) and *Hakea acicularis* (Proteaceæ).

Under Leguminosæ we may notice the gradual reduction of the leaves to spines in the gorse (*Ulex europæus*) (Fig. 27); and the appear-

ance of the pinnate form in the early leaves where the later are reduced to the petiole (phyllodes) as in the acacia (Fig. 28). The figure of a pod of *Medicago orbicularis* (Fig. 29), containing nine germinating seeds, gives a good idea of the competition among seedlings where a fruit contains several seeds, and yet does not rupture. The same results where one seed contains many embryos, as in *Ardisia japonica* (Fig. 30).

The variety in Leguminosæ contrasts with the great similarity prevailing among the seeds and seedlings of the following order, Rosaceæ.

In the sun-dew (*Drosera*) it is interesting to find that even the first leaves catch insects, though the gradation to the adult form is very gradual.

Under *Viburnum* (Caprifoliaceæ) is described the development of the lateral raphe which almost encompasses the seeds—a very exceptional case in any natural order of Phanerogams. It is found to result from a localisation of growth in the lower end of the young seed. (Fig. 31.) The distance between the chalaza and the hilum and micropyle remains unaltered, while, owing to growth of the lower part of the seed, the raphe seems to creep farther and farther round it.

Psychotria (Rubiaceæ) is remarkable in that the cotyledons are provided with stipules.

The germination is of special interest in Valerianeæ and Dipsaceæ, where the single seed remains in the fruit, which is usually capped by a membranous crown (involucel). In *Scabiosa australis* (Fig. 32) the involucel is short and the base of the hypocotyl has a sort of foot which presses against its rim, pinning it to the earth. In *S. graminifolia* (Fig. 33) the radicle, which here also makes its exit at the upper end of the fruit, pierces the membranous lamina of the involucel, fixing it to the soil by a thickening at the base of the hypocotyl.

The cotyledons of the buckwheat have the two halves unequal, as in the geranium, and for the same reason, as they are curved like an S in the seed. The hop (*Humulus*) affords an interesting comparison; the cotyledons are spirally coiled to fill the nearly globular seed, but the inner has one coil more than the outer, so that after germination the two are equal in length.

A full account is given of the development of the cotyledons of the walnut (*Juglans*), where they are subterranean, and its ally *Pterocarya*, where they are aërial, the corrugated folds of the former corresponding to the spreading lobes of the latter.

The seedling of *Casuarina* (Fig. 34) is curious, resembling that of the Gymnosperm *Ephedra* (Fig. 35) (Gnetaceæ).

Finally, we may notice the good series of figures illustrating the germination of the water-plantain (*Alisma plantago*) (Fig. 36).

We have only briefly alluded to a few of the many interesting points which arise from a perusal of the text, and a comparison of the

figures and descriptions by which each family is illustrated. There is still room for almost boundless speculation, and even when we have duly digested Sir John's classical work, we are fain to admit with Klebs that the diversity in form, and its biological significance, is often still a riddle, though here and there some light has been thrown on its solution.

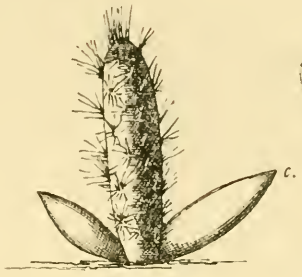
A. B. RENDLE.

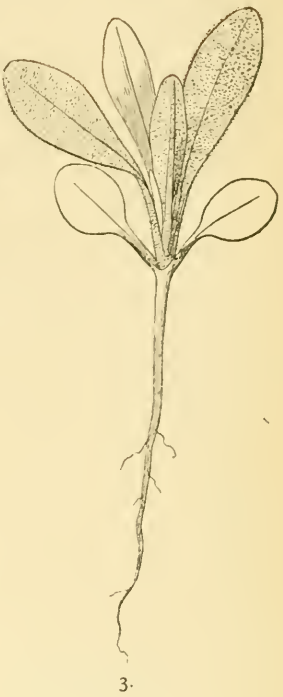
EXPLANATION OF FIGURES.

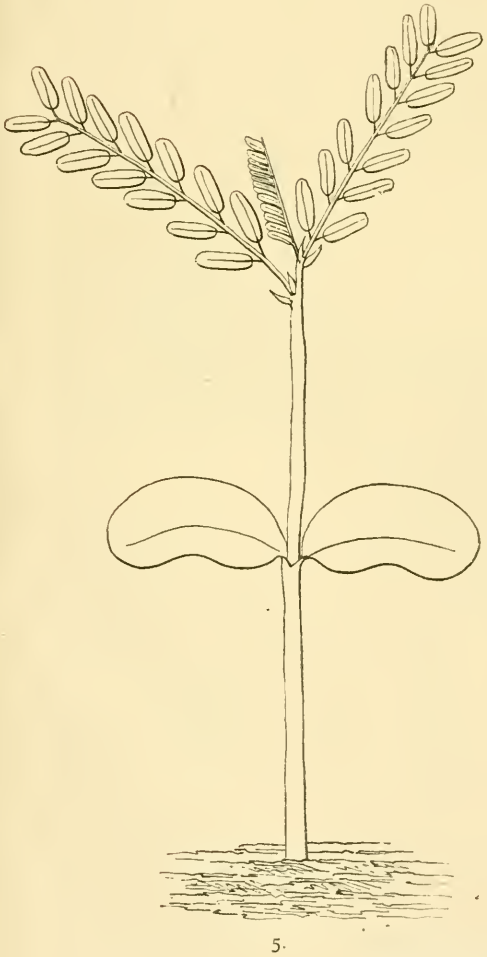
N.B.—*c* indicates a cotyledon.—The numbers in Figs. 8, 9, and 23 indicate the order of development of the leaves.

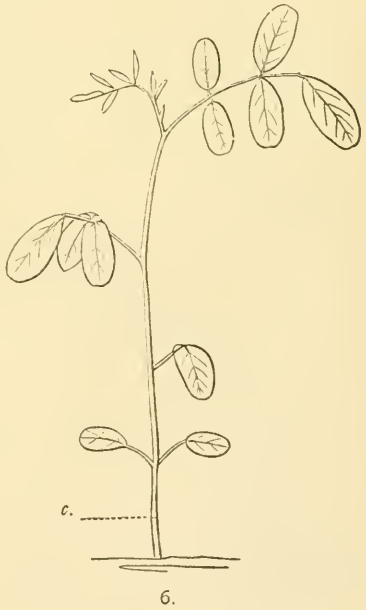
1.—Seedling of *Opuntia occidentalis*. (Nat. size.)
2.— Do. *Echinocactus viridescens*. (Nat. size.) Cot. indicates one of the minute cotyledons.
3.— Do. *Matthiola incana*. (Nat. size.)
4.— Do. *M. bicornis*. (Nat. size.)
5.— Do. *Æschynomene aspera*. (× 2.)
6.— Do. *Indigofera australis*. (Two-thirds nat. size.) C indicates the place from which the cotyledons have dropped.
7.— Do. *Pueraria thunbergiana*. (Nat. size.)
8.— Do. *Ceratocephalus falcatus*. (Nat. size.)
9.— Do. *Ranunculus arvensis*. (Half nat. size.)
10.— Do. *Sapindus inæqualis*. (Half nat. size.) S, seed containing the cotyledons.
11.— Do. *Clarkia sp.* Showing different stages. (× 2.)
12.— Do. *Streptocarpus* (hybrid). A, B, and C, × 5. D, older seedling, Nat. size. F L, first leaf, developed from one of the cotyledons. C, the other cotyledon. S L, second leaf.
13.—*Galium aparine*, germination. (× 4.)
14.—*G. saccharatum*, germination. (× 4.)
15.—*Pinus rigida*, germination. (× 2.)
16.—*Chenopodium bonus-henricus*. (× 8.) A, vertical; B, transverse section of seed; showing cotyledons and radicle.
17.—Sycamore (*Acer pseudo-platanus*). (Nat. size.) Fruit and embryo, showing two modes (A and B) of arrangement of the cotyledons.
18.—Seedlings of *Brassica oleracea*. A (× 4) cotyledons still folded; B (× 3), cotyledons spread.
19.—Section through embryo of *Geranium*, showing mode of folding of cotyledons.
20.—A, longitudinal section of fruit of the lime (*Tilia*) (× 4); Pc, Pericarp; O T, outer; I T, inner coat of seed; H, hilum; Ch, chalaza; P, endosperm; C, cotyledons; A O, aborted ovules. B, seedling. (Nat. size.)
21.—Seedling of *Clematis recta*. (Nat. size.)
22.—Stages in germination of *Schizopetalon walkeri*. (× 2½.)
23.—Seedling of *Viola tricolor*. (Nat. size.)
24.— Do. *V. palustris*. (Nat. size.)
25.— Do. *Linum monogynum*. (Nat. size.)
26.— Do. *Olea cuspidata*. (Two-thirds nat. size.)
27.— Do. *Ulex europæus*. (Nat. size.)
28.— Do. *Acacia verticillata*. (Half nat. size.)
29.—Pod of *Medicago orbicularis*, with seeds germinating. (× 2.)
30.—Germinating seed of *Ardisia japonica*, showing six embryos. (× 2.)
31.—Longitudinal sections of fruit of *Viburnum* in three stages. (× 5.) R, raphe; Ch, chalaza.
32.—Commencement of germination of *Scabiosa australis*. (× 6.)
33.— Do. do. do. *S Gramuntia*. (× 4.)
34.—Seedling of *Casuarina*. (Nat. size.)
35.— Do. *Ephedra vulgaris*. (Two-thirds nat. size.)
36.—*Alisma plantago*, stages in germination. A, B, and C, × 8; D, × 4; E, nat. size; F, nat. size; *C*, cotyledon.

For the illustrations we are indebted to the courtesy of Sir John Lubbock, Bart., and of Messrs. Kegan Paul, Trench, Trübner & Co.









SEEDLINGS (Figs. 1—6).

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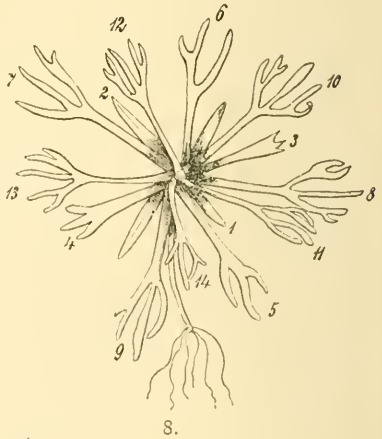

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SEEDLINGS (Figs. 7—10)

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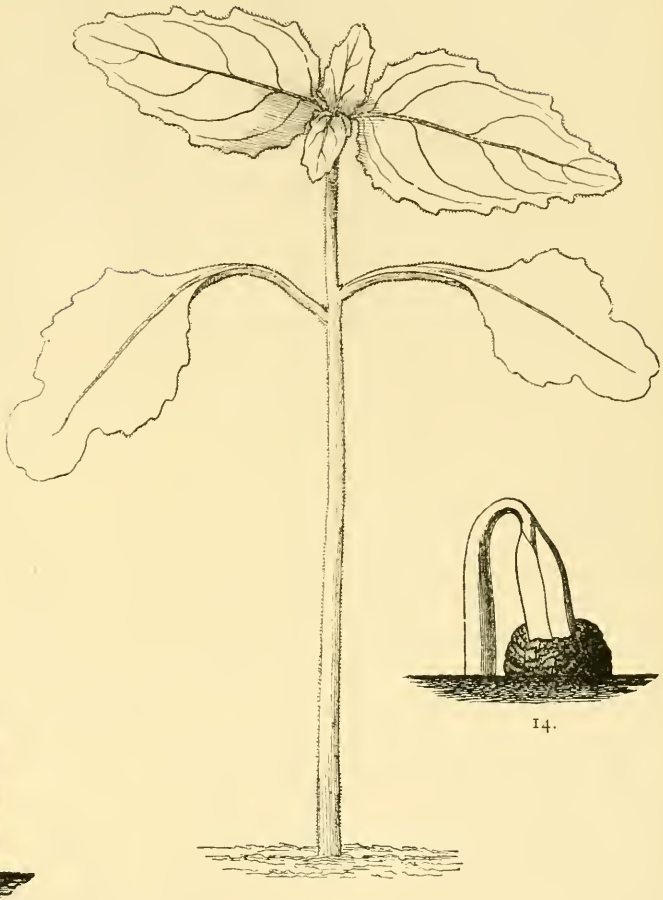

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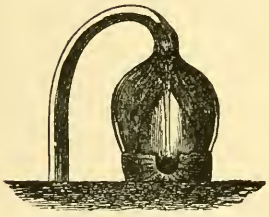

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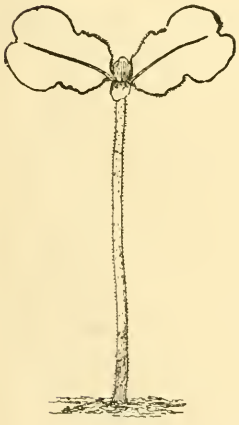

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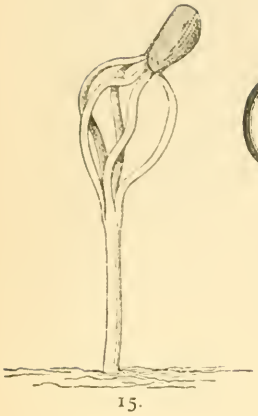
SEEDLINGS (Figs. 11--14).



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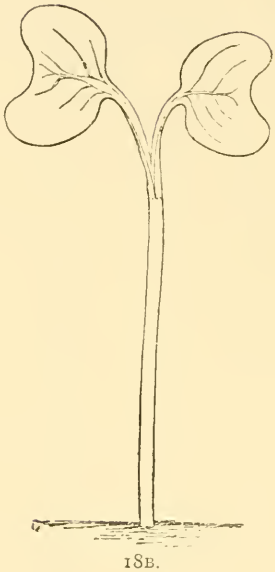

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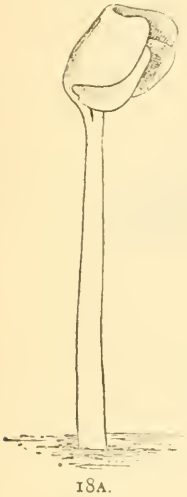

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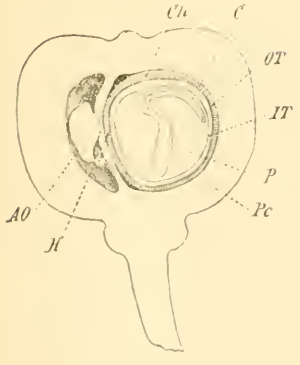

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SEEDLINGS (Figs. 15—21).

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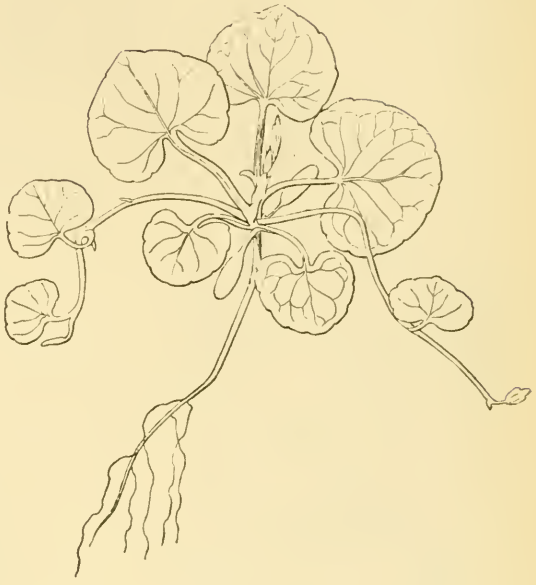

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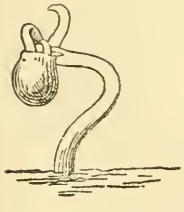

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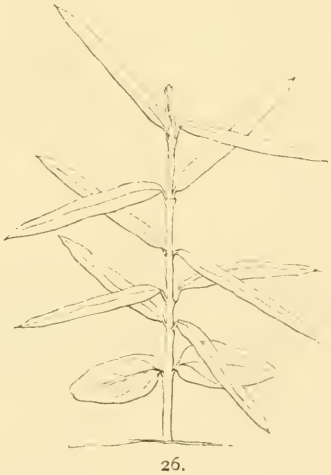

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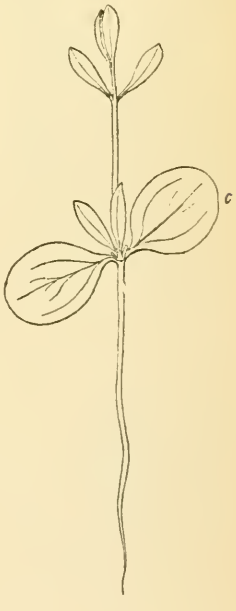

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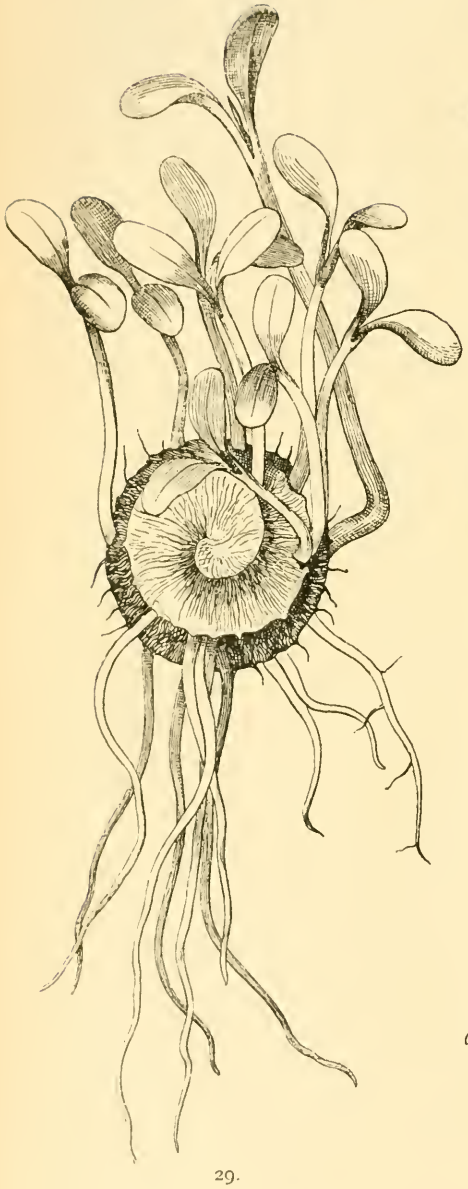
SEEDLINGS (Figs. 22—26).



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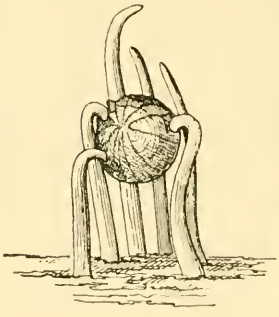

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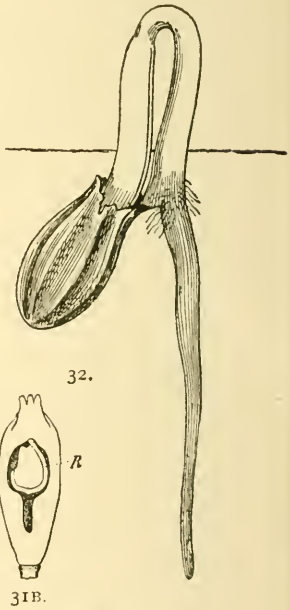

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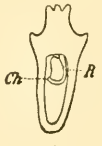

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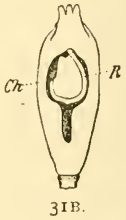

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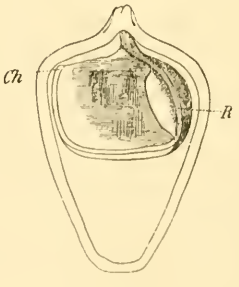

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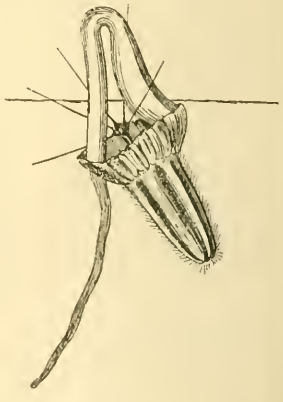
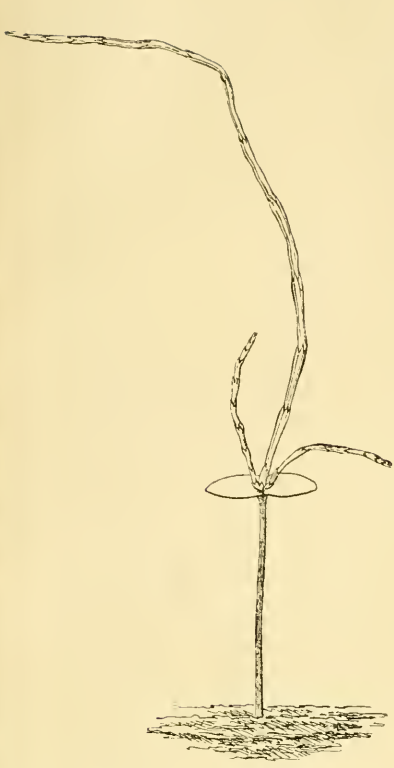


33.

SEEDLINGS (Figs. 27—33).



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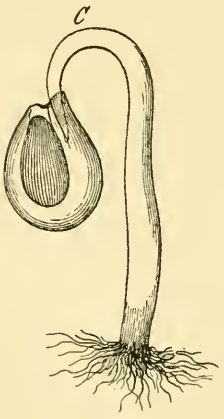
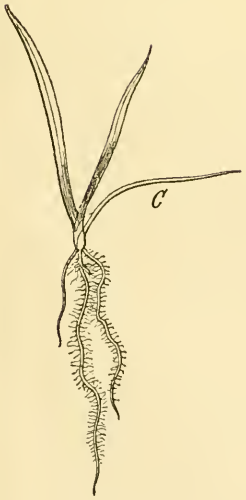


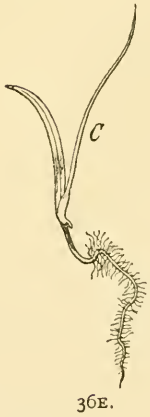
36B.

36D.



36F.

36C.



36E.

36A.

SEEDLINGS (Figs. 34—36).

V.

Exploration in New Guinea.[1]

WHAT may be called the golden age of exploration in New Guinea dates from about the year 1870. Previous to that time various navigators had laid down for us different portions of coast line, beyond which almost nothing was known. During the last two decades scarcely a year has passed without some expedition of greater or less importance visiting its shores. Since the establishment of the Protectorate in 1884, our knowledge has grown with marked rapidity. To the distinguished mariners Kolff, Blackwood, Owen Stanley, and Schleinitz, and to those talented surveyors Moresby, Field and Pullen, we are indebted for the accurate delineation of its coast-lines, and of the depth and trend of its under-water margins. To such naturalists as Macgillivray, Teijsman, Meyer, Wallace, D'Albertis, Beccari, Miklucho Maclay, and Macgregor, we chiefly owe the materials we now possess for the biological history of the country. Prominent among the large contributors to the geographical features of the interior of New Guinea are Van der Crab, Lawes, Chalmers, Maclay, D'Albertis, Pfeil, Cuthbertson and others, of whom the present Administrator, more favoured than his predecessors, in wielding the authority and the resources of the Possession, as well as profiting by the experience of preceding explorers, is pre-eminent.

Of the three territories into which the great island is divided, the British possession, occupying its narrowest part, and being easy of access from both shores, has been far best explored. Of the interior of the Dutch portion we know very little indeed, and of late years almost no exploratory work has been attempted, nor any colonisation, though it will probably yet be found that the Netherlanders possess in the slopes of the Charles Louis Mountains, which are said to ascend by gentler gradients than those of the Owen Stanley range, the most valuable and salubrious tract of the whole island. Of the large region on the south-eastern coast, between the meridians of 135° and 141° of east longitude, we are in almost entire ignorance. It is probable that much of the country for a long distance inland south of the 5th parallel of south latitude will be found quite low and similar to the adjacent region of the Fly River, which lies across the

[1] "British New Guinea" By J. P. Thomson, F.R.S.G.S. Pp. xviii., 336, with map and numerous illustrations. London: George Philip & Son, 1892. Price 21s.

141st meridian, the Anglo-Dutch boundary line. That portion of our Possession lying between its boundary line and the 146th meridian is as large as the whole of the rest of our mainland portion. It is occupied chiefly by the deltas of the rivers draining the ranges running along the confines of Kaiser Wilhelm's land, which have as yet been sighted only from a long distance. D'Albertis described the interminable swampy flats extending for hundreds of miles along both sides of the Fly, the largest of these rivers, and the more recent explorations of Sir William Macgregor have but confirmed the observations of the Italian traveller. This is the chief and almost the only level land on our southern coast, but in being swampy, very malarious, rarely approachable in the dry season because of the surf that hides the reefs, and accessible only by sea during the quieter rainy season, when most of the land is inundated and its rivers are flooded and dangerous, it is of no practicable use now, or likely ever to be till time has elevated it higher above sea level. No minerals have been discovered in this region in payable quantity; nor has recent exploration brought to light any extraordinary additions to its fauna or flora. The palæontological specimens brought back by recent expeditions are few and in poor condition, but they are just sufficient to indicate that in the Papuan Gulf region there occur Tertiary beds of Miocene and Pliocene age, containing mollusca and corals, some of them related to forms found in the Tertiary of Patagonia, and others similar to species now living in the Pacific Ocean. These beds overlie (whether directly or indirectly is unknown) strata of Jurassic or perhaps of Lower Cretaceous age.

In the remaining portion of the Possession, the two highest summits have been ascended within the last few years—Mt. Owen Stanley by the Administrator himself, and Mt. Yule by Mr. Belford. The interest of the splendid achievement of the ascent of Mt. Owen Stanley centres in the account of the fauna, and, perhaps, especially of the flora of its alpine regions, where have been found commingled South American, New Zealand, Australian, Himalayan, and European forms, affording us data which must be of the greatest assistance in the solution of the distribution of life in the Southern Hemisphere. The ascent of Mt. Yule was too rapid, and the party too short-handed, to reward Mr. Belford with any special novelties among plants and animals. This mountain is 10,600 feet in height, and stands isolated from the Owen Stanley range, and from the others of the same cluster, all of them being volcanic. In the plains to the south-west, a large lake, the only one yet known in New Guinea, was discovered, varying from three to eight miles in width, situated between Mt. Yule and the sea, twelve miles distant from the coast. During his official voyages of inspection, the Administrator has visited Rossel, Joannet, Sudest, and St. Aignan, the larger islands of the Louisiade Archipelago, and made excursions of greater or less extent into the interior of several that had never before

been penetrated. With their geological formation and their inhabitants we had in a general way been fairly well acquainted, through the observations of the naturalists of H.M.S. "Rattlesnake," and of many others who have visited the archipelago for various purposes from time to time. The annual reports issued by the Colonial Office —which one would scarcely think of consulting for scientific observations of an exact kind—record additions to our knowledge of the north-east coast and of the D'Entrecasteaux Islands made by Sir William Macgregor. He appears to have touched at a few places not previously visited, but, except in matters of detail, he has contributed little to the excellent descriptions given by Captain Moresby in his account of his surveying voyage along that coast; or to the observations made by the officers with Sir Peter Scratchley on board the "Governor Blackall," in 1884. The Administrator's visits to the Trobriand, Woodlark, and Laughlan islands have, on the other hand, augmented our previous information. The whole of the group appears to have recently been elevated, or to be still rising. Most of the islands are surrounded by raised beaches and cliffs of elevated coral, as is the case also in many parts of the Louisiade Archipelago.

Crossing over into German territory, we find that Herr Hugo Zöller has recently determined that the mountains running southward, parallel to the coast from Astrolabe Bay, are separated by an open space from the Finisterre range, which he ascended to an elevation of 9,000 feet. From this spot he discovered, in the Bismarck range to the west, an enormous snow-capped summit, which he believes to be the highest peak in New Guinea, and to the south still another range—probably an extension of the Rawlinson Mountains—attaining also an altitude of 10,000 to 12,000 feet. The north-westerly summits of the Victor Emmanuel range, which were discovered by D'Albertis from his furthest on the Fly River, have been estimated by Sir W. Macgregor, who sighted them from the upper reaches of the same river, to ascend to 12,000 ft., as disrupted and precipitous peaks. Whether they terminate the range, or whether they run on north-westerly to meet the Charles Louis Mountains is a problem left for some future explorer to solve. The region between these ranges and the line of peaks west of the British boundary is still occupied by a blank in our maps, which widens as it proceeds northward, till it unites with the great unknown expanse of Central New Guinea, belonging partly to the Germans, but chiefly to the Dutch. Though much of the interior is unknown, the whole of the north-east coast has been well surveyed, but the number of rivers that fall into the sea along its length is remarkably few, even opposite the great mountain ranges.

Meteorological observations taken at several places on this north-east coast and in China Straits show that the distribution of rain-fall depends on the position of a place in regard to the prevailing winds. In this region rain comes generally with the north-west monsoon, and in the Southern summer, and the table of rain-fall at

Hatzfeldt Haven shows its monthly maximum (14·88 inches) in the summer. Finsch Haven is, on the other hand, sheltered from the monsoon, but lies open to the trade wind, which reaches its greatest development in the Southern winter, and strikes the east side of Huon Gulf, and of the peninsula which juts out north of it. Thus the monthly maximum of rain falls there in July. The north-east coast, although not so directly swept by the trade wind as the east coast, is not altogether outside its influence. Thus we find here a secondary maximum of rain-fall in July, when the trade wind is at its height.[2]

This short summary has been suggested by the perusal of the latest addition to the list of works on New Guinea, a volume just published by Messrs. Philip and Son, entitled "British New Guinea," by J. P. Thomson, of the Royal Geographical Society of Australasia. "The author having during the past six or seven years (so he tells us in his preface to the English edition) contributed numerous papers on subjects connected with New Guinea to the Geographical Societies of Australasia, England, Scotland, and France," has now essayed to place "before his numerous and distinguished co-workers in the department of Geographical Science an authentic record of our knowledge of Her Majesty's youngest colonial possession—a modest contribution to the British reader." He has been good enough to inform us that he bases his qualification for undertaking a volume on "the physical and broad geographical aspects of New Guinea" and picturing truly its Melanesian inhabitants, on his being in Fiji a surveyor and fellow-officer of the "distinguished Administrator" of the Possession; and he has an "intimate acquaintance with Polynesians and other coloured races." In order also to produce a "book different from the ordinary narrative of travel—something of a representative character—" he was fortunate in obtaining the co-operation of workers eminent in geology, botany, zoology, and the languages of the country.

Adequately impressed by these remarks, we turned, with much expectancy, to the book itself. After reading the very meagre historical sketch which forms the first chapter, we proceeded to the second, headed "Explorations in the Louisiade Archipelago." To our amazement, we discovered that they were not the explorations of the *author* of the "numerous papers on New Guinea," but the explorations of "a beloved administrator," *compiled* by Mr. Thomson. Nowhere in the book could we find that the compiler himself had ever been in New Guinea. The title page would have been less misleading if it had run:—British New Guinea: the Explorations of "His Honour the Administrator of the Possession and others," paraphrased out of the official reports and papers elsewhere by J. P. Thomson. The only original portions of the book

[2] Proc. R.G.S., 1891, p. 176.

are the valuable scientific appendixes prepared by "the distinguished collaborators" of the compiler. We hope that these gentlemen will feel equal satisfaction in the association, and appreciate the title conferred upon them. The pronoun *we* is used throughout the volume in a sense which is not altogether customary. It is employed by Mr. Thomson as well when he is relating the exploits of Sir William Macgregor, in which he can have had no part, as when he is giving expression to his own opinions. For instance, on page 188 we find it stated, "to the knowledge of its (the Fly River) tributaries, *we* have made several important accessions"—a way of putting it that reminds one of the well-known story of the organ-bellows blower who claimed an equal share in the compliments bestowed on the organist's skill.

In a work printed and published in 1892, we are surprised to find no mention of the ascent of Mount Yule, so successfully accomplished on Christmas Day, 1890, by Mr. Belford, who accompanied Sir William Macgregor to the summit, and the writer to the base of Mount Owen Stanley; nor any recognition taken of the elaborate surveys of Commanders Pullen and Field, R.N. In this compilation, the observations made by the officers under Sir Peter Scratchley in the "Governor Blackall" are totally ignored, and places fixed and entered in the map published for the Protectorate Government soon after his death are claimed as new discoveries and re-named. If Mr. Thomson could have condescended to consult the literature of his subject, as authors who desire to be accurate generally do, he would have found in the volumes of the Royal Geographical Society of London information that might have saved him from many mistakes. The observations also made during Mr. Douglas' voyages while Special Commissioner are never referred to, nor are Mr. Milman's, nor Mr. Romilly's. Captain Moresby's thorough survey of the north-east coast is apparently entirely forgotten in our compiler's eagerness to claim renown at all hazards for the hero of his book; for we read on pages 172, 173, "this coast has hitherto been comparatively unknown . . . Recently, however, the pall of obscurity which for ages clothed the coast has been uplifted by the British New Guinea Government, and we [!] are now enabled to describe the geographical conditions with some degree of authority." It is to be regretted also that everywhere throughout the book the name Mt. Victoria is still applied to Mt. Owen Stanley. On the occasion on which Sir William Macgregor's paper was read before the Geographical Society in London, the president pointed out that this peak had borne for nearly forty years the name of the captain of H.M.S. "Rattlesnake," and that it had been distinctly given to the peak and not to the range. We venture to believe that even Mr. Thomson's persistent use of the new designation, in spite of the attention repeatedly called to the scientific illegality of the change which he apparently intentionally ignores, will not be effectual in

legalising this unwarrantable contravention of the laws as to priority of geographical nomenclature.

Although the book would, from its title and preface, lead one to expect a general account of British New Guinea in all its aspects, it is almost entirely devoted to an exposition of Sir William Macgregor's exploits. Yet narrowed down to this, the record of so much work of the highest excellence and importance would have been welcome if it had been compiled with more judgment and less bias; and if in its preparation the official reports had been followed more closely, we should have been spared many inaccuracies, and much bad grammar, as well as a superfluity of inflated language which in a book which its maker hopes may be "welcome to students of Natural Science," is nauseous in the extreme.

The names of the authors who have contributed the scientific appendixes to the volume are sufficient guarantee for their accuracy. Especially valuable are the vocabularies furnished by the Rev. W. G. Lawes, and those collected by the Administrator in so many different districts of the Possession. We miss with regret any report on the mammalia, on the birds, and on the land and fresh-water mollusca, though they are, we believe, to be found in the Parliamentary papers. The distribution of these groups is peculiarly interesting, and we should have been grateful for a complete *résumé* of the fauna collected here from the publications through which the notices are scattered, and especially from the official papers, where they are practically buried. Throughout the volume there are several excellent illustrations, and there is a good map. The publishers are to be warmly congratulated on the binding, paper, and printing of the book.

HENRY O. FORBES.

VI.

The Migration of Birds.

THE subject of the Migration of Birds is one which has attracted much attention in times both ancient and modern. It is only, however, during recent years that any light has been thrown on the probable origin of these periodical movements by the collection of new facts and observations from every part of the world, so that now the general principles and laws which govern the movement are capable of being better understood, and the chosen paths of the bird through the air remain no longer shrouded altogether in mysterious gloom.

In the new volume, mentioned below, Mr. Dixon has brought together from various sources, doubtless with much painstaking research, a great array of facts bearing on this special subject, and these he has endeavoured, how far successfully we must judge, to reduce to some system or law.

The book is divided into twelve chapters, with special headings, and the first of these deals with ancient and modern views of migration and the presumptive habit of Hibernation; in the terrestrial form of which, as opposed to the subaqueous, Mr. Dixon seems to retain some sort of belief, and as a suggestive fact, which he thinks deserves notice, tells us that the winter quarters of the two most northerly ranging of the Hirundinidæ, namely, the house and the sand martin, are yet practically unknown. Is it possible, he asks—without venturing to express any decided opinion—that the most northerly birds hibernate and pass the long boreal winter in torpidity? Mr. Dixon does not seem to be aware that both these species, which have a most extended range, cross the Mediterranean, in spring and autumn, on migration in great number, and the range of the latter is known to go as far south as the Transvaal.

The subject of the hibernation of some birds may, however, be dismissed as unworthy of serious consideration.

Passing to the subject of Migration, the author thinks that birds change their countries from necessity, and that instinct or hereditary desire gives the impulse to move, but then gives place to reason, memory and knowledge of locality. He is evidently no believer in the

[1] The Migration of Birds. An attempt to reduce Avian Season-flight to Law. By Charles Dixon. London: Chapman and Hall, 1892.

inherited instinctive sense of direction possessed by birds in general, and the power of passing from one extreme of their range to the other without the assistance of land marks, or, in the case of the young, direct guidance or personal conduct of such old birds who have made the journey before. A little examination, however, will show that his hypothesis cannot be maintained. It is a well-known fact that the young of various species of wader breeding in high latitudes flock together when they have acquired their flight feathers, and, perfectly independent of their parents, migrate to southern lands, and appear late in July or early in August on the East Coast of England at a time when the supply of food is still most abundant in the arctic lands they have left—lands also in the full glory of the latter summer when as yet the sun has hardly dipped below the horizon.

Birds go north in the spring to reproduce their species. When this is accomplished, the young are cast off and have to shift for themselves, and in all matters relating to food and movement are dependent on their own inherited instincts to find their way to the ancestral winter quarter—old and young, with the same common object, can and do act independently. The old also are not always able to follow the old trail southward, but frequently enough, under unfavourable meteorological conditions, are compelled to make considerable deviations from the normal route; hence it is that the phenomena of migration vary so much from year to year, both as observed on the East Coast of England, and for half a century at Heligoland. The act of migration in the young bird is not a habit improved and acquired by observation or instruction, or in any sense to be compared to the training of a homing pigeon, a production of artificial selection and most careful education to a special purpose, and, while possessing no doubt, with all birds, an inherited and instinctive sense of direction more or less latent, has never been subjected to the struggle for existence and the same necessities as a wild bird.

The occurrence of young and old of various species together in the great autumn "rushes" is no indication of personal guidance, and the young of the year are quite as capable of reaching their winter quarters as the old, and for anything we can tell the impulse, instinct, or whatever we may choose to call it, is more strongly developed in the young and vigorous. The great majority of birds migrate during the night, and this alone will preclude any guidance of the young by the old, or knowledge gained by experience of leading land-marks along the route.

Among the waders the earliest arrivals on the East Coast of England are often a few old birds more or less in faded summer plumage. That these cannot have in any manner acted as guides, or voluntarily undertaken the task of personally conducting the young, is obvious from the time of their arrival not conforming within days and weeks with that of the birds of the year.

Young birds migrate in the autumn unattended by the old, and without any previous knowledge of the route. The force, stimulus, or impelling power is what we call instinct, which may be defined as a special adaptation to a special circumstance, a non-intelligent action, that is instinctive, and, as it has been recently expressed,[2] performed in virtue of the innate and co-ordinated mechanisms of the organism, an innate capacity developed by Natural Selection. Instance the selection of a butterfly of the leaf upon which she lays her eggs, the only leaf, it may be, which serves her grubs, and a food of the kind which she herself does not eat ; the instinct of a young pointer, brought up away from the parent, drawing on game and standing the first time it is taken into the field ; the instinct of the young bird which, independent of all guidance and unimproved by intelligence, can find its path unerringly to the winter home of its parents across thousands of miles of land and water.

Regarding the origin of these ancient and complicated movements, granting that birds have ascended from reptilian ancestors, they would begin to emigrate from place to place as soon as the exigencies of life and their powers of aerial locomotion became developed. There is, however, no necessity, when considering the origin of migration and those ever-varying phenomena which now attend these movements, to go back to the great unthinkable ages—the cooling down period of the earth's existence, when the Arctic and Antarctic zones became first capable of supporting life ; the key and explanation of the mystery may be found in a comparatively recent geological period, commencing with the slow recession of the great ice-cap from Europe and North America in Post-pliocene times.

There is nothing in the present position of bird life in the globe which can be accounted for by the present known geographical distribution of species ; the explanation must be sought in the unreckoned ages of the past.

Mr. Dixon dwells much on what he calls inter-polar migration, and endeavours to prove the probability of some ancestral forms of the Charadriidæ inhabiting, during remote ages, a south polar area. He considers that long journeys now taken by some species of waders to south of the Equator—as the knot, curlew sandpiper, sanderling, and grey plover—and reaching over 140 degrees of latitude, or nearly 10,000 miles direct, are the result of the transfer of these species from the North Polar Basin to the South Polar Basin during favourable intervals of climate. Unfortunately for Mr. Dixon's theory, there is absolutely no proof in the present distribution of bird life that the antarctic regions were ever the breeding quarters of the progenitors of the vast bird hosts which now repair in the breeding season to the immense solitudes surrounding the North Pole. All scientific evidence points in the opposite direction, and to the conclusion that life has

[2] "The Study of Animal Life," J. A. Thomson, chap. x., Instinct.

not been distributed from the South Pole. If, in some remote period, life existed there—and there is no reason to suppose the contrary—it has long since been extinguished and crushed out by the superincumbent ice-cap, from which no retreat was left open. A comparatively narrow strait or channel of ancient sea is known to be sufficient to separate and keep apart very distinct faunas, and to have proved an all-sufficient barrier to the passage and inter-breeding of birds; and, so far as our knowledge goes, from the very earliest period of this earth's history the great Antarctic continent has been isolated by the oldest and deepest oceans—a belt 700 miles across in the narrowest part—reposing on a foundation of primitive rocks, and without any sedimentary deposits suggestive of ancient land-lines.[3]

The only probable representatives now surviving of an Antarctic avifauna are the Penguins and, perhaps, some of those Petrels which are so numerous in the Southern ocean.

It is to the North Pole then that we look for the origin of present life on the globe, both animal and vegetable, and it is in this polar origin of life that we must in the future expect to find the key to the riddle of migration in Post-pliocene ages. In the great North Polar continent of the Eocene period, with its tropical warmth and abundance of space and food, and the facilities of unbroken land routes extending to the boundaries of the Antarctic Circle, and the subsequent vast climatic changes of the Pliocene age, which were slowly and gradually evolved, we find the primary factors in bringing about the present distribution of bird life on the globe, and the segregation and differentiation of species. The subject, however, is much too vast to enter upon in a short review.[4] The autumn migration, then, of birds to south of the equator cannot be explained by heredity or any inherited love of home, at a time when the Antarctic world was the favourite ancestral breeding ground, and the Northern Hemisphere a winter home.

Mr. Dixon gives in a tabulated form, under the headings of "Incipient," "Short," "Moderate," "Long," "Extended," illustrations of the longitudinal range of various species; and as an example of "incipient" migration, with a normal range of 1,000 miles downwards, cites the Grey Phalarope (*Phalaropus fulicarius*), a species circumpolar in its breeding quarters, and migrating southward for winter to Scinde, Northern Africa, and Central America. Again, the knot is quoted as an instance of "extended" migration, covering from 7,000 to 10,000 miles. The fact is, however, that the winter range of those

[3] "The Polar Origin of Life considered in its bearing on the Distribution and Migration of Birds." By H. B. Tristram. *The Ibis*. Part I., 1887, p. 236; part II., 1888, p. 148.

[4] Those interested in the subject of the North Polar distribution of life should read Mr. Seebohm's "Evolution of Birds," "Differentiation of Species," "The Glacial Epoch," "Migration," being chaps. II. to V. of "The Geographical Distribution of the Charadriidæ."—H. Sotheran & Co., London, 1888.

knots which pass along the West Coast of Europe extends from Southern Scandinavia, along the British and Continental coast-lines, to the south of Africa, wintering wherever the conditions are favourable along that line for the support of life. Several instances could be adduced to show that an abnormal supply of food in a special locality has the effect of staying altogether or retarding the passage and range of species which, under normal conditions, would have been more extended. The extent of the longitudinal migration of a bird is the affair of the individual, not of the whole race, moving from one extreme of the range to the other. Therefore, it appears to be impossible to draw, as Mr. Dixon attempts to do, deductions as to short or long migratory flights correlated with ancient or more recent geological epochs.

There are certain general laws of migration accepted by ornithologists; these may be briefly stated. (1) Each bird (excepting purely tropical species) breeds in the coolest, that is the most northerly, part of its range, and that these nesting quarters are reached by horizontal migration. Some species and individuals, however, obtain the necessary conditions for reproduction in a vertical migration by ascending mountains till they meet with the same climatic conditions which exist in high latitudes. (2) Northward migration is for reproduction, southward for food and climate. (3) Birds which go furthest north to breed often retire the furthest south. (4) Each species has its regular periods of migration. (5) No bird is known to breed a second time during absence from its northern home.

The lines of migration, or bird routes, in the east and west Palæarctic and Nearctic regions are infinite and differ in the case of each species and even with individuals. The general direction is from north to south, but there are many variations apparently running in very erratic directions; the bird lines also vary from time to time under the influence of the weather. There are also certain routes or great highways where the migratory host focusses its millions into streams. Such is the line across Heligoland and along the west coast of Europe. There are also many facts suggestive of birds continuing to follow persistently ancient land-lines, now submerged; and we know of no species or individuals which systematically cross seas or oceans where land has not probably existed in recent or more remote geological ages, and since the habit of migration has been evolved.

There is evidence of ancient lines of migration still followed by a few species between India and South Africa, also by Eastern Asia to New Zealand, and from the western coast of North America to the Isles of the Pacific, routes which, probably at some remote period, offered greater facilities over a more continuous land surface. Evidences of more recent land routes are suggested across the North Sea to the British Islands, and over the Mediterranean by Sicily and

Malta. Where a line of migration has been used from remote ages, birds continue to follow the same by hereditary instinct, be it sea or land.

Within comparatively modern times some species of Western Palæarctic birds are known to have extended their summer range eastward, and likewise Eastern Palæarctic birds have spread westward; yet notwithstanding the increased geographical area occupied —notwithstanding the much greater distances to be traversed—they still continue to follow the old lines of migration which they have been accustomed to from the remotest periods.

Islands situated far from migratory routes, as a rule, become isolated areas, and include in their special avifauna the greatest number of endemic species. The reverse is the case when they are situated within the range and influence of long-established routes. As one instance of a sedentary and island species which does not migrate, Mr. Dixon has inaptly chosen the so-called St. Kilda wren. This most unfortunate little bird was recently (1884) exalted into a distinct species under the name of *Troglodytes hirtensis*, but without sufficient cause; and one result is that it has already been exterminated by greedy collectors, who are able to offer the poor islander considerable reward for its capture.

Mr. Dixon says "it is a rule almost without exception that the wings of migratory birds are long and pointed"; also "that migrants decline to make any very extended flights across the sea, except the most fleet-winged and robust." "Small terrestrial species of comparatively feeble flight choose, invariably, the easiest way across, where choice is possible." The fact is, however, that many birds, with wings suggestive of weak flight, or such as are not in the habit of taking extended journeys on land, do succeed annually in the autumn in crossing the North Sea in its widest part in a single flight. Such are the jay, grey shrike, common wren, hedge-sparrow, redbreast, great and blue titmice, water-rail and others. On the other hand, some purely sedentary species possess long and pointed wings.

Having lived near the East Coast of England nearly all our life, we have in each autumn had unusual opportunities, for days in succession, of watching immigration in actual operation, and to note that, under the conditions of favourable weather, birds appear to be absolutely unaffected by the sea passage; and even after a bad crossing—storms or adverse winds or driving snow—a few hours' rest seems sufficient to enable them to renew the journey.

We cannot understand what Mr. Dixon intends when he speaks of "migratory land-birds, many of them frail in form and constitutionally weak and feeble." Such, had they existed, would have been surely eliminated in the life-struggle, and only the most vigorous and robust left to migrate. The occurrence of the turnstone in Polynesia and New Zealand, the sanderling in the Hawaiian Islands and Galapagos Archipelago, the Siberian bar-tailed godait (*Limosa uropygialis*)

in New Zealand, the New Hebrides, and Norfolk Island; the knot in New Zealand; the Asiatic golden plover in New Zealand, and the isles of the Pacific; and the short-eared owl, and others, in the Sandwich, Ladrone, and Caroline groups, are all suggestive of immense ocean flights undertaken by birds on migration.

At the same time, we confess we are very sceptical as to the fact of American land-birds crossing the Atlantic without a more or less assisted passage. Possible exceptions are the snowy owl and Greenland falcon, which are known to have occurred occasionally on the west coasts of Scotland and Ireland, arriving most likely by a route direct from Greenland by the Färoes, and, it may be, assisted by drifting ice. American birds which turn up in the British islands probably travel first north along the East Coast of Greenland, and across the Pole, then south along the Scandinavian coast-line; in support of which it may be urged that the greater portion of stragglers which reach our shores from the New World are recorded from the East and South coasts as far west as Devon and Cornwall. Many of these latter have no doubt first struck the East Coast, and followed the coast-line to the south-west limit of land.

When on the subject of altitude, Mr. Dixon remarks "that in no part of the world do any regular migration routes cross seas too wide to be bridged by the eye of a bird flying at a sufficient altitude." Does Mr. Dixon seriously mean that a migrant over the North Sea, between the Elbe and Humber, can bridge the entire distance of four hundred miles with its eyes from shore to shore, and this by night as well as by day, and in all conditions of weather? What is intended by "sufficient altitude"? and is it really the case that birds in the sea-passage do move as a rule at vast altitudes? The height of migratory flight appears to be dependent on wind and weather: under favourable barometric conditions—chiefly anti-cyclonic—birds will fly high,[5] but in cyclonic periods, the prevailing weather in the autumn, very low; and when over the sea just clearing the water or following the curve of the wave. Often when on the North Sea in the autumn have we seen for hours in succession flights of birds moving westward low near the surface; and in the autumnal "great rushes" over and past the East Coast lightships, the observers, acting on behalf of the Migration Committee of the British Association, have yearly entered in their schedules the records of a low-flying and continuous migration, often lasting for days and weeks. All experience, then, independent of mere theory, is in the direction of a low or medium flight, showing that very lofty flight is the exception and not the rule in the autumnal passage of the sea. The inrush of migrants coming under our own observation on the East Coast of Yorkshire in the last autumn was in many respects very instructive,

[5] With gentle northerly and easterly winds birds are noted as often flying high. Strong winds, however, from any quarter, are known to beat them down to the surface of the sea.

showing the capabilities of small land-birds to make a successful passage of the sea under the most trying and adverse conditions :—

On October 13, wind northerly, moderate, very fine, little or no migration ; one woodcock, one golden-crested wren, and a few redwings, the latter increasing in the afternoon. Early morning of 14, soon after midnight, wind veered to east, blowing more than half a gale, with heavy rain continuous to morning of 16th. During this time very heavy migration, low flying when actually seen. Enormous numbers of gold-crests came in, also nearly equal numbers of redbreasts, these two species, separately, probably outnumbering all the others put together ; these included rough-legged buzzards, about 20 grey shrikes, long and short-eared owls, rooks, starlings, larks, great numbers of blackbirds, ring-ousels, thrushes and red-wings, stonechats, willow-wrens (very many), hedge-sparrows, woodcock (first-flight), common and jack snipes ; also a few grey crows and field-fare on 16th. The circumstances under which the sea passage was made, on the three days, must have been very trying to the birds ; a continuous heavy rain, with a strong easterly wind, and so thick that a large steamboat, anchored a mile from the shore, was seldom visible ; nights also pitchy dark. In fact the meteorological conditions were such as to preclude any special advantages of guidance or sight operating to the advantage of the migratory hosts pouring helter-skelter on to the land.

" The young birds," Mr. Dixon tells us, " are the greatest blunderers, the birds, which have practically no knowledge whatever of the road, and have to depend entirely on the guidance of older birds. That this is the case is abundantly proved by the fact that nearly all the birds that accidentally wander to the British Islands from more or less remote countries are birds of the year " (pp. 178–9).

Although sufficient British statistics are yet wanting, we are inclined to think that among our rarest visitors on migration—birds which have lost their route or been driven over by stress of weather, adults turn up nearly as frequently as the young of the year. We have now before us a list, kindly supplied by Mr. Gätke, of Heligoland, showing for the last fifty years all the rare captures actually made on the island. Of 133 exceptional occurrences, belonging to 20 species whose home is Greece, Asia Minor, Palestine, Southern Russia, Turkestan, &c., with few exceptions rare summer visitors to the island, and occurring in May to the end of July, 122 were adult, and 11 young. Of purely Eastern species, ranging as far as Kamtschatka, buntings and thrushes, occurring during September to the end of November, 34 are old and 13 young. Of American visitors, five have been old and two young. Of *Otocorys alpestris*, now a common migrant in the autumn, two-thirds are old. Of *Anthus richardii* and *Phylloscopus superciliosus* about one-half are old.

Mr. Dixon is inclined to underrate the work done in Heligoland by the veteran ornithologist Herr Gätke. It must be borne in mind

that the value to students of migration of this outpost cannot be reckoned merely by comparative lists of rare wanderers which have turned up on migration there and in the British Islands, but rather by the fact that the red sandstone rock—28 miles from the nearest coast of Denmark—is directly in the line of the principal bird highway between the north and south of Europe; while in no other place in the world have continuous notes been taken for fifty years, in connection with bird passage, and these by one of the oldest and most painstaking workers in Europe, one to whom the author of this book is indebted for some of his most interesting facts.

We think Mr. Dixon greatly overestimates the "Perils of Migration" to birds. These he divides under the heads of "Fatigue," "Natural Enemies," "Blunders and Fatalities on the Way." The death-rate from the three causes he puts as high as 500 to 600 for every thousand, or more than one-half, and of these he thinks by far the greater number succumb by sea. During forty years of observation on the East Coast of England we recollect only two instances in which we have found birds washed up in any numbers, and these were rooks, starlings, and redwings. It is true that occasionally, in fogs, snowstorms, and thick weather, as many as 1,000 to 1,200 birds are recorded as killed against the lanterns of lighthouses or lightships; these "big bird nights" are, however, the exception and not the rule, and the loss is small when compared with the multitudes on actual passage. The number of migratory birds which come within the influence of sea-lights is as a drop in the ocean in the great tide sweeping southward over Europe, Asia, and North America, and scarcely worthy of serious consideration. Probably falcons, hawks, owls, and greycrows are no greater causes of destruction to small migrants on passage than at any other period of their existence.

When on the subject of lighthouses, Mr. Dixon remarks that "The brilliant lamps of the lighthouses are too often the only visible places for which to steer—an inherent knowledge teaches them that light leads to safety." If the author's supposition be correct, then it must have been a bad time for the birds before lights were thought of. Artificial lights are of no use in directing or pointing out the route to be followed, as these beacons must be all much alike to passing flocks.

As an illustration of the rapidity of flight of some species during the spring migration, Mr. Dixon cites the Dotterel (*Eudromias morinellus*), estimating its progress from point to point—that is from North Africa to the Arctic Circle in a single flight between sunset and sunrise—at 200 miles an hour.[6] We doubt very much if normal flight on migration exceeds 50 miles an hour, and very frequently not

[6] When Mr. Dixon says this bird is scarcely ever seen in intermediate places in the spring migration, he appears entirely to have overlooked the fact that it is by no means an uncommon visitor in certain localities on the East Coast of England, frequently lingering some time before resuming its journey.

so much as this. That birds can and do fly with great rapidity for short distances we cannot doubt, but in a continuous journey of many hundreds of miles, the pace probably is not in excess of that of the carrier pigeon. In the migration report for 1885 the flight of woodcocks between the coast of Denmark and the West of England is calculated from fairly reliable data as probably not exceeding 52 miles an hour.

Space will not permit us any further to notice Mr. Dixon's book, although there are other points in which the facts do not appear to warrant his deductions, and we cannot consider this attempt to unravel the very complex web of migration successful. Although our knowledge of the subject has been greatly increased, many more facts and observations are wanted from all parts of the world before we can hope to arrive at any satisfactory and strictly scientific conclusions as to the periodical movements of

> " . . . . wild birds that change
> Their season in the night, and wail their way
> From cloud to cloud " . . . .

In recent years there has been a tendency to much wild speculation, and much written which will not stand the test of critical examination; and we are entirely in accord with our author when he says that " with the gradual growth of ornithology as a science the wildest opinions have been expressed, and the most absurd theories put forward, concerning " migration."

In conclusion, we may state that Mr. W. Eagle Clarke, of the Science and Art Museum, Edinburgh, is at present engaged in the laborious task of reducing the mass of information, collected during nine years by a committee of the British Association, from the reports annually published, and from an examination *de novo* of the schedules sent in from the lighthouses and lightships, so as to show on strictly scientific lines, in a condensed form, and by as clear a method as possible, the results of the enquiry. When this is completed it is hoped some fresh light will be thrown on the phenomena of migration as observed in the British Islands.

SOME NEW BOOKS.

THE BEAUTIES OF NATURE: and the Wonders of the World we Live in. By the Right Hon. Sir John Lubbock, Bart., M.P., F.R.S. 8vo. Illustrations. London: Macmillan & Co., 1892. Price 6s.

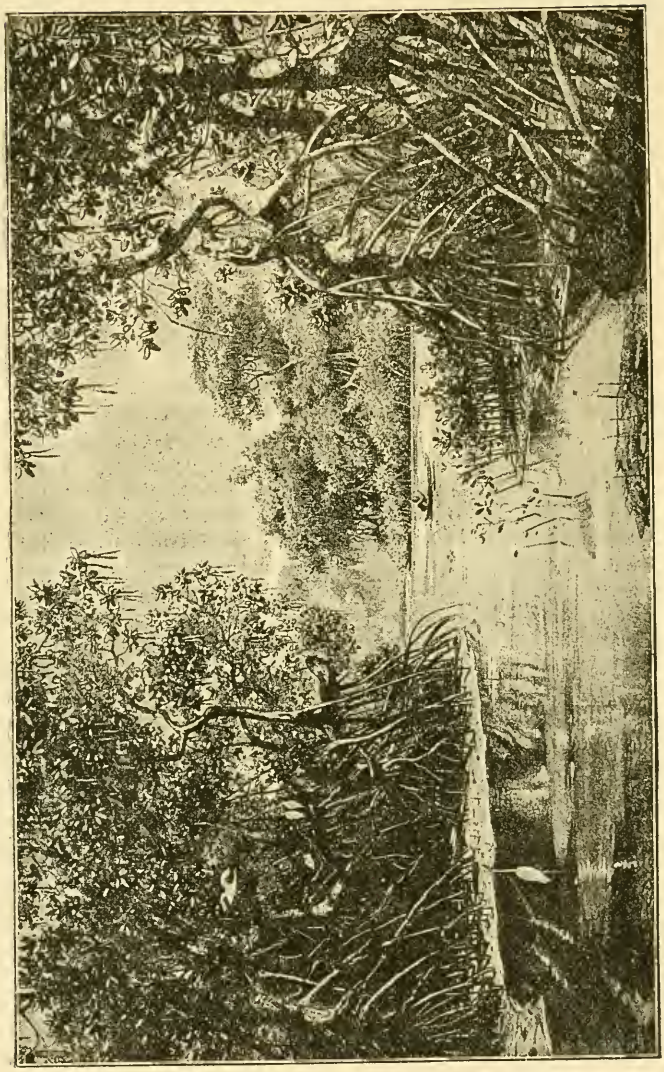
IN this new volume, Sir John Lubbock has produced a series of chapters, such as the tired naturalist will be delighted to read, though he will not find in them much that is absolutely new. The ordinary intelligent reader, without special scientific training, will also take up the book with pleasure, for the author possesses the rare gift of explaining difficult scientific problems in perfectly simple language. Sir John Lubbock takes for the texts of his different chapters various common natural objects, and in passages which will be appreciated as much for the charm of the language as for the ideas expressed, he points to the beauty and unfailing interest in Nature everywhere.

In ten chapters the author speaks of animal life, plant life, woods and fields, mountains, water, rivers and lakes, the sea, and the starry heavens; each chapter dealing, of course, with a few only of the problems. For instance, among the various headings in the first four chapters we find Sir John's special subjects: Communities of Animals, Senses of Animals, Structure of Flowers, Insects and Flowers, Fruits and Seeds, Seedlings, Ants and Plants, &c. Then the chapters deal with wider themes, and under "Woods and Fields" there are descriptions of Tropical Forests, of Meadows, and of Downs. Chapter VI. speaks of Mountains, their origin, scenery, and inhabitants, and of Volcanoes. Chapters VII. and VIII. treat of Rivers and Lakes, and their origin, thus leading to Chapter IX., the Sea.

The book is capitally illustrated, with the exception of one or two of the plates, for which the process adopted is not suitable. The plates we do not like are those representing "Aquatic Vegetation, Brazil," and the "Summit of Mont Blanc." The process used does not succeed with the fine lines of the feathery vegetation, or with the masses of white snow.

THE GREAT WORLD'S FARM; some account of Nature's Crops and how they are Grown. By Selina Gaze. 8vo. Pp. x. and 365, with 16 plates. London: Seeley & Co.

THIS is the best popular scientific book we have seen for a long time. The authoress has first thoroughly mastered her subject, often giving evidence of extensive knowledge of minute detail and wide research into the literature; and then in two and twenty chapters, almost every one of which is a complete little story in itself, she tells the tale in language which though simple is not silly, and is accurate though popular. The plates, of which we reproduce an example by the courtesy of the publishers, are often taken from exhibits at the British Museum of Natural History, or from photographs of living plants at Kew; and there are also wood-cuts scattered through in the text.



A MANGROVE SWAMP.—*(From Gaze's "Great World's Farm.")*

The first chapter is introductory, and shows how man, with all his machinery, is, after all, greatly dependent on natural forces for the production of his crops. Chapter II. brings forward the "pioneer labourers," the gases of the atmosphere, moisture, frost, alternation of heat and cold, wind and rain, glacier and volcano, the great natural forces which split or wear away the fundamental rock. Then come the humbler "soil-makers," the tiny lichen, which, living on the bare rock, helps to break up the surface and by its decay to form a thin film of soil in which mosses can root, to be ultimately followed by dwarf shrubs. But the soil to be really productive must be mixed; any one rock will not supply the necessary ingredients in fitting proportions. This leads to the consideration of the soil-carriers, the river and the wind, and we are made to feel the truth of the old proverb that "a peck of March dust is worth a king's ransom." The use of the bind-weed and its relatives, the salt-worts, the mangrove, and other soil-binders is duly explained; and then follow two most interesting chapters on the "field-labourers," the burrowing mammals, earth-worms, burying beetles, termites, ants, and ant-eaters.

However rich and well prepared the soil, the plant cannot get on without water, and Chapter VIII. deals with its absorption, retention —especially by desert plants—and transpiration. This leads to "deserts," those regions where the soil may or may not be poor, but where water is scarce, and which are hence comparatively barren. The relation between want of water and scarcity of vegetation, especially of trees, is brought out, and stress is laid on the frequency of drought as a result of ruthless demolition of forests. Roots and food from the soil, leaves and their work, followed by a short chapter on climate, blossom and seed, and the development of the second from the first, the working of Nature's golden rule for flowers—"cross fertilisation" wherever possible, guests welcome and unwelcome, and the scattering and carrying of seeds, are subjects of successive chapters. The "Chances of Life" gives a graphic account of the keen struggle for existence, and some of its causes, in the vegetable kingdom. Under "Friends and Foes" the relation between animals and plants is aptly described, the foe of the individual being often the friend of the race. "Nature's Militia" is just now more appropriate than ever when the plague of field-mice in the South of Scotland has been evoking so widespread an interest. The last chapter—"Man's Work on the Farm"—includes a brief historical summary of the changes in vegetation which man has, intentionally or otherwise, effected by his migrations and taste for cultivation.

The work as a whole is so good and so admirably put together that one quite regrets to find a slip such as is evidently the statement on p. 162, to the effect that one part in twenty-five thousand is the proportion of carbonic acid gas present in the air; it should be one in two thousand five hundred, or ·04 per cent. It is not correct to say that the leaf of *Dionæa* secretes an acid which digests the captured insects, for though the acid may be a necessary accompaniment, the peptic ferment is the actual digestive agent. In the chapter on "Chances of Life," the authoress, while rightly denouncing the popular error of mummy-wheat's germinating, unfortunately adduces, as well-authenticated, the statement that seedlings were raised by Dr. Lindley in Chiswick Gardens from raspberry seeds found in Celtic tumuli, perhaps some two thousand years old, and mentions also as a fact the germination of seeds from Roman tombs. These "facts," however, have never borne the light of investigation, and

it is much to be regretted that the authoress has gone astray on a subject which has been so thoroughly threshed out.

MODERN SCIENCE IN BIBLE LANDS. By Sir J. William Dawson, C.M.G., F.R.S. Popular Edition revised. 8vo. London: Hodder & Stoughton, 1892. Price 6s.

THE wide title of Sir W. Dawson's book obviously allows the inclusion of a multitude of interesting topics on geology, volcanoes, and early flint implements, before the topography of the Holy Land with the resources and prospects of Bible lands is reached. The author, to do him justice, avails himself of this to the full, and thus succeeds in putting together a volume of much value both to students of the Bible and the Earth. "It is my desire," he writes, "to lay before the reader a clear statement of the local facts which have furnished to me the solution of doubtful and disputed questions, and to throw on the Hebrew and Apostolic literature such light as may be afforded by the structural features and geological history of the countries of the Bible." The book, in short, may be regarded as a modern Bridgewater Treatise, and, it may be added, it possesses all those elements of interest which characterised the original treatises. Sir W. Dawson's bias against Darwinism is well known, and it is only fair to state that he here delivers many damaging blows to that theory. Thus the evidence against Pliocene Man is strongly marshalled. "I have for many years," he adds, "maintained the recency of man on geological grounds, more especially on the evidence of the absence of any change in organic beings, or any considerable physical changes since his introduction, and of the rate of cutting of river valleys." He points to the recession of the Falls of Niagara (which is fully illustrated here) as affording conclusive testimony of this. Man belongs, therefore, to the modern period alone. He has only existed on the earth for 6,000 or 7,000 years. We give this conclusion *valeat quantum*. It must be left for students to trace its steps for themselves.

The character of this book being so wide and covering so much ground, it is possible to discuss numerous subsidiary topics, as it might seem at the first blush, but which form in reality parts of the author's argument. Thus the primitive Chaldæan documents recently discovered are pressed into the service of defending the Bible. Professor Sayce is quoted to show the extreme antiquity of the art of writing. The critical school has often assumed that it was unknown in Palestine before the age of David; whereas, "long before the Exodus Canaan had its libraries and its scribes, its schools and literary men." The characters were in the Babylonian language, not Phœnician but cuneiform, as the tablets of Tel Amarna testify. The flint implements of Egypt, the Nile mud, the pyramids, and a multitude of ancient objects and geological changes are carefully treated. Sir W. Dawson's pages deserve to be seriously weighed, and they are written in so pleasant a style that they attract the reader instead of repelling him by marshalling a number of bare facts. The author's derivation should be borne in mind, however, during this study: "when facts fail to sustain certain theories, we are usually in the habit of saying 'so much the worse for the theories,' not 'so much the worse for the facts,' or at least we claim the right to hold our judgment in suspense till some confirmatory facts are forthcoming." His arguments derived from the border-land where geology and mythology seem to meet are somewhat untrustworthy speculations. They enable the student, however, to understand over what a wide region such enquiries as those which are here pursued must necessarily lead him.

With regard to what may be termed the second part of the book, nothing but praise can be awarded. The physical structure of Egypt in connection with the residence of the patriarchs and of Israel in that country is most carefully drawn out and illustrated by maps and figures. Palestine is treated in the same painstaking fashion. The topography of the Exodus, as portrayed by the author's own as well as by the most modern researches of other travellers, is fully detailed. Appendixes and an index enable the reader to find what he wants at once. Whatever he may think of Sir W. Dawson's geological conclusions and his views on palæocosmic man, he will entertain but one opinion on the industry, the acuteness, and, above all, the conscientiousness of Sir W. Dawson. This is only what his previous publications would lead all to expect.

CURIOSITÉS DE L'HISTOIRE NATURELLE. By Henry de Varigny. Small 8vo. Pp. 413. Paris: n.d. [1892]. Price 3fr. 50c.

THIS work may be described as a kind of scientific "Half-hours with the best Authors." The book mainly consists of a series of excerpts from different writers, and these not always, it may be observed, very well-known writers. With the names of Darwin, Wallace, Paul Bert, de Saussure, and M. de Varigny himself, there are a number of others which it would be invidious to mention, but whose dicta do not exactly carry weight.

The author explains in his preface that his book is intended for young readers, and that it is furthermore intended to amuse as well as to instruct; it might, therefore, have been wise to correct several fables which are included, such as the old story about the *Hydræ* turning itself inside out and living just as happily in the reversed condition.[1] On the whole, however, we think this a decidedly interesting little book. We do not pretend to have read it through; it is not a book to read studiously, but to pick up for an odd half-hour, which odd half-hour will be by no means unpleasantly spent.

F. E. B.

EXPERIMENTAL EVOLUTION. By Henry de Varigny, D.Sc. London: Macmillan and Co., 1892, *Nature Series*. Price 5s.

THIS volume contains the lectures given by Dr. de Varigny, in 1891, at Professor Patrick Geddes' fascinating Edinburgh Summer School of Science and Art. If in his English the author needed much of the assistance from Professor Geddes and Mr. J. A. Thomson he so modestly acknowledges, he could have been advised by none better, and the book is pleasantly and clearly written.

The first lecture gives a general account of evolution, with an avowed list to French contribution. At the end of the lecture the case for experimental evolution is stated. In the second and third lectures variation in nature, and in the fourth, variation under domestication, are considered as the groups of fact upon which experimental evolution must be based. In the last lecture, the author explains that change of environment, use and disuse, natural, sexual and physiological selection must be the methods of enquiry; that the enquiry will be difficult and prolonged, and will raise many side issues of interest.

This is all well-meaning, and the amiable author may be justified of his book if he stimulates readers to take an interest in natural

[1] *See* NATURAL SCIENCE, vol. i., p. 86.

history beyond the collection of specimens; and for this, his abundant collection of facts and experiments dealing with variation and the influence of changed environment will no doubt suffice. But, alas! if these readers set up aquaria, and in the name of scientific investigation diligently tease their inhabitants; for experimental work, to be of the slightest value, must be done in a much more critical and disciplined way than they are likely to learn from Dr. de Varigny. At least that is so if his observations of animals and plants at all resemble his observations of the authors he cites. Take one or two examples.

On page 107, after an account of Yung's experiments on the determination of sex in Tadpoles, he proceeds:—"Thus food, and the nature of food, has much to do in the determination of sex. The same is the case with bees, where the production of queens, workers, and drones is in great part a matter of nutrition. A worker larva may be reared into a queen, if royal food is provided." Quite so; but if Dr. de Varigny had read with any attention even the short account given by Geddes and Thomson (to whose book he refers on the same page) he would have seen that, while royal food may ripen the latent sex of a worker, a different factor has to do with the distinction between male and female.

On pages 223 and 224 the author gives an account of the modifications produced by cultivation on *Bacillus anthracis*, and then proceeds: "It seems, then, that Professor Weismann goes certainly too far when he asserts that we have no proof of the direct production of transmissible changes by means of external influences. It may be said that he restricts his denial to the metazoa." It may indeed! In this restriction and its consequences is the very gist of Dr. Weismann's difficulty about the transmission of acquired characters.

On page 225 the author writes:—"On the other hand, we possess, in the facts of domestication and cultivation, a large number of cases of variation—which occurs in every part—due to environment, and transmitted by inheritance in various degrees." If the statement were true there would be no controversy.

On pp. 229 and 230 he quotes with approval from Le Conte (*Monist*, April, 1891) his curiously inept classification of the factors of evolution. Of the five heads, the first two are most interesting:—

"First.—Presence of a changing *environment* affecting functions, and functions affecting structure, and the changed structure and function inherited and integrated through successive generations indefinitely.

"Secondly.—*Use and Disuse* of organs re-acting on growth force, and producing change in form, structure, and relative size of parts, and such change inherited and integrated through successive generations."

To include together an admitted truth and a disputed interpretation, so that denial of both may be attributed to those who differ from one about the latter, is an old device of political controversy; but it is unsuited to scientific writers, whose purpose is to reach the truth rather than to confound their adversaries. P. C. M.

ELEMENTARY BIOLOGY. By H. S. Campbell, M.D. London: Swan Sonnenschein and Co., 1892. Price 6s.

THIS excellently illustrated little book includes all the types selected by the conjoint Board of the Royal College of Physicians and Surgeons for the examination in Elementary Biology. It also goes over the ground prescribed in the syllabus for that examination.

The chapters on animal classification are the weakest. Metazoa are divided into Vertebrata and Invertebrata. Of the latter, it is stated that the nerve cord or cords are never dorsal. Dr. Campbell must know very well that there are dorsal cords in both nematodes and *Balanoglossus*. The further subdivisions and reasons given for them are not at all good. Sponges do certainly *not* "form the connecting link between the unicellular animals on the one hand and multicellular animals on the other." The classification of Vermes is quite inadequate.

Of course classification of the Animal Kingdom in a few pages is impossible, and its details to anyone who intends to get no further than elementary work are useless. But Dr. Campbell should have taken more care with this part or omitted it altogether.

Otherwise, the book is well treated, and seems quite suitable for its purpose. P. C. M.

AN INTRODUCTION TO THE STUDY OF BOTANY, with a special chapter on some Australian Natural Orders. By Arthur Dendy, D.Sc., and A. H. S. Lucas, M.A. Pp. xv. and 271. Illustrated. Melbourne: Melville, Mullen and Slade, 1892.

THERE is certainly no dearth of text-books on Botany, yet each new teacher of the Science is fain to admit, like the authors in the present case, that "hitherto no single elementary book has been sufficient to fulfil all requirements," and forthwith to compile a new one which may be above or below the average. The requirements are, perhaps, sufficient justification for the appearance of the work before us, in which Messrs. Dendy and Lucas combine an account of the general structure and life-history of plants, with a special study of Australian forms.

The book is divided into two parts. Part I. (General Botany) is a progressive study of the series of plant-forms, commonly known as types, which are nowadays considered to illustrate best the various groups and the advance in complexity of structure and function from the lowest or simplest to the highest. The stonewort, *Chara*, has been omittted, we think wisely, for it is certainly "a very aberrant form," and therefore not one for an elementary text-book, though not necessarily "of little value in comparative morphology." With the description of structure is blended an explanation of function, and except for occasional looseness the physiology is well and carefully expressed. A few points, however, call for remark. The cells of a plant should not be compared with the bricks of a house without explaining the vital difference between the two, especially when in the same page we read that they are generally "united together in groups" to form tissue, which recalls the building of a house by the bringing together of originally separate elements.

In Chapter II., under *Protococcus*, starch, cellulose, and sugar are mentioned as allied substances having a similar composition and generally known as *carbohydrates*; but the few words necessary to explain wherein lies the similarity are wanting. *Protococcus* is also described as a "typical" vegetable cell; one hardly knows what to understand by "typical," but a cell consisting very largely of protoplasm is certainly not the commonest form, which must rather be sought in such an one as Vines adopts in his "Physiology" to illustrate cell principles, namely, the ordinary tissue-cell with a delicate "primordial utricle" lining the wall. In spite of a few blemishes like

these, the first part, which is mainly Dr. Dendy's work, is a clear and well-written account of the principal forms and their relations to each other.

In the second part, Mr. Lucas deals with the morphology of flowering plants, and gives a description of a few of the more important Australian Natural Orders. The five chapters in which the root, stem, leaf, flower, and fruit are respectively considered, form a capital little introduction to structural and systematic botany; the matter is well connected, while nothing important has been overlooked. In referring to twining plants, however, it is not correct to say that the stem swings round its support "much in the same way as a weight attached to a string held in the hand and swung round"; this view of the vital process of twining is too mechanical for modern notions of physiology. A good classification of inflorescences and fruits is given; and the last chapter contains short descriptions of twenty-three Natural Orders, grouped according to the plan adopted by Baron von Mueller in his census of Australian plants. No apetalous division is recognised, and Euphorbiaceæ, Casuarineæ, and Salsolaceæ are found among the hypogynous polypetalæ; but recent research has shown that the Casuarineæ, at any rate, have no place in this group. An appendix gives useful hints on collecting, observing, preserving, and describing flowering plants.

The book is illustrated with thirty-one plates, the figures in which are sometimes original, sometimes borrowed from previous authors, such as Sachs, Le Maout and Decaisne, and others. They are, on the whole, clear and good, though occasionally sketchy. The text is well produced, on good paper, and remarkably free from misprints.

CASTOROLOGIA: or the History and Traditions of the Canadian Beaver. By H. T. Martin. 8vo. Pp. xi. and 238. London: E. Stanford. Montreal: Drysdale and Co., 1892. Price 10s. 6d.

WHAT Mr. Hornaday has done for the American Bison, and Mr. Barrow for the House Sparrow in America, Mr. Martin has accomplished for the Canadian Beaver in the present exhaustive memoir. The value of such animal biographies can scarcely be overrated to the working zoologist, and this value is much enhanced in the case of creatures which, like the Canadian Beaver, are only too likely to share the fate which has already overtaken the American Bison.

That the Canadian Beaver is, indeed, a doomed animal, seems only too apparent from the work before us, Mr. Martin stating in one passage that its "days have been lengthened to the present, only by contributions levied upon the musquash and the coypu, whose numbers have been heavily taxed"; while later on he writes as follows:—"As to the ultimate destruction of the beaver, no possible question can arise, and the evidences of approaching extermination can be seen only too plainly in the miles of territory exhibiting the decayed stump, the broken dam, and deserted lodge. The passing bear or wolverine tears open the lodge, partly in the vain hope of finding a meal, partly from habit; the rising waters float the logs away, while the drifting ice in fall and spring gradually destroys the dam, till within a decade, where once the busy colony spent their happy domestic lives, no sign remains of all their wondrous toil."

The author treats his subject in a most thorough manner, commencing with the mythology and legends relating to the beaver, next passing on to the considerations of the larger fossil forms more or less

nearly related to this animal, and then devoting a chapter to its living European congener. The fourth chapter treats of some of the more important of the other larger American Rodents, while the fifth and sixth describe the life-history and geographical distribution of the animal forming the subject of the memoir. These are followed by others treating of the engineering feats accomplished by this interesting Rodent, and of the importance of the animal in commerce and manufacture. In the concluding chapters we have interesting accounts of beaver-hunting, of experiments in the domestication of the animal, of its anatomy and osteology, and, lastly, of the beaver in heraldry. Finally, three appendixes reproduce some documents and accounts relating to the animal as known to the earlier settlers.

Whether, therefore, the reader is interested in the beaver from a purely zoological, from a distributional, from an historical, or from a commercial point of view, he will find plenty of matter in this work worthy of his best attention; and the book will, indeed, prove absolutely invaluable to all who may have to write upon any one of these divisions of the subject. The illustrations with which the work is embellished are for the most part excellent, and, as life-like scenes, we may especially call attention to the picture of a beaver-haunt which forms the frontispiece, and to the plate of the "beaver-canal" facing p. 72; while as an example of the illustration of a morphological structure, the figure of the tail of the beaver on page 179 is beyond all praise.

From a literary point of view, Mr. Martin strikes us as being at his best when describing the habits of the beaver in the fifth chapter. In his preface the author takes the precaution to inform his readers that he is not a practised zoologist; and, this being so, we venture to think he would have been better advised if he had taken counsel from someone who is, in regard to the strictly zoological portions of the work. We find, for instance, on page 35, the following sentence:—"The River-rat, or Coypu as it is called by the natives, is in many ways the intermediate species between the musquash and the beaver, and having been known as the 'Castors of La Plata,' might appropriately be named the South American beaver." Now, with all due deference to the author, we think that a more *inappropriate* term could scarcely have been proposed; and when it is remembered that the beaver, the musquash, and the coypu severally belong not only to three perfectly distinct families, but likewise to as many different and apparently divergent sections of the Rodent order, we quite fail to perceive how the latter can in any sense be regarded as intermediate between the other two. Then, again, we must take exception to the statement on page 36, that the affinities of the capybara, which the author terms the "water-hog," "are evidently there with the pachyderms,"—whatever he may mean by the latter obsolete term.

The author's observations on the habits of the beaver will be found full of interest; and many erroneous ideas as to the engineering powers of the animal will be, we may hope, thereby finally dispelled. Attention is directed to the prevalent notion that the beaver always fells his timber in the direction of the water; the true explanation of this being that trees on a river's bank always naturally incline in that direction. The theory that the beaver's "lodge" is merely a special development of the burrow of the musquash is new to us, but is doubtless the true explanation. There are many other observations of equal interest which we must leave the reader to discover for himself in a work which may lay claim to a high position among animal biographies.

R. L.

Marine Shells of South Africa. By G. B. Sowerby, F.L.S., F.Z.S. Pp. 89, pls. v. London: Published by the Author, 1892. Price 12s. nett.

This work is, as its title denotes, a list of the Mollusca inhabiting South Africa; it contains, however, diagnoses of a few new species, and is illustrated by numerous plates which place the book on a somewhat higher level than that of the simple catalogue. In looking over a work of this kind one develops a feeling of gratitude to those hard-working persons who are good enough to do for us this necessary but decidedly not entertaining labour; for, to our mind, there could be no more depressing pursuit in the whole field of natural history than the description of recent shells, excepting, perhaps, the description of fossil shells. The least exhilarating work of this kind that we know of is a vast series of tomes entirely devoted to the almost endless description and arrangement of the species of a single genus of bivalves, the genus *Unio*. The devotion of a lifetime to the study of antimony, in which something new could be discovered every day, is nothing to this. It is, nevertheless, a pity that those who spend their days in the examination of shells do not also take into consideration the contents of the shells. They do sometimes, it is true, speak depreciatingly of the "animal" in giving a description of a new species; but among the students of no group is there a more distinct separation between the anatomist and the systematist than in the case of the Mollusca.

Mr. Sowerby enumerates 740 species from South African seas, and estimates that more than 300 of these are confined to the region of which he treats. We fear, however, that the list cannot be complete, for we fail to find the new univalves, described by the Rev. Boog Watson in his "Challenger" report, and if that great work be overlooked it is not unlikely that some smaller contributions may also have escaped notice.

The work is beautifully printed, with useful illustrations, and deserves a place in every reference-library of Conchology.

Mountaineering. By C. T. Dent and others. Pp. xx., 439. With 13 plates and 94 other illustrations. London: Longmans, Green & Co., 1892. Price 10s. 6d.

The "Badminton Library of Sports and Pastimes," edited by the Duke of Beaufort and Mr. A. E. T. Watson, already comprises 14 volumes, some of which, on Hunting, Fishing, and Shooting, deal with subjects of interest to the Field Naturalist.

The volume before us on Mountaineering, however, appeals more directly to students of Natural Science. It is pleasantly written, and admirably illustrated with pictures that are both humorous and instructive, while the information will be valuable alike to those who ascend mountains as a pastime, and to those who pursue Science at the same time.

The short introduction is written by Mr. Justice Wills, one of the pioneers of modern mountaineering, who made the ascent of the Wetterhorn, from Grindelwald, in 1854. He remarks that "few sports, perhaps few pursuits, afford keener or more lasting enjoyment, or contribute more to the acquisition of self-reliance, patience, and self-restraint"; at the same time, he and other contributors to this volume adopt as their key-note, words of warning rather than of encouragement, and "teach the immorality, as well as the folly, of running into useless danger and risking valuable lives."

The first chapter is on the *Early History of Mountaineering*, by Sir Frederick Pollock; and it is interesting to learn that some of the philosophers and naturalists of the sixteenth and seventeenth centuries were foremost among the mountaineers of their days. Conrad Gesner, of Zürich, in 1541, "resolved for the future, so long as God grants me life, to ascend divers mountains every year, or at least one, in the season when vegetation is at its height, partly for botanical observation, partly for the worthy exercise of the body and recreation of the mind." Sir Frederick says that modern mountaineers ought to revere Gesner as a true ancestor, for he possessed the genuine mountaineering spirit.

Interesting accounts are given of the explorations of other pioneers, of Albert von Haller, Jean Jacques Rousseau, and De Saussure; and there is a reproduction of the old print representing De Saussure, his son, and guides, on the march in 1788. References are also made to the partial ascent of Chimborazo, in 1802, by von Humboldt.

The first recorded ascent of any permanent snow-peak was in 1739; the first successful attempt to reach the summit of Mont Blanc was made in 1786; and the final conquest of the highest peak of Monte Rosa took place in 1855. The author points out that, among English leaders of Alpine exploration in its most active period, a notable proportion were scholars and men of science.

The subjects of *Equipment and Outfit* (for women and men), of *Mountaineering and Health*, *the Principles of Mountaineering*, *Reconnoitring*, *Snow Craft* and *Rock Climbing* form the subjects of six chapters by Mr. C. T. Dent. It is shown that in rock climbing, as in snowcraft, scientific knowledge can always be turned to practical account; that an acquaintance with the chief formations of mountains, and their individual peculiarities, is as useful as a knowledge of the physical phenomena of glaciers and snowfields. Illustrations are given of the features produced by ordinary limestone, dolomite (magnesian limestone), and granite rocks; and of the relations between the dip of the rocks and the configuration of the slopes. The aid of Professor Bonney is acknowledged in reference to these matters.

Attention is drawn to the dangers of climbing steep grass-covered slopes. Thus "the inexperienced person who goes out for a ramble by himself will often attempt rock passages actually more difficult than any he would have to overcome if he ascended half-a-dozen of the reputedly most formidable rock peaks in the Alps"; and botanists, entomologists, and others, "professedly of sound mind," may start forth from their hotel, "shod in boots destitute of nails, and equipped with an umbrella or a walking-stick." The consequence is, they "find themselves in tolerably awkward predicaments," for grass, after dry and hot weather, is extremely slippery, and at a height of six or seven thousand feet the soil clings but loosely to the rocks.

A short chapter on *Maps and Guide-books* is contributed by Mr. W. M. Conway; a chapter on *Mountaineering beyond the Alps* is by Mr Douglas W. Freshfield; and *Climbing without Guides* is dealt with by Mr. C. Pilkington.

We pass on to *Hill Climbing in the British Isles*, also by Mr. Pilkington; a chapter of especial interest to home mountaineers. Our hills are always accessible, and most of them can be climbed at any period of the year, but there are many ascents that may be dangerous, and some that are impassable in the winter months. The author remarks that it is never well to ascend an out-of-the-way hill alone.

Sligachan, in Skye, is spoken of as "the rock-climbing centre *par excellence* of the British Isles," and the author remarks, "always take

a rope." The so-called "Inaccessible Pinnacle" of Sgurr Dearg is the Matterhorn of the district, and to climb it a man must have a steady head. The east ridge "is very narrow, with a sheer drop on each side; great care should be taken in the descent, one man only moving at a time, and let the last man be steady and keep the rope tight." Useful advice is given on the dangers of climbing chalk-cliffs. Speaking generally of our hills and cliffs, the author, however, remarks "that they form a good and safe training-ground where men may learn and practise nearly all that is necessary in the art of mountaineering."

Another chapter deals with the *Recollections of a Mountaineer*, by Mr. C. E. Mathews, who gives reminiscences of J. D. Forbes and John Ball (with portraits), and of others who have developed the craft of mountaineering, not omitting the great Alpine guides to whom mountaineers so largely owe whatever success they have achieved.

Mr. H. G. Willink contributes a short chapter on *Sketching for Climbers*, and Mr. Dent writes the concluding chapters on *Camping* and *Photography*. A Glossary and a good Index complete this very interesting volume.

New Works of Reference.

Part 10 of Taschenberg's *Bibliotheca Zoologica II.*, dealing with works 1861–1880, has lately been issued. It extends from Gastropoda to part of the section Pisces. The author visited London last month to complete his references to the publications of the British Field Clubs.

The volume of the *Zoological Record* for 1891 has appeared at an unusually early date (October, 1892). The editor, Dr. David Sharp, hopes in future to be able to issue the work in August or September of each year.

The Geological Survey of India has published an index to the genera and species described in the "Palæontologia Indica," and an index to the "Memoirs" of the Survey from 1859 to 1883. Both publications have been prepared by Mr. W. Theobald.

A third edition of A. de Lapparent's "Traité de Géologie" is being issued by F. Savy, Paris. The first volume of 567 pages, dealing with what Lyell termed the "Principles of Geology," has already appeared (though dated 1893). The second volume, treating of Geology proper, will consist of five fascicules, of which the first has also been received (though similarly dated 1893).

Messrs. Williams and Norgate have issued another useful "Natural Science Catalogue" (no. ix., 1893).

OBITUARY.

FREDERICK SCHWATKA.

Born September 29, 1849. Died October, 1892.

WE regret to chronicle the death of this able explorer. Schwatka was born at Galena, Illinois. After graduating at West Point in 1871, he was admitted to the Nebraska Bar, and about the same time took a medical degree in New York. He was a lieutenant of cavalry till 1877, and in 1878 was appointed as commander of the expedition to King William's Land. Among other most important work, he completed the gaps in the narratives of Sir John Rae and Sir Leopold McClintock; and he discovered skeletons of several of Sir John Franklin's party. His great sledge-journey on this expedition, which lasted from April 1, 1879, to March 4, 1880, covered 3,250 miles. He left the army in 1884, after a visit to Alaska, where he followed the course of the Yukon River. Returning to Alaska in 1884, as the leader of the "*New York Times*' Expedition," he explored the Mount St. Elias district, and ascended that peak to a height of 7,000 feet. After visiting Chihuahua in 1889, for the proprietors of the journal *America*, he once more returned to Alaska in 1891, and opened up some 700 miles of previously unknown country.

Schwatka's best-known works are:—*Along Alaska's Great River* (1885) and *Nimrod in the North* (1885). Mr. W. H. Gilder has recorded his 1878–80 expedition under the title of *Schwatka's Search*; *Sledging for Franklin Records* (1882).

THOMAS JOHN MOORE, A.L.S., Corr. Mem. Z.S.

Born 1824. Died October 31, 1892.

BY the death of Mr. T. J. Moore on October 31 last, the Liverpool Museum loses its first Curator, and a familiar figure is removed from scientific circles in the seaport. The son of a Keeper in the Zoological Gardens, London, Mr. Moore early evinced a keen interest in Natural History pursuits; and at the age of 19 he was appointed Assistant-Curator of the Earl of Derby's Museum at Knowsley. On the death of its founder, the 13th Earl, in 1851, this collection was bequeathed to Liverpool, and Mr. Moore was then appointed Curator by the Corporation. During the last forty years he devoted unceasing energy to the perfection of the Museum, more

especially taking care to enlist the services of numerous friendly sea-captains in obtaining specimens; and the result of his labours is one of the foremost among provincial Museums, not merely of value to the student, but full of interest to the general sight-seeing public. Mr. Moore, moreover, not merely accumulated treasures, but made them available for the advancement of research, either by describing the novelties himself or by submitting them to those actively engaged in investigation—to Owen, Huxley, W. K. Parker, Darwin, and many others, who recognised him as a valued coadjutor. At the same time he interested himself in the spread of scientific teaching among the people, frequently lecturing; and during the years 1865 to 1884 he arranged and carried on the Liverpool Free Public Lectures. So long ago as 1860, Mr. Moore was elected a Corresponding Member of the Zoological Society of London; and quite lately he was made an Associate of the Linnean Society and an Honorary Member of the Literary and Philosophical Society of Liverpool. He was identified, more or less actively, with all the scientific bodies in the city of his adoption, and the large representative gathering at his funeral testified to the esteem in which he was held by those most intimately associated with him.

Mr. James Plant, F.G.S., died at Leicester on November 8, aged seventy-four. He was an active member of the British Association Boulder Committee, and did much useful geological work in the neighbourhood of Leicester.

M. Victor de Robillard, of Port Louis, Mauritius, well-known as an enthusiastic collector of Natural History specimens, died about three months ago. For a long time past he had sent to the British Museum rare and beautiful specimens of marine animals, one of the finest of which, *Cidaris curvatispinis*, was described by Professor Bell so recently as November 1 before the Zoological Society of London. The Botanical Department of the British Museum secured Robillard's collection of marine algæ a few years ago.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Of the 250 students enrolled this Session at University College, Aberystwith, nearly one-third are women.

Mr. E. E. Prince, M.A., Professor of Zoology in St. Mungo's College, Glasgow, has been appointed Commissioner and General Inspector of Fisheries in Canada.

Dr. R. von Wettstein has been appointed Professor of Botany in the German University of Prague, Bohemia.

Mr. W. F. Smeeth, B.A., of Dublin University and the Royal College of Science, London, has been appointed Demonstrator of Petrology and Mineralogy in the University of Sydney.

Mr. George W. Card, Assistant-Demonstrator in Geology at the Royal College of Science, London, has been appointed Curator and Mineralogist to the Geological Survey of New South Wales.

Dr. George A. Koenig has been appointed Professor of Chemistry in the Michigan Mining School, Houghton. Dr. Koenig is well known as one of the first mineralogists in the United States, and was formerly attached to the University of Pennsylvania.

Dr. F. H. Hatch has resigned his appointment on the Geological Survey of Great Britain, and has gone to seek his fortune in the auriferous districts of the Transvaal.

The University of Cape Town has been admitted to affiliation with the University of Cambridge. This is the second of the Colonial Universities thus affiliated, the other being that of New Zealand.

During the completion of the new buildings for the Durham College of Science, the Natural History Society of Newcastle-on-Tyne has lent one of the corridors in its Museum for the use of the practical classes in General Biology and Botany. The Botanical Department of the College has been re-constituted, the former lecturer, Mr. M. C. Potter, this Session attaining to the rank of Professor.

The annual conferring of degrees at the Royal University of Ireland took place on October 28. A conversazione was held in the evening at which a numerous company, including the Lord Lieutenant, attended. Besides musical and other attractions there were exhibits in Natural Science by Professors O'Reilly and Cole, who showed rock-specimens, and by Professor Haddon, who showed photographs of great anthropological interest, taken on a recent visit to the Aran Islands. The Royal University is only an examining body, but this annual gathering fosters a social feeling among its graduates. Something of the same kind might be of advantage to the University of London.

A VALUABLE collection of fossils, minerals, and shells, comprising several thousand specimens, and particularly rich in specimens from the Carboniferous formation, has just been presented to the University College of North Wales by Mr. Evan Roberts, of Manchester. It is hoped that this gift will become the nucleus of an important geological collection, suited to the educational requirements of a University College, and that similar gifts will, from time to time, be made to the College by those interested in the progress of geological study.

ACCORDING to the *Anatomischer Anzeiger*, the Biological Station at Plön (Holstein), under the direction of Dr. Otto Zaccharias, has accomplished successful work during the past summer, and has been visited by nearly one hundred zoologists and botanists. Of the species of animals occurring in the Lake of Plön, there have been determined 20 fishes, 40 crustacea, 69 worms, 14 mollusca, and 74 protozoa; while among these there are 10 undescribed species and several new genera. Of greatest interest is the discovery in fresh water of many organisms that have hitherto been supposed to occur only in the sea; and among these more especially may be noted certain genera of diatoms, rhizopods, and worms. An illustrated report on the results is in preparation.

THE tercentenary of the establishment of the Botanic Gardens of the University of Montpellier will be celebrated next year, probably during the week from 10 to 18 June. Invitations have been issued by Professor Flahault for that date (subject to alteration), since it is the best season for botanising in that district. The Garden was founded by Henri IV., in 1593, and has had a distinguished history. Those botanists who are fortunate enough to be invited are assured a hospitable and cordial welcome. Professor Flahault published in 1890 an account of the admirable Botanical Institute, accompanied by plans of its arrangement and a photogravure view (Firmin et Montane, Montpellier), which will interest those who intend to be present. Among the herbaria of Sir Hans Sloane in the British Museum is a very valuable series of plants of historical interest from the Montpellier Garden.

AT a special general meeting of the shareholders of the Bristol Museum and Library on November 3, it was resolved "that the Council be authorised to transfer the Museum and Library, and all the property of the Association, to the Mayor, Aldermen, and Burgesses of the City of Bristol, upon such terms as the said Council may agree upon, consistently with discharging the debts and liabilities of the Association, and maintaining the Museum and Library for the use of the citizens of Bristol." The Association in its present form is due to the amalgamation of two societies, the Bristol Library, founded in 1772, and the Bristol Institution for the Advancement of Science and Literature, opened in 1823. The proposal to erect a building for the joint occupation of the two societies, and to be called the Bristol Museum and Library, was resolved on in 1867, but the process of raising funds and providing the building was a long one, and it was not until 1871 that the removal to the new home took place. Unfortunately, the funds at the disposal of the committee were not sufficient to carry into effect completely the original design, and in a few years it was found necessary to extend and complete the existing building, and especially to add a lecture hall. These works were resolved on in 1874, and the additional rooms were opened in 1876. Altogether, a sum of £22,000 had been expended on the building, of which about £6,000 was obtained from the sale of the Park Street building, and some £10,000 by donations contributed by those anxious to see the scheme carried into effect, and subsequently represented by shares or left in the way of voluntary donations. Before the Association took possession of its new home, it seems to have been in a flourishing condition for several years. It derived an annual income of over £1,000 a year from the subscriptions, and this reached the maximum in 1879, when, under the new circumstances, the institution was just paying its way. Since 1879, however, the subscriptions have continually decreased in amount, and it is now considerably in debt. The Mayor of Bristol,

Sir Charles Wathen, has generously undertaken to pay outstanding liabilities, on condition that the city accepts and agrees to maintain the museum intact; and there is thus a good prospect of the unique collection at Bristol becoming once more, as it was in former years, a leading factor in the progress of Natural Science in the West of England. The Council has the services of one of the most accomplished of Museum Curators, Mr. Edward Wilson, and it will be a real gain to Science if he can be placed in a position to carry out adequately the work that he has hitherto had to attempt under circumstances of extreme difficulty.

THE *Brighton Herald* of November 5 contains the first of a series of articles entitled "Half-hours in the Brighton Museum." The Willett collection of Pottery claims first attention, and is treated in several readable instalments by an expert. This collection, one of the largest and most curious belonging to a private owner in England, includes many rare examples of the inscribed pottery in which a former generation of Englishmen found a vent for expressing their patriotic and domestic sentiments or for celebrating national rejoicings and victories.

WE are glad to note that the Museum at Perth, in Western Australia, is not only growing, but is being more appreciated by the inhabitants of this vast colony Special attention is paid by the Curator, Mr. Bernard H. Woodward, to the natural products of the country, and it is anticipated that large additions will be annually made, if a sufficient grant be voted by the Government. Moreover, as the Aborigines are beginning to make use of European manufactures for the construction of their implements, it becomes increasingly desirable to acquire primitive specimens from those districts where they will soon be unobtainable.

THE Academy of Sciences at Philadelphia has commenced the extension of its museum by the addition of a new wing. There will be four stories and a basement, and the area of the building will measure about 130 feet by 50 feet.

ACCORDING to the *American Naturalist*, materials for a Museum of Ethnology at Chicago are now being collected in South America.

THE removal of Professor H. F. Osborn to New York has infused new life into biological movements in that city. In addition to his work as Director of the Biological Institute of Columbia College, he has organised a department of Vertebrate Palæontology in the American Museum of Natural History; and on October 17 he presided over a new Biological Section of the New York Academy of Sciences, which he has been mainly instrumental in founding, and which will hold monthly meetings.

THE inaugural "Robert Boyle Lecture," delivered last May before the Oxford University Junior Scientific Club, has now been published. Sir Henry W. Acland has fittingly taken for the subject of this lecture the life of Boyle, whose many-sided studies he speaks of in detail The value of Boyle's work is thus summarised: "It is probable that the greatest service he did to his country and to mankind, was by kindling in the minds of his contemporaries an enthusiasm for science, a desire to explore and know Nature."

NATURAL Science is specially honoured this year by the Royal Society of London in the person of Professor Rudolf Virchow, who receives the Copley Medal for his researches in pathology, pathological anatomy, and prehistoric archæology. A Royal Medal is awarded to Mr. J. N. Langley in acknowledgment of his researches on secreting glands and on the nervous system. The Darwin Medal is appropriately offered to Sir Joseph Hooker, one of the earliest supporters of the author of "The Origin of Species." The Annual Meeting of the Royal Society is held on November 30, and the Biologists and Geologists nominated for the new

Council include Professor Bayley Balfour, Mr. F. Du Cane Godman, Professor J. G. McKendrick, Dr. W. T. Blanford and Mr. Adam Sedgwick. The officers remain as before.

THE Rev. Leonard Blomefield (formerly Jenyns) joined the Linnean Society on November 19, 1822, and at the meeting of the Society on November 17 an address of congratulation to him was moved and heartily passed on the occasion of the virtual completion of his seventy years' Fellowship of the Society—a period scarcely likely to be matched in the annals of any other learned Society. Mr. Blomefield was offered the appointment of naturalist to the "Beagle," and it was on his finding himself unable to proceed on the voyage that Mr. Darwin's opportunity came. He described, however, the fishes obtained on the voyage of the "Beagle" in a book of very great value to students of fishes. He enjoys excellent health, and we trust will long be spared as the oldest Fellow of the Society.

THE Norwich Science Gossip Club, which was founded in 1870 by Dr. J. E. Taylor, F.L.S., consists of 74 ordinary and eight honorary members, and so far as we know it fills a unique position among the learned societies. The object of this Club is the promotion among its members of a spirit of enquiry and the investigation of scientific and literary knowledge, by means of fortnightly papers, and occasional excursions. The evening meetings are held at the Royal Hotel, Norwich; tea coffee, and other beverages may be ordered before or after the reading of a paper, and the proceedings are conducted amid a fragrant cloud of tobacco-smoke. To this social feature the Club, no doubt, owes much of its long-continued success but the papers read, to judge from the last published Report of Proceedings, are of a varied and interesting nature, they are carefully prepared, and to them in particular is to be attributed the fact that "every evening has been pleasant and instructive." Mr. M. P. Squirrell, of Norwich, has been elected President for the Session 1892-93.

THE Royal Scottish Geographical Society has organised in connection with its Education Scheme, two courses of lectures to be delivered this session in Edinburgh. Before Christmas Mr. J. G. Goodchild, of the Geological Survey, treats of Volcanoes in six lectures; and after Christmas Mr. J. Arthur Thomson will undertake a course on the Geographical Distribution of Plants and Animals.

THE Naturforschende Gesellschaft of Danzig celebrates its 150th anniversary on January 2 and 3, 1893. Societies desiring to send representatives should communicate with Professor Dr. Conwentz, the Secretary. The naturalists of Danzig have displayed much activity of late years, especially in geological researches, and many valuable memoirs hail from the remote German city.

THE principal paper in the current number of the *Proceedings of the Cotteswold Field Club* deals with "Some Laws of Inheritance, and their Application to Man." The author, Mr. S. S. Buckman, is one of that numerous body of naturalists who refuse to believe with Weismann in the non-heredity of "acquired characters"; and in applying his views (originally derived from a study of Ammonites) to man, furnishes, incidentally, a considerable amount of very good reading. He extends Dr. Louis Robinson's investigations upon the manners and customs of the newly born child, and finds relics of monkeyhood everywhere; it is even boldly suggested that the lines, "Lull-a-by baby on the tree top," drew their inspiration from oral tradition of a former arboreal existence. A baby, too, we are told, "seldom refuses a bone, even after the very fullest meal"; this, perhaps, is not especially ape-like, but, on the whole, the application of the term "young monkey" to children, is one of those truths often said in jest.

THE Session of the tenth Congress of the American Ornithologists' Union was opened at Washington on November 15.

OBSERVATIONS AND CORRESPONDENCE.

The Geological Society of London and Equity.

While I am much obliged for the notice taken in the current No. of Natural Science (p. 654) of my last paper at the Geological Society, I must ask to be allowed to say a few words from my point of view. All that is quoted from Professor Judd's Address of 1888 most reasonable people would be prepared to endorse; but it is altogether wide of the point. If the Council of the Society applied such a principle fairly and *consistently*, no one would have any just cause of complaint It is, however, because they have, while suppressing a mass of new evidential facts during the last two years on one side of the controversy, given space, to the extent of eleven pages of the Society's *Journal*, to "A paper [which] is a controversial one, and deals with a number of minor points connected with the stratigraphy of the Bagshot Area," and deals, moreover, in part with a number of minor points belonging to that part of the area which I had specially reserved for further investigation, while it ignores many facts now well-established, and goes out of its way to criticise some things in my earlier papers which have been superseded in my later papers, that I feel they have treated me unfairly.

I make allowance for the human nature to be found in any average body of some two dozen fairly educated men, which accounts, perhaps, for their unwillingness to publish my last paper, and I make full allowance for that intellectual colour-blindness in a referee which may result from what has been called "unconscious cerebration"; and so my quarrel resolves itself into a raid upon the system rather than upon individuals. A great deal may be said—as I know very well—for the referee system, but the responsibility for their own action must abide with the Council; and if the affairs of the Society had not got rather into a groove detrimental in every way to true science, it would not be necessary for me to suggest that the occasional appointment at least of a "standing committee" would be a useful innovation upon traditional procedure. A. Irving.

Wellington College, Berks.

The Parasites of Cut Corks.

In answer to the query proposed under the above title, I would refer Mr. J. Lawrence Hamilton to a short note on "Insect Damage to the Corks of Wine Bottles" (in *Insect Life*, vol. i., no. 3, p. 91, 1888), which may be of some use to him. It is the summary of a paper by M. Preudhomme de Borre, read at the Entomological Society of Belgium on May 7, 1887. Two species of insects, *Oenophila v. flavum* and *Rhizophagus bipustulatus* are described as eating holes through the corks. The eggs are often deposited in the cork while on the tree, so that it is likely every form of cork may be infested. Some other insects are mentioned as being similarly troublesome.

Paris. Henry de Varigny.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

ERRATA.

P. 555, line 18.—*For* Firth of Forth *read* Firth of Clyde.

P. 688, lines 16, 17.—*For* "they get their" *read* "the ash gets its."

P. 703, line 23.—The price of Ward's "Horn Measurements" is £1 1s. (not £1 10s., as stated).

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